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THE GIFT OF MOVING:
INTERGENERATIONAL CONSEQUENCES OF A MOBILITY SHOCK

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ABSTRACT

We exploit a volcanic “experiment" to study the costs and benefits of geographic mobility. We show that moving costs (broadly defined) are very large and labor therefore does not flow to locations where it earns the highest returns. In our experiment, a third of the houses in a town were covered by lava. People living in these houses were much more likely to move away permanently. For those younger than 25 years old who were induced to move, the “lava shock” dramatically raised lifetime earnings and education. Yet, the benefits of moving were very unequally distributed within the family: Those older than 25 (the parents) were made slightly worse off by the shock. The large gains from moving for the young are surprising in light of the fact that the town affected by our volcanic experiment was (and is) a relatively high income town. We interpret our findings as evidence of the importance of comparative advantage: the gains to moving may be very large for those badly matched to the location they happened to be born in, even if differences in average income are small.

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1 Introduction

Wages differ enormously across space. One interpretation of such differentials is the presence of large moving costs, arising from informational, cultural, legal, and economic barriers that impede labor from flowing to its highest return activity (Munshi and Rosenzweig, 2016; Bryan and Morten, 2018). However, just because the inhabitants of some locations have higher incomes than others does not mean there is a large causal effect of moving to these locations. The variation in average income across locations may be due to selection effects, whereby high productivity workers sort into certain locations, as opposed to the location having a direct causal effect on earnings (e.g., Lagakos and Waugh, 2013; Young, 2013).

Distinguishing between selection and direct causal effects of locations is challenging. Large, exogenous relocation shocks are few and far between. Consequently, most work on this topic has used structural methods. However, a small number of recent papers have made use of experimental and quasi-experimental variation to identify the consequences of moving. Bryan, Chowdhury, and Mobarak (2014) find that randomly giving workers an inducement to move, in the form of a $8.50 bus ticket, yields large effects on subsequent economic outcomes. Chetty, Hendren, and Katz (2016) show that giving families vouchers to move from high-poverty areas to lower-poverty areas improves long-term outcomes for young children. Sarvimäki, Uusitalo, and Jäntti (2016) study the long-term impact of forced migration in Finland after World War II. They estimate large positive long-run effects of displacement on earnings of men working in agriculture prior to displacement.

These results suggest that some people are "stuck" in locations that do not fully exploit their economic potential. However, many questions remain unresolved. Do the benefits of moving apply only to situations where people are leaving behind a desperately poor location for better economic opportunities? How do the benefits of moving vary with age? Would the benefits of moving accrue to all workers, or does comparative advantage play an important role as suggested by Bazzi et al. (2016) and Lagakos, Mobarak, and Waugh (2017)?

We shed new light on the role of location in shaping economic outcomes by studying the consequences of a true "natural" experiment. On January 23, 1973, a long-dormant volcano erupted unexpectedly on the Westman Islands, off the coast of Iceland. A volcanic fissure opened only 300 yards from the edge of the island's town forcing the entire population of the island to be evacuated in a matter of hours. The eruption continued for several months and about a third of the houses on the island were destroyed by lava. The owners of these lava-stricken homes were "cashed out" of their property by a government disaster relief fund. After the eruption ended, a majority of the residents of the island returned and the population of the island quickly rebounded to almost its pre-eruption level. However, those whose homes were destroyed were substantially less likely to return.

We interpret this "lava shock" as a large, quasi-random shock to mobility. We can estimate the causal effect of moving by comparing outcomes for those whose houses were destroyed by lava (our "treatment group") versus those whose houses were intact after the eruption (our "control group"). To do this, we gather information on exactly which houses were destroyed and which not. We then merge this information with data on the inhabitants of each house, their tax records over a 34 year period, data on their educational attainment, and genealogical data allowing us to analyze their descendants. We are therefore able to study the economic consequences of the mobility shock over the full lifetimes of the individuals affected and their children. This turns out to be important for our results.

We document a remarkable reversal of fortune for those less than 25 years old at the time of the eruption. Being "unlucky" enough to have one's house destroyed is associated with a large *increase* in long-run labor earnings and education. Using the destruction of houses as an instrument for moving away from the Westman Islands, we estimate a causal effect of moving of $27,000 per year, or close to a doubling of the control group's average earnings. The income effect is particularly large at the upper tail of the income distribution: the effect on the 95th percentile of the earning distribution is $47,000 per year. There is also a large causal effect on education: those younger than 25 that were induced to move because their house was destroyed by lava got almost 4 years of additional schooling (and their children's education responded even more).

Our findings imply that moving costs (broadly defined) must be large. If not, out-migration in the control group would have been larger. We cannot tell whether the eruption made the treatment group better off or the control group worse off. Both groups are likely affected by such a large disruption. Our experiment identifies the difference in earning and education outcomes for these two groups. We calculate that for an 18 year old who is induced to move, the difference in the net present value of life-time earnings is roughly $440,000. This difference can be viewed as an estimate of the cost of moving (broadly defined). This large barrier to moving actually lines up quite well with existing structural estimates. Kennan and Walker (2011) estimate a structural model of migration decisions for young men within the United States, and find that the typical worker could roughly double his or her income by moving.

The benefits of moving are, however, very unequally distributed within the family. While losing the family home in the eruption had large *positive* effects on the adulthood earnings of people younger than 25 years old at the time of the eruption (mostly children), the earnings effects for older cohorts are somewhat negative (but statistically insignificant). In other words, the economic costs of moving fall disproportionately on the parents in a family, while the economic gains accrue to the children. This implies that moving can be an immensely valuable but also somewhat costly gift that parents can give to their children. Conversely, the large intergenerational differences in returns to moving may help explain the large barrier to moving we estimate for younger cohorts. As we show below, the large barrier may partly reflect limits to parents' understanding of the potential gains to their children of moving, limited parental altruism, or aversion to the uncertainty associated with moving.

The large positive causal effects we estimate for those younger than 25 at the time of the eruption are particularly surprising in light of the fact that the Westman Islands was (and is) one of the highest income towns in Iceland. Those induced to move for the most part moved to places with lower average income (e.g., the capital area). Previous studies have tended to find gains for households moving from very disadvantaged places to places with substantially higher average income. We are, however, studying a situation where households appear to be "moving *away* from opportunity" from the perspective of average income. How can it be that the effects are so positive in this case?

The most compelling interpretation for these facts, in our view, is that they reflect the importance of comparative advantage. Roy's classic 1951 paper studies the matching between workers and tasks for the case of fishermen and rabbit hunters (Roy, 1951). Naturally, those with greater relative prowess in fishing will sort into that industry, and the same will occur for rabbit hunting. While those who moved away from the Westman Islands did not become rabbit hunters (more likely, they became bankers), they did leave an economy that was highly concentrated in fishing. Many smaller communities are, like the Westman Islands, specialized in a particular industry that is unlikely to be suitable for everyone. In such a setting, potential gains from moving may be large since workers are "stuck" in locations in which the occupational mix is not well suited for their talents. While the Westman Islands—with its high-paying fishing jobs—may be an ideal place for some workers, it is unlikely to be the best match for a future computer whiz or a great legal mind.

We present a Roy model with heterogeneous comparative advantage and moving costs (building on recent work by Lagakos and Waugh (2013), Young (2013), Bryan and Morten (2018), and Adao (2015)) to study these effects. A key insight from our model is that the "compliers" in our natural experiment—i.e., those induced to move by the volcanic eruption—gain a particularly large amount from moving. Intuitively, it is those that are not well suited to live on the island that are induced to move.[1] The model makes clear that other groups may gain much less or even lose from moving since they are better suited to live on the island (and therefore not induced to move by the eruption).

This insight provides a natural interpretation for how the benefits of moving we estimate can be so large, despite the fact that the individuals are moving away from a high income location. Those induced to move are selected on their comparative advantage, which implies that they have particularly large gains from moving. The Westman Islands have high average income, however, because they are a particularly good place for many *other* workers to earn income. One piece of evidence for this comparative advantage interpretation comes from our analysis of the pre-treatment characteristics of the compliers.[2] What stands out from this analysis is that the compliers in our experiment are more likely to come from highly educated families, whose children are likely to have the most to gain from moving to a location where the returns to education are larger.

Our model features an overlapping generations structure and an education choice. This allows the model to capture the large difference in causal effects we estimate between younger and older individuals in our sample: The young can reoptimize their education and career choice when they move, while this is more difficult for older individuals.

Our focus on comparative advantage contrasts with the simple wage model of Abowd, Kramarz, and Margolis (1999) (hereafter, AKM), which allows only for absolute advantage. AKM decomposes wages as a sum of worker and firm (or location) effects, where the latter are empirically identified off of movers. Viewed through the lens of the AKM model, our data would imply that the Westman Islands is a "bad" place to live—it has a negative location effect—since there is a large positive causal effect of moving away. But to fit the high average incomes in the Westman Islands, the people living there would have had to have large positive worker effects to more than cancel out the negative location effect. While logically consistent, we do not view this as the most compelling explanation for the facts, given the low levels of standard human capital measures in the Westman Islands. We discuss this, as well as other competing possible explanations for our results in section 9.

Might compensating differentials explain the large effects of moving we estimate? While any

[1]This result echoes and extends earlier results by Borjas et al. (1992).

[2]While it is not possible to identify exactly who the compliers are, it is possible to compare their characteristics versus the average person in the population, using the methods described in (Angrist, 2004).

pattern of results can be explained by a sufficiently flexible model of (unobserved) compensating differentials, this does not seem like a likely explanation in our case. Conventional wisdom in Iceland is that the price level in rural towns like the Westman Islands is and has always been higher than in Reykjavik (except possibly when it comes to housing) and product variety much more limited. Any compensating differential of living in the Westman Islands are, therefore, unlikely to arise from prices, but might arise from differences in preferences (Atkin, 2013). However, this interpretation seems difficult to square with the time pattern of earnings effects which appear to grow across generations. If compensating differentials associated with preferences for living in the Westman Islands were behind our effects, one would expect them to be smaller for children than parents, and even smaller for descendants born outside of the Westman Islands. But the earnings gains from moving are the reverse: highest for the young and their descendants, and much smaller for the parents. We also estimate causal effects of moving on a number of non-monetary outcomes and find that movers are less likely to die before the age of 50, less likely to receive pension payments before the retirement age of 65 due to illness or disability, and more likely to marry. None of these support the compensating differentials interpretation.

Our findings corroborate recent work arguing that location plays a key role in determining income. Several recent papers on this topic are worth highlighting in addition to the papers already mentioned. Yagan (2018) shows that, even controlling for a detailed set of characteristics, workers living in an area hit worse by the Great Recession had lower employment many years later. Chyn (2018) finds that children from households forced to relocate due to demolition of public housing in Chicago have higher earnings and employment rates as adults compared to children from nearby public housing that was not demolished. Deryugina, Kawano, and Levitt (2018) and Sacerdote (2012) show that those displaced by Hurricane Katrina had higher long-run income and educational outcomes.

The paper proceeds as follows. Section 2 provides a short description of the volcanic eruption and its aftermath. Section 3 describes our data. In Section 4 outlines our empirical strategy. Section 5 presents results on the effects of the shock on mobility. Section 6 presents pre-treatment balance test. Section 7 presents our results on the effects on earnings, while section 8 presents our results on the effects on education. Section 9 discusses our interpretation that the results imply that moving costs are large and comparative advantage important. Section 10 concludes.

2 A Volcanic Experiment

Just before 2:00am on January 23 1973 a volcanic eruption began on the tiny island of Heimaey off the southern coast of Iceland. Heimaey is the main island in a cluster of islands called the Westman Islands. Despite their small size, the Westman Islands are of great economic importance to Iceland because they are the only location where a fishing harbor can be built over a several hundred mile stretch on the southern coast of Iceland. As a consequence, a prosperous town of 5,200 inhabitants was situated there.

The eruption began on a 1500m long fissure only about 200-300 meters from the easternmost part of the town (Thorarinsson, 1973). All inhabitants were immediately evacuated from the island. Luckily, the island's entire fishing fleet was in harbor that night due to bad weather the preceding day, which was crucial in the evacuation. Within 4 hours, the evacuation was complete. Only one person died due to the eruption that night. Over the following days and weeks, rescue units did their best to recover valuables—everything from livestock, to household appliances, to photo albums.

The eruption lasted for roughly 5 months. During this time it produced enormous amounts of lava and ash, which destroyed the eastern third of the town. Figure 1 shows a map of the town after the eruption, with the area covered by lava from the eruption shaded in red. Of the roughly 1400 houses and apartments in the town at the start of the eruption, roughly 30% were destroyed. These houses are colored pink (darker) in the figure, while the residential units that survived are colored green (lighter). Most of the destroyed houses were engulfed by lava, but some were hit by "lava bombs" (*pyroclasts*) which were projected from the volcano or collapsed under the weight of ash.

People began moving back to the Westman Islands in the summer and fall of 1973. Figure 2 shows that by the end of 1975, the population of the Westman Islands had returned to roughly 85% of its pre-eruption level. The lava field created by the eruption actually improved the town's harbor.[3] This meant that the economic fundamentals of the Westman Islands were, if anything, improved by the eruption. Figure 3 shows that the fishing industry barely skipped a beat, and by 1974, fishing companies in the Westman Islands were back to normal production levels.

While many people quickly moved back to the Westman Islands after the eruption ended, those whose houses had been destroyed by the eruption were substantially less likely to return.

[3] For a time during the eruption, the lava flow threatened to block the harbor. This would have been devastating for the economic prospects of the islands. A Herculean effort to divert the flow of the lava by spraying water on it and cooling it was successful at averting this calamity.

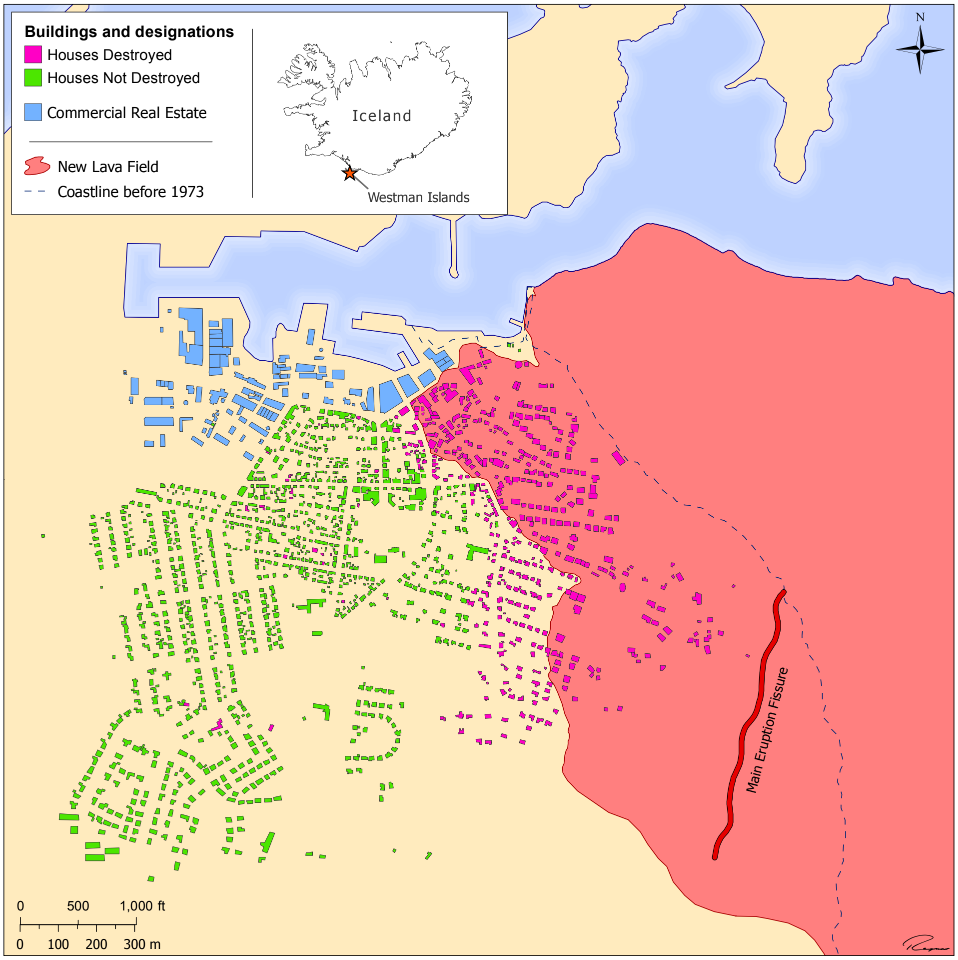


Figure 1: Map of Westman Islands town Post 1973 Eruption

Note: The map was created by Ragnar Heidar Thrastarson based on data from the Icelandic Disaster Relief Fund (Viðlagasjóður Íslands) and the National Land Survey of Iceland (Landmælingar Íslands).

Table 1 reports statistics on this. The people who had lived in the houses that were destroyed were 15 percentage points—or roughly 50% less likely—to return before the end of 1975. We refer to those that did not return before the end of 1975 as "movers." The proportion of movers was 42% among those with destroyed houses, while it was only 27% among those whose houses were not destroyed.

The Icelandic government set up a Disaster Relief Fund (Viðlagasjóður Íslands) to compensate those that lost their houses in the eruption. The Disaster Relief Fund "cashed-out" those whose houses and land was destroyed at the current replacement value of their house and land.[4] The cash value of houses and land was determined according to annual fire insurance and tax valuations,

[4]It was not possible to build again on the land covered by lava—at least for several decades. This land was therefore effectively "destroyed".

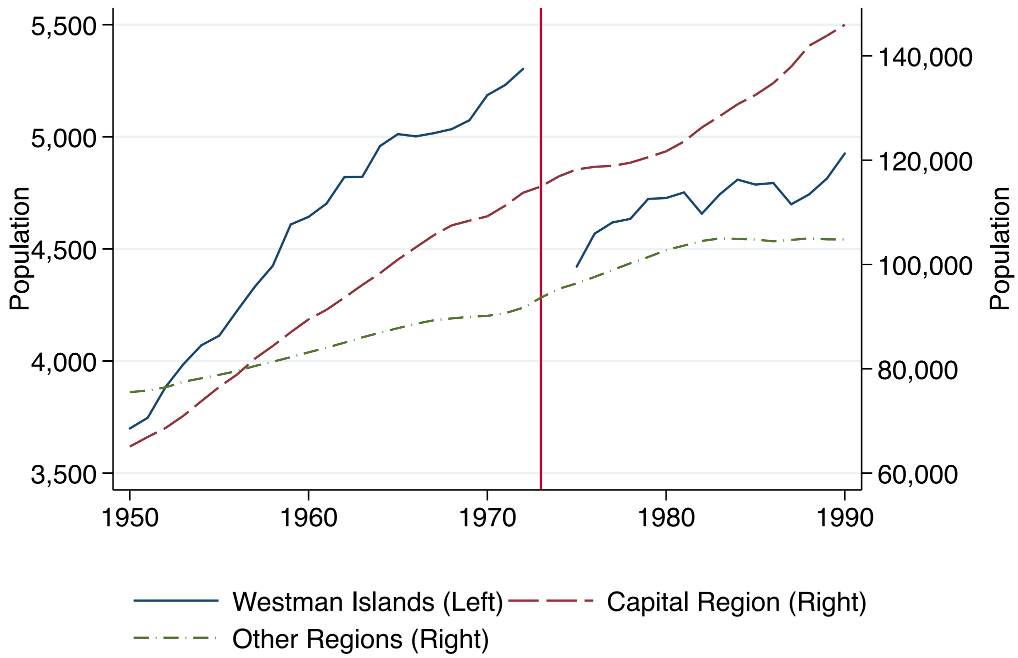


Figure 2: Population by Year

Note: The figure plots the evolution of the population of the Westman Islands (left axis), the Iceland's capital region (right axis), and other regions of Iceland (right axis). These data were obtained from Statistics Iceland.

Figure 3: Fish Catch by Year

Note: Total fish catch in thousands of tones per year by area. Westman Islands accounts for 60-85% of all fish landed in harbors in South Iceland. These data were obtained from Fiskifélag Íslands and various issues of *Útvegur*.

Table 1: Probability of Moving

	P(Move)	Sample
Overall	0.311	4,807
House Destroyed	0.420	1,341
House Not Destroyed	0.269	3,466

Note: The table reports the probability of moving away from the Westman Islands (i.e., not returning before the end of 1975) for three groups: those whose house was destroyed in the eruption, those whose house was not destroyed in the eruption, and the total population. We also report the sample size of each group.

respectively.[5] Households were then compensated for the value of the destroyed houses and land, net of any associated mortgages. The compensation was paid out in four equal payments over the period October 1973 to July 1974. The replacement values were increased to reflect October 1973 prices. (Inflation in Iceland was 33% in 1973 and 51% in 1974.) The Disaster Relief Fund took ownership of the destroyed real estate (and any associated mortgages) as soon as the first payment was made. The Icelandic Disaster Relief also paid the cost of infrastructure repair and rescue operations.

It is worth emphasizing that the Icelandic government took steps to try to ensure the accuracy of these compensation payments. The government employed a private company to assess the damages to all houses on the island, and augment the baseline fire insurance assessments to account for any additional features that were not included in the original assessments. While it is inevitable that these valuations contain some error, we believe that they are likely modest in relation to overall household wealth.

How might errors in these valuations affect our analysis? Our main results are a large positive effect on lifetime earnings for those younger than 25 at the time of the eruption—to a large extent arising from earnings differences occurring more than a decade after the eruption—and a small negative effect on the older generation. The most natural way in which errors in payouts may affect these results is through wealth effects. But it is hard to see how such a modest wealth shock could explain the large effects on earning we identify many years later, and the pattern of effects we observe on children versus their parents.

[5]The fire insurance valuation of houses are meant to estimate the cost of rebuilding the house. These are based on characteristics of the house (size, age, etc.) and are indexed to the construction cost index in Iceland.

3 Data

To analyze the long-term consequences of our "volcanic experiment" we leverage the exceptionally detailed data on income, education, and genealogical linkages that are available for the Icelandic population. Our first task is to identify who lived in the Westman Islands at the time of the eruption. To do this, we obtained from the Icelandic National Registry scanned images of inhabitant registers of the Westman Islands on December 1 1972, less than two months before the eruption.[6] We converted these images to machine-readable form. These data contain the full name, unique personal identifier, address, date of birth, place of birth, gender, marital status, and citizenship status of all residents of the Westman Islands.

Next we need to identify who moved away from the Westman Islands following the eruption. For this, we obtained analogous data to those described above on the population of the Westman Islands on December 1 1975. We choose 1975 as opposed to 1974 because of possible inaccuracies in the 1974 data arising from people who had not yet updated their permanent addresses after the eruption. We have also redone our entire analysis using the location of residence in 1981 as opposed to 1975. The results are very similar.

We identify which houses were destroyed by the eruption using scanned images of records from the Icelandic Disaster Relief Fund obtained at the Icelandic National Archives, which we converted to machine readable form. We have also collected data on all residential real estate in the Westman Islands from the 1970 Property Registry of Iceland. These data provide us with information on the year of construction and tax valuation of the houses, which we use to carry out balance tests between the destroyed and non-destroyed houses.

We are interested in analyzing the effects of the eruption on the descendants of the original inhabitants of the Westman Islands at the time of the eruption. To this end, we obtained data on all the descendants of the original inhabitants from deCODE Genetics. Specifically, we obtained a list of these descendants along with the name and unique personal identifier of each person's mother and father. This allows us to assign these descendants to either the treatment or control group.

We have linked these data to administrative data on earnings and educational attainment. Our earnings data are from the Icelandic Longitudinal Income Database (ICELID). This database was constructed by Statistics Iceland from tax records over 34 years, spanning 1981-2014, and includes both earnings and demographic characteristics. We were able to match 95% of the inhabitants to

[6] At this time, the Icelandic National Registry was updated once a year on December 1.

the earnings data.[7]

Our data on educational attainment are from Statistics Iceland's Education Registry, which contains information on educational attainment for the Icelandic population in 2011. The highest level of completed education is reported on a five-step scale using the International Standard Classification of Education (ISCED). We map this variable into a measure of years of schooling. Appendix A describes this mapping.

4 Empirical Strategy

Our goal is to estimate the causal effect of moving away from the Westman Islands on key long-term economic outcomes such as income and education. The relation of interest is captured by the following equation

$$Y_{it} = \alpha + \beta Moved_i + \mathbf{X}_i'\gamma + \delta_t + \varepsilon_{it}, \tag{1}$$

where Y_{it} denotes earnings or education for individual i in year t. The variable $Moved_i$ is an indicator for having moved from the Westman Islands as of 1975. The causal affect of moving is denoted by β. $\mathbf{X}_i$ is a vector of demographic characteristics, including a set of age fixed effects, with coefficient γ, and δ_t is a set of year fixed effects. Finally, ε_{it} is an error term that captures other determinants of income and education.

If people were to move at random, estimating equation (1) by ordinary least-squares (OLS) would deliver the average causal effect of moving. Yet, the decision to move is clearly far from random. The central empirical challenge faced by the literature on the effects of migration is how to deal with these selection effects. For example, if low skilled workers with unstable jobs are more likely to move than the rest of the population, then movers may have a lower long-term income than stayers even if there is *no* causal effect of moving.

To overcome this challenge, we employ an instrumental variables (IV) strategy that exploits the quasi-random destruction of houses by the volcanic eruption. More specifically, we instrument for the variable $Moved_i$ using an indicator variable for whether the person lived in a house that was destroyed in the volcanic eruption. The "first-stage" regression in our IV strategy is then given by

$$Moved_i = \alpha_f + \phi Destroyed_i + \mathbf{X}_i'\gamma_f + \eta_{it} \tag{2}$$

where $Destroyed_i$ is an indicator for individual i having lived in a house that was destroyed by

[7]Unmatched individuals either died before 1981 or live abroad and therefore do not file taxes in Iceland. The age distribution of those we cannot match suggests that most of the people we cannot match likely died before 1981.

the eruption. The coefficient ϕ on the instrumental variable captures the effect of living in a house that was destroyed on the probability of moving.

This empirical strategy identifies the causal effect on the "compliers" in our experiment—i.e., those that are induced to move by having their house destroyed (Imbens and Angrist, 1994). As we discuss in section 9, we believe that the causal effect on compliers is likely larger than the causal effect for the population as a whole since the compliers in our experiment are a subgroup of the population that is less well matched to living in the Westman Islands than the average person living there.[8]

A recent literature emphasizes the potential heterogeneity of treatment effects across different cohorts of individuals (e.g., Chetty and Hendren, 2018). We will in most cases present results separately for those less than 25 years old at the time of the eruption, and those who were 25 years old and older. Our chosen age break-point of 25 is meant to distinguish between people that had settled on a career at the time of the eruption and those that had not yet settled on a career. We also explore a specification that allows for linear exposure effects during childhood. In appendix D, we present results for an alternative—non-age based—way of grouping people. There we group people into "household heads" versus "dependents." The idea is to distinguish between those that make the decision to move (household heads, e.g., the parents in a family with children) and those that don't (dependents). This yields similar results to our baseline grouping.

The definitions we give above for the variables $Moved_i$ and $Destroyed_i$ pertain to the "original inhabitants"—i.e., those that lived in the Westman Islands at the time of the eruption. We also consider the effect of the lava shock on their descendants. In particular, we consider children (but not grandchildren) of the original inhabitants that were born after the eruption (1973) but before 1997. Restricting the sample to those born before 1997 guarantees that everyone in the descendant sample is at least 18 years old by the end of our sample. This ensures that we are able observe them in our administrative data. The reason for restricting the sample to children (but not grandchildren) born after the eruption is to avoid including descendants who had already moved away before the eruption.

[8] As with all IV identification strategies, our empirical strategy requires a monotonicity assumption to be valid. In our context, this assumption rules out the existence of individuals that would have moved away after the eruption if their house had not been destroyed but were induced to stay (move back) by the fact that their house was destroyed. Recall that in our setting all Westman Islanders were forced to relocate away from the Islands for at least six months and we define the "Moved" variable in terms of where people live two years after the eruption (18 months after the eruption ended). While it is possible that the monotonicity assumption is violated in our setting, we think it is unlikely. A reaction of defiance is likely to be strongest among those with the strongest attachment to the Westman Islands. But these are "never-takers" in our experiment.

Table 2: Descendant Groups

	Parent's Status ({father, mother})	Size
Treatment	{D, D}, {D, A}, {A, D}	965
Control	{N, N}, {N, A}, {A, N}	2,775
Excluded	{D, N}, {N, D}	282
Total		4,022

Notes: *D* denotes a parent that was living in a house destroyed by the eruption, *N* denote a parent that was living in the Westman Islands but in a house that was not destroyed, and *A* denotes a parent that did not live in the Westman Islands at time of the eruption.

For the descendants, the definitions of $Moved_i$ and $Destroyed_i$ are somewhat more subtle, since it was not the individuals themselves that moved due to the eruption or lived in houses that were destroyed, rather it was their parents that were directly affected by the eruption. For the descendants, $Moved_i$ is, therefore, an indicator for whether the *descendant* lived outside the Westman Islands when first observed in the administrative records. For $Destroyed_i$, there is the additional issue that each descendant has two parents, who may each have come from a destroyed (D) or non-destroyed (N) house in the Westman Islands, or may have come from another location in Iceland (A). Table 2 illustrates our assignment of different descendants into the treatment and control groups. The treatment group is those descendants whose parents' status is one of the following {D,D}, {D,A}, or {A,D}, where the first entry is the father and the second entry is the mother. The control group is those whose parents' status is one of {N,N}, {N,A}, or {A,N}. We choose to exclude those that have one parent from a destroyed house and one parent from a non-destroyed house, i.e., the {D,N}, {N,D} groups. We could alternatively have added these groups to both the treatment and control groups. This would not have affected our point estimates (since their presence in both groups would mean that they would cancel out) but would have complicated the calculation of standard errors.

5 Propensity to Move

The first thing that we need to establish is that the "lava shock" does, indeed, have a strong and statistically significant effect on the propensity of people living in the Westman Islands at the time of the eruption to move away. Table 3 reports estimates of the first-stage regression where $Moved_i$ is regressed on $Destroyed_i$ as well as controls—equation (2). We report results for all inhabitants as well as separate results for those younger than 25 years old at the time of the eruption and those 25 years old and older. In all cases, the first-stage coefficients are statistically significant at the 1%

Table 3: First Stage Regressions

	All		Younger than 25		25 and older		Descendants	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Destroyed$	0.151***	0.160***	0.114***	0.125***	0.194***	0.200***	0.058***	0.059***
	(0.030)	(0.029)	(0.035)	(0.034)	(0.031)	(0.030)	(0.017)	(0.017)
Control Mean	0.269	0.269	0.284	0.284	0.250	0.250	0.621	0.621
Controls	No	Yes	No	Yes	No	Yes	No	Yes
F-statistic	17.9	21.1	10.9	13.6	25.8	27.7	10.4	12.3
N	4,807	4,807	2,609	2,609	2,198	2,198	3,740	3,740

Notes: This table reports coefficients from OLS regressions of $Moved$ on $Destroyed$. For the original inhabitants $Moved$ is an indicator for having moved away as of 1975 and $Destroyed$ is an indicator for living in a house that was destroyed by the eruption. For descendants, $Moved$ is an indicator for living outside the Westman Islands when first observed in the administrative records, while the definition of $Destroyed$ is more involved and is described in section 4. The set of controls includes gender, age, a dummy for having changed houses after 1960, and a dummy for being born in the Westman Islands. Robust standard errors clustered by address are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1

level. Living in a house that was destroyed raises the probability of moving by 15% points for the overall population. There is some heterogeneity across the age groups. The effect is about 12% for those younger than 25, while it is roughly 20% for those 25 and older. The first-stage F-statistic ranges from 28 to 70.

Table 3 also reports first stage estimates for the descendants. The estimates show that individuals that have parents that lived in houses destroyed by the eruption are about 6 percentage points less likely to live in the Westman Islands when they first appear in our administrative records. This difference is statistically significant at the 1% level with a first-stage F-statistic of 12.3.

6 Balance Tests

The Westman Islands is a small and relatively homogeneous community. Our discussions with locals who lived in the Westman Islands at the time indicate that the neighborhoods destroyed by the volcanic eruption were essentially similar those that were not destroyed. While we cannot fully test this assumption, a basic requirement is that observable pre-eruption features of the people and the houses in the destroyed and non-destroyed areas should be similar.

Table 4 presents balance tests for various pre-eruption characteristics that are available in our data. While we have limited data on pre-treatment economic characteristics, importantly, we do have data on housing values prior to the eruption (from tax valuations). There are no systematic differences in values of houses between the destroyed and non-destroyed neighborhoods. As housing wealth is likely to be correlated both with total wealth and income, this test confirms the perceptions of the locals we have talked with that the destroyed neighborhoods were neither richer nor poorer than neighborhoods that were not destroyed.

We also have information on the year of construction of houses in the Westman Islands. These data show that the destroyed houses were slightly older, but only by roughly two years on average. The average age of houses in the Westman Islands was roughly 30 years. So, the two year difference is quite minimal. But it does suggest that the destroyed area was a slightly older part of town on average.

We have information on several pre-treatment demographic characteristics. Among those 25 years old and older at the time of the eruption, about half of the population was female, the average age was 46 years, 76% were married, 47% were born in the Westman Islands, they had on average 12 years of education, and had slightly less than 2 children on average. When we test for differences in these characteristics (as well as the rate of divorce and widowhood and the probability of moving houses after 1960), we find that in all cases the differences are small and statistically insignificant. The last row of Table 4 also shows that there is no difference between the treatment and control samples in terms of the number of individuals we were unable to match to their long-term outcomes on earnings.

We also perform these same balance tests for those younger than 25 years old. In this case, there is a statistically significant difference between the treatment and control sample for one of the 10 characteristics—the probability of being born in the Westman Islands. The treatment group is somewhat more likely to have been born in the Westman Islands (83% versus 78% for the control group). To assess whether these results indicate a true difference in the nature of the destroyed neighborhoods or random variation (one out of 20 tests being significant), we carried out two additional tests. We performed a test of the omnibus null hypothesis that *all* the balance test coefficients are zero and are not able to reject that hypothesis. We also used a Bonferroni adjustment to assess whether *any* of the coefficients are non-zero taking account of multiple hypothesis testing. We are not able to reject zero for any coefficients with this adjustment.

We should also note that, to the extent that the destroyed neighborhoods *were* different from the non-destroyed neighborhoods in ways that were correlated with long-term outcomes, one would expect these selection effects to run primarily through the *adults* who lived in the affected neighborhoods and only secondarily through their children. Yet our results illustrate a large, positive effect of the lava shock on outcomes for those less than 25 years of age, and a small,

Table 4: Sample Characteristics and Covariate Balance Test

	Younger than 25		25 and older	
	Control Mean	Treatment vs. Control	Control Mean	Treatment vs. Control
	(1)	(2)	(3)	(4)
Value of house (2014 $)	65,576	-306	61,321	-111
		(2,146)		(2,419)
House construction year	1943.2	-1.76*	1941.2	-2.45**
		(0.96)		(0.97)
Female (%)	0.48	0.023	0.48	0.002
		(0.022)		(0.022)
Age	11.8	0.22	46.1	0.81
		(0.29)		(0.72)
Married (%)	0.08	-0.006	0.76	0.010
		(0.011)		(0.019)
Number of children	0.14	-0.030	1.86	-0.018
		(0.018)		(0.077)
Widowed (%)	0.000	0.000	0.08	-0.010
		(0.000)		(0.011)
Divorced (%)	0.001	-0.001	0.03	-0.010
		(0.001)		(0.007)
Years of schooling	–	–	11.95	0.167
		–		(0.165)
Move house after 1960 (%)	0.61	-0.022	0.46	0.013
		(0.021)		(0.022)
Born in the Westman Islands (%)	0.78	0.051***	0.47	0.036
		(0.017)		(0.022)
Not matched to outcomes (%)	0.02	-0.007	0.12	0.016
		(0.005)		(0.015)

Notes: Columns 1 and 3 report sample means by age at the time of the eruption. Columns 2 and 4 report results from a covariate balance test. We regress the variable in question on $Destroyed$ and report the coefficient and robust standard errors in parentheses. *Move house after 1960* is a dummy for having moved houses after 1960. *Missing* is a dummy for being missing from the outcome data in 1981. *Years of schooling* is based on educational attainment as of 2011. We only report a balance test on this variable for those 25 and older. The validity of this balance test relies on the assumption that this group has already completed their education by the time of the eruption. We verify this assumption by showing no significant effect on education for this group in Table 7. We do not, however, report a test of balance in years of schooling for the younger cohorts, who have not completed their education by the time of the eruption. *** p<0.01, ** p<0.05, * p<0.1

negative effect on those 25 years of age or older. This pattern argues against an interpretation of our findings based on selection effects.

7 Earnings Effects

The main outcome variables we focus on are labor earnings and education. In this section, we consider the effects on labor earnings. We consider the effects on eduction in section 8. Our measure of earnings includes wage income and proprietors' labor income, but excludes pension income, transfers, and capital income.[9] We have annual earning data for the sample period 1981 to 2014. We restrict attention to earnings in years when individuals are prime age, which we define as being between the ages of 25 and 64 years old. For ease of exposition, we first convert all monetary variables to 2014 prices using the Icelandic CPI and then convert them into US dollars (USD) using an exchange rate of 125 Icelandic króna (ISK) per USD.

Let's consider first the cohorts that were younger than 25 years old at the time of the eruption. For these cohorts, we start with a simple comparison of the average labor earnings by year of those whose houses were destroyed by the eruption and those whose houses were not destroyed by the eruption. This comparison is plotted in Figure 4. The figure illustrates a remarkable reversal of fortune for these younger cohorts. The "bad luck" of having their houses destroyed in the 1973 eruption was associated with persistently higher average earnings over the next 35 years. It is worth noting that this difference in earnings does not seem to be driven by the financial boom that Iceland experienced between 2002 and 2008. The gap opens up long before this and persists after the financial crisis.

Regression estimates of these reduced form results pooled across years are reported in the first two specifications in Table 5. The annual earnings effect of living in a house that was destroyed at the time of the eruption is estimated to be roughly $3,400 in a specification with controls. This estimate is statistically significant at the 1% level. When constructing standard errors, we cluster observations by address to allow for arbitrary correlation across time and across individuals that live at the same address at the time of the eruption.[10] The point estimates are similar with and without controls. The controls we include are age and year fixed effects as well as dummies for

[9] We have considered broader measures of income as well and the results are similar.

[10] We have investigated whether there is broader spatial correlation in our data. Due to data limitations, we can only do this for the data we have on house prices prior to the eruption. We find statistically significant but very small spatial correlation of house prices in the Westman Islands. The magnitude of the spatial correlation we estimate is sufficiently small that we have not pursued further adjustments to our standard errors for spatial correlation. See appendix E for details.

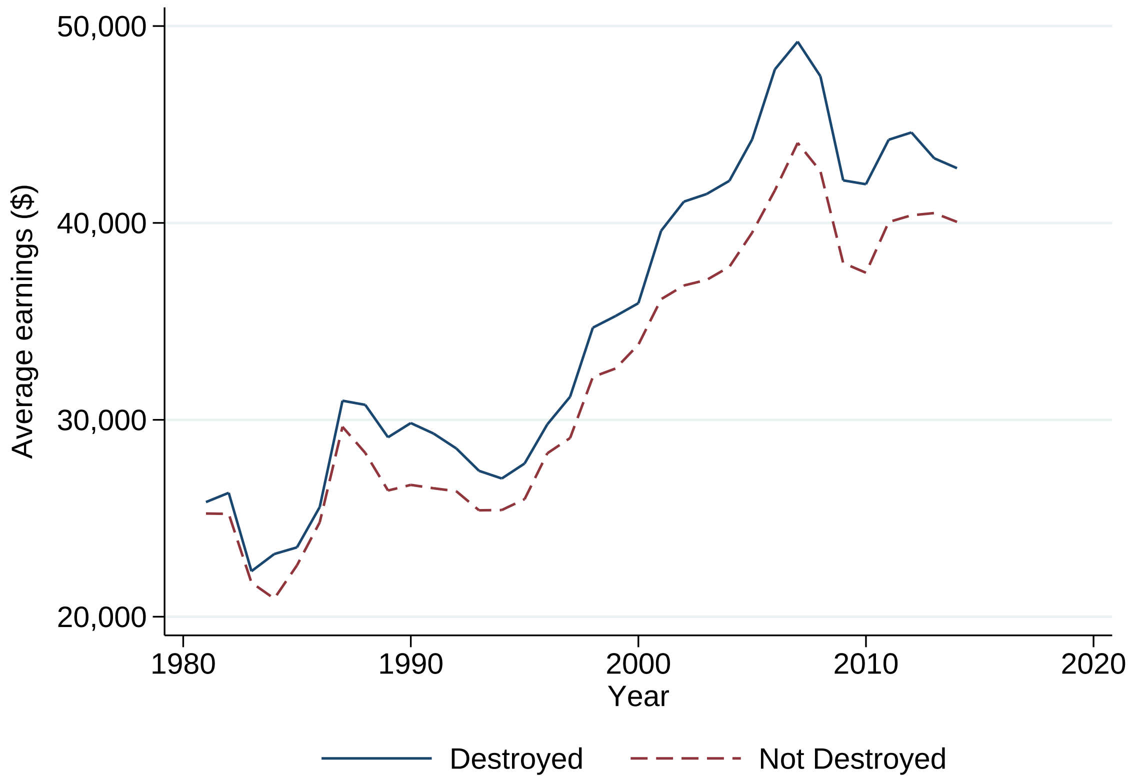


Figure 4: Earnings by Year – Cohorts Younger than 25 at time of Eruption

gender and two controls intended to capture an individual's attachment to the Westman Islands (an indicator for whether the individual was born in the Westman islands and an indicator for whether the individual, or his/her parents, had been living in the same house since 1960).

A simple Wald estimate of the causal effect of moving on earnings can be constructed by dividing the difference in average earnings between the destroyed and non-destroyed samples by the respective difference in the probability of moving. Recall that the difference in moving probabilities—the first stage—is 11.4 percentage points and the difference in earning—the reduced-form—is roughly $3,000 (without controls). The Wald estimate of the annual earnings gain of moving is, therefore, roughly $26,600. This estimate is the third specification reported in Table 5. It is, of course, not unlikely that having one's house destroyed by lava might also affect earnings through other channels than only whether one moves. However, it seems likely that these other channels would negatively affect earnings, making our (already large) estimates of the earnings effect an underestimate.

We also report a two-stage least squares (2SLS) estimate of equation (1) with controls for the demographic factors discussed above. This yields a slightly larger estimate of the causal effect of moving of roughly $27,500, which is equal to 83 percent of the average earnings of the control

Table 5: Effect on Earnings – Cohorts Younger than 25 at Time of Eruption

	Reduced Form		Wald	2SLS	OLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Moved			26,628*	27,532**	-2,570**	-1,906*
			(15,638)	(13,146)	(1,149)	(1,046)
Destroyed	3,037**	3,408***				
	(1,485)	(1,279)				
Control group mean	33,347	33,347	33,347	33,347	—	—
Controls	No	Yes	No	Yes	No	Yes
Age fixed effects	No	Yes	No	Yes	No	Yes
Year fixed effects	No	Yes	No	Yes	No	Yes
Observations	68,539	68,539	68,539	68,539	68,539	68,539

Notes: The dependent variable in all cases is labor earnings. Coefficient estimates are reported in US dollars as of 2014 (125 ISK = 1 USD). The set of controls includes gender, a dummy for having changed houses after 1960, and a dummy for being born in the Westman Islands. Robust standard errors clustered by address are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

group in these regressions. The IV estimates are significant at the 1% level and are somewhat more precise than the Wald estimate since they include controls for the life-cycle profile of earnings.[11] As Figure 4 suggests, these causal effects are not driven by the financial boom that Iceland experienced between 2002 and 2008. We present subsample analysis in appendix B.

Our quasi-experimental design is crucial in estimating the causal effect of moving. Columns 5 and 6 of Table 5 report OLS estimates of equation (1). The resulting estimates of β are slightly negative. The large downward bias of the OLS estimate relative to the IV estimate suggests that movers are overall substantially adversely selected relative to stayers and relative to the "compliers" in our quasi-experiment (i.e., those that are induced to move by having their house destroyed). This finding seems natural in light of the fact that the Westman Islands is a relatively affluent place in Iceland. People moving away from the Westman Islands are likely to do so because of adverse events such as job loss that signal weak unobserved characteristics.[12]

The average treatment effect we estimate in Table 5 is very large. Does this large average treatment effect reflect disproportionate increases at the top of the earnings distribution? Or are they evenly distributed through the earnings distribution? To answer these questions, we estimate

[11] The dependent variable in our baseline specification is the level of earnings. An alternative would be to use the logarithm of earnings. Table A.1 in the appendix reports estimates from this alternative specification. It yields a somewhat larger estimate of the causal effect: moving causes about an 138 percent increase in life-time labor earnings (0.87 log points). As we show in Figure A.1, this difference versus the results in levels is driven partly by very large proportional increases for the lower tail of the earnings distribution.

[12] Yagan (2018) finds that moving is strongly negatively correlated with employment (conditional on age and other demographics).

quantile treatment effects using the methods developed in Abadie, Angrist, and Imbens (2002). We estimate the treatment effect for the 5th to the 95th percentile in 5 percentile increments and then the effect for the 96th-99th percentile in 1 percentile increments.

Figure 5 plots the resulting quantile treatment effects. We find that the treatment effect for the median and for all quantiles between the 15th percentile and the 85th percentile are roughly $20,000, which is roughly 60 percent of the average earnings of the control group. This is a somewhat smaller effect than the average effect reported in Table 5, but still large. Towards the top of the income distribution, the estimated treatment effects rise substantially. Evidently, some people do very well after having been induced to move.[13] Figure A.1 in the appendix plots quantile treatment effects when the logarithm of earnings is the dependent variable. When viewed in proportional terms, it is the lower tail of the distribution of earning that moves the most. However, movements at the top of the distribution are also substantial at roughly 100 percent (0.7 log points).

Figure 6 plots the raw data on average earnings by age separately for those whose houses were and were not destroyed in the eruption. This figure shows how the earnings effects of the lava shock differs over the life-cycle. This simple comparison indicates negative earnings effects early in adulthood—from ages 18 to roughly 25. This likely reflects the fact that those whose houses were destroyed attend school for longer (see section 8). After people's mid-20s the earnings effect is positive. It rises over the life-cycle peaking relatively close to retirement.

One useful way to summarize our results is to do a simple calculation of the net present value of moving. To do this we need to estimate the life-cycle profile of the causal effect of moving—i.e. estimate the earnings effect by age. Appendix C describes the details of the the specification and Panel B of Figure A.2 presents the earnings effects by age. The resulting estimates start off small and grow at least until age 50. At age 50, they are estimated to be roughly $50,000.[14] If we adopt the viewpoint of an 18 year old complier at the time of the eruption, and assume the future is discounted at a rate of 4% per year, the net present value of moving is $444,473.[15]

The large positive causal effects of moving we estimate for those younger than 25 years old at the time of the eruption contrast sharply with our estimates of the causal effects of moving for

[13]We should note that our estimator yields estimates of the causal effect on different quantiles of the distribution of earning, not the causal effect on the person that is at any particular quantile absent treatment. If treatment leads individuals to switch places in the income distribution, these two will be different.

[14]The precision of our estimates diminishes substantially for ages above 50 (since many of those younger than 25 at the time of the eruption are in their 50's at the end of our sample period).

[15]Here we assume that the causal effect remains constant over the age range 50-63 at its estimated value for age 50 and is zero after age 63. If we instead use the estimated coefficients for the 52-63 age range (which are imprecisely estimated), we get a net present value of moving of $518,934. On the other hand, if we assume that the value of moving after age 51 is zero, we we get a net present value of moving of $311,453.

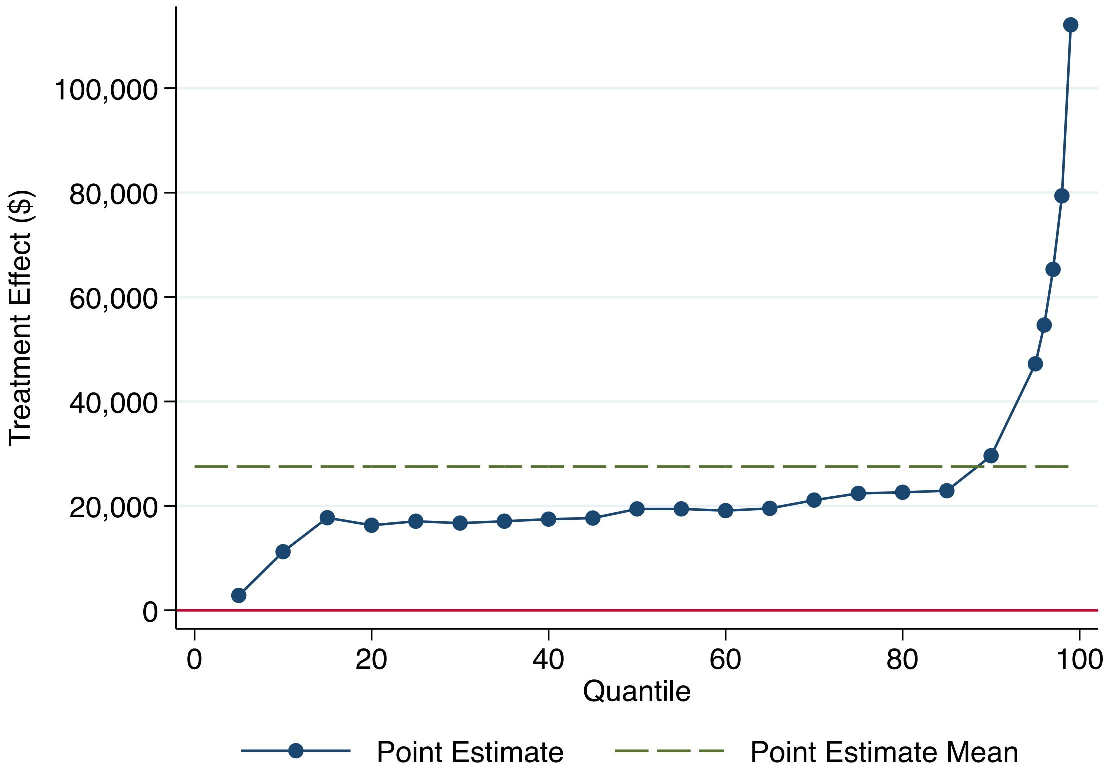


Figure 5: Quantile Treatment Effects on Earnings – Cohorts Younger than 25 at time of Eruption

Note: The figure plots quantile treatment effects using the estimator proposed by Abadie, Angrist, and Imbens (2002) for the 5th to the 99th percentile. The effects are estimated in 5 percentile increments up to the 95th percentile, and in 1 percentile increments for 96th to 99th percentile. The green horizontal dashed-line plots the mean effect (2SLS) for comparison.

those 25 years old and older. Table 6 presents results for this older set of cohorts. For these cohorts, we estimate the causal effect of moving to be a small negative number that is not statistically significantly different from zero. Taken together, these results imply that the benefits of moving are very unequally distributed within families with the children reaping large benefits but the parents bearing the costs.

We have also estimated the effect of the lava shock on the earning of the descendants of those living in the Westman Islands at the time of the eruption. These estimates are reported in Table A.2 in the appendix. The point estimates are large but imprecise, which is not surprising given how young on average this group is during our sample period. More accurate analysis of the earnings effect of the descendant group will be possible after a decade or two.

Our result that the young that disproportionately benefit from moving is consistent with recent work by Chetty, Hendren, and Katz (2016) and Chetty and Hendren (2018) in other settings. Chetty and Hendren (2018) find evidence for a linear exposure effect—i.e., that the benefits of liv-

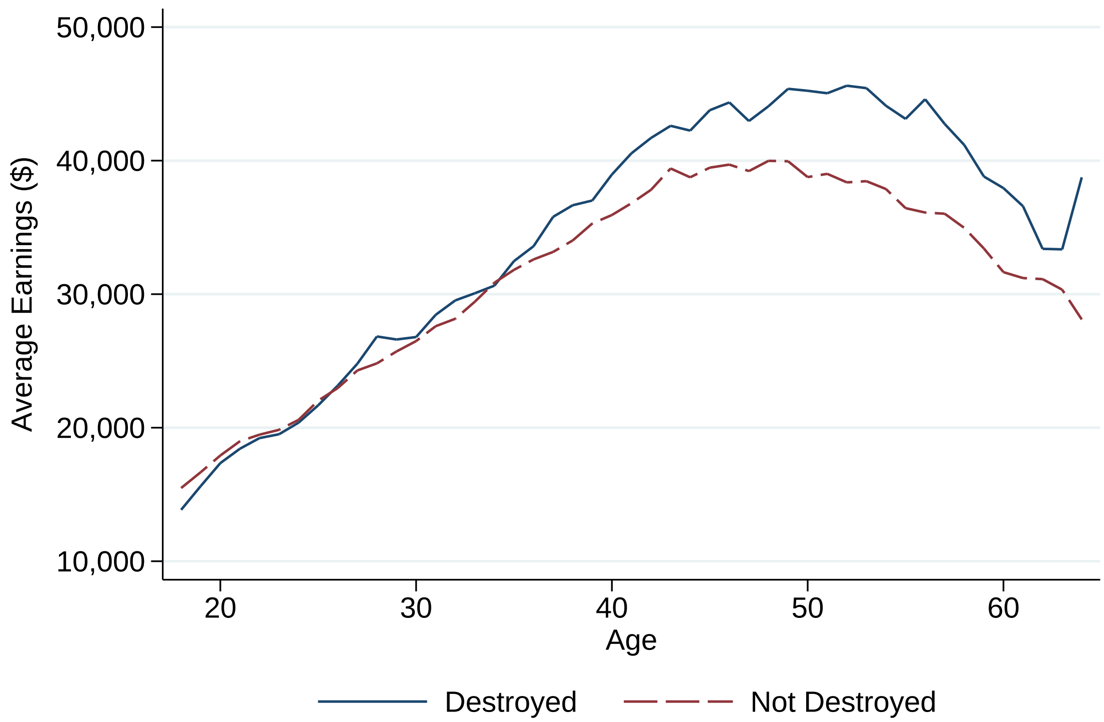


Figure 6: Earnings Effect Over the Life Cycle – Cohorts Younger than 25 at time of Eruption

ing in a "good" location grow linearly with the number of years of childhood exposure to that neighborhood. To shed further light of this in our setting, Figure 7 presents causal effect estimates for four groups of cohorts: those 0 to 9 years old, those 10 to 24 years old, those 25 to 50 years old, and those older than 50 at the time of the eruption. While the estimates for these subgroups are quite noisy, there seems to be a "break" in the causal effect of moving at age 25, but the causal effect for the 0 to 9 year old cohorts is not estimated to be larger than for the 10 to 24 year old cohorts.[16] Our results, therefore, suggest that the crucial distinction is whether individuals had finished their education and settled on a career at the time of the eruption. Those young enough to make changes to the educational choice and shift careers were better able to take advantage of the "opportunity" the lava shock presented them.[17]

[16]We have also run linear specifications similar to those reported by Chetty and Hendren (2018). These do not support the existence of a linear exposure effect in our setting.

[17]Our results also differ from those of Chetty, Hendren, and Katz (2016), who find positive effects only for children who are younger than 13 at the time they move.

Table 6: Effects of Moving on Earnings – Cohorts 25 and Older at Time of Eruption

	Reduced Form		Wald	2SLS	OLS	
	(1)	(2)	(3)	(4)	(5)	(6)
$Moved$			-5,265	-3,931	-3,323***	-3,017***
			(5,149)	(5,374)	(1,029)	(953)
$Destroyed$	-1,024	-725				
	(999)	(992)				
Control group mean	28,089	28,089	28,089	28,089	—	—
Controls	No	Yes	No	Yes	No	Yes
Age fixed effects	No	Yes	No	Yes	No	Yes
Year fixed effects	No	Yes	No	Yes	No	Yes
Observations	30,861	30,861	30,861	30,861	30,861	30,861

Notes: The dependent variable in all cases is labor earnings. Coefficient estimates are reported in US dollars as of 2014 (125 ISK = 1 USD). The set of controls includes gender, a dummy for having changed houses after 1960, and a dummy for being born in the Westman Islands. Robust standard errors clustered by address are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

8 Education Effects

We next estimate the causal effect of moving on educational attainment for those induced to move by our lava shock. Table 7 reports results separately for cohorts younger than 25 at the time of the eruption, cohorts 25 years old and older, and descendants of the original inhabitants (see section 4 for a discussion of how exactly we define the descendant group). We present both OLS estimates and IV estimates where we instrument for $Moved_i$ using $Destroyed_i$. The regressions for the "younger than 25" and "25 and older" groups include as controls gender, cohort, an indicator for whether the individual was born in the Westman islands, and an indicator for whether the individual, or his/her parents, had been living in the same house since 1960. The regressions for the descendants include gender and age as controls.

Our estimates indicate that the lava shock caused those younger than 25 and induced to move by the eruption to increase their educational attainment by 3.5 years. To interpret this large estimate, it is useful to understand the structure of the Icelandic educational system. Iceland has 10 years of compulsory schooling from ages 6 to 16. The next stage in the Icelandic educational system is a four-year junior college degree (usually done from ages 16 to 20). Junior college has traditional academic tracks required for university enrollment, as well as vocational tracks such as carpentry and hairdressing.

Table 8 presents estimates of the causal effect of the lava shock on the probability of finishing

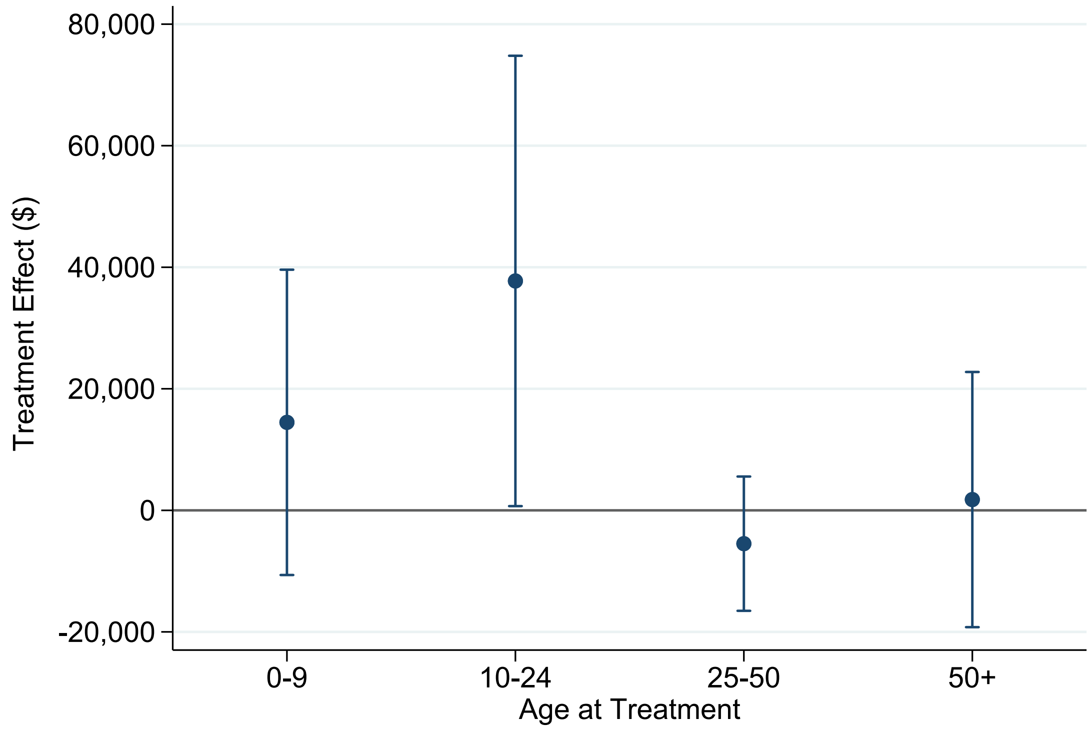


Figure 7: IV Earnings Effect – Four Age Groups

a junior college degree and a university degree. Moving raises the probability of getting a junior college degree by 63 percentage points. The corresponding estimate for the probability of getting a university degree is positive, with a point estimate of about 23 percentage points, but the effect is not statistically significant. The 3.5 additional years of schooling induced by moving therefore mostly reflect a large increase in the rate of attending junior college.

The lava shock has an even larger causal effect on the educational attainment of the descendants of those living in the Westman Islands at the time of the eruption than on the inhabitants themselves. Our estimate of the causal effect on the descendants is 5.7 years of extra schooling. This estimate, though large, may be somewhat downward biased. The youngest cohort in the descendant group was only 15 years old in 2011 (the year for which we have data on educational attainment). Many individuals in the youngest cohorts of the descendant group had therefore not yet finished their educational attainment.

In contrast, the causal effect of our lava shock on the education of those 25 years old and older at the time of the eruption, while positive, is small and statistically insignificant. It may seem natural to view this as a placebo test. However, the forgiving nature of the Icelandic education

Table 7: Effect of Moving on Years of Schooling

	Younger than 25		25 and Older		Descendants	
	IV	OLS	IV	OLS	IV	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
$Moved$	3.54**	0.13	0.81	0.13	5.69**	-0.24**
	(1.77)	(0.16)	(0.77)	(0.15)	(2.49)	(0.11)
Control group mean	13.40	13.40	11.94	11.94	12.71	12.71
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	2,262	2,262	1,101	1,101	3,207	3,207

Notes: The dependent variable is years of schooling for the group listed at the top of each column. In the first four columns, we report robust standard errors clustered by address in parentheses. In the last two columns, we report robust standard errors clustered by individual are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Effects on Post-Compulsory Education
Cohorts Younger than 25 at Time of Eruption

	Junior College	University
	(1)	(2)
$Moved$	0.638**	0.226
	(0.283)	(0.212)
Control group mean	0.609	0.224
Controls	Yes	Yes
N	2,262	2,262

Notes: The dependent variable is listed at the top for each column (Junior College degree or University degree). In all cases, we report IV regression results with $Moved_i$ instrumented with $Destroyed_i$. Robust standard errors clustered by address are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

system makes this a somewhat imperfect placebo test. In Iceland it is not uncommon for people to return to school in adulthood, finish previously started but unfinished degrees, and take additional courses and certificates, such as specialized vocational education. The fact that our point estimate is positive for this group (yet statistically insignificant) may be reflecting this channel.

9 Interpretation

We have shown that our lava shock caused a large increase in life-time income and educational attainment for those younger than 25 years old at the time of the eruption who were induced to move. These results cannot be explained simply by a (possibly unanticipated) decline in the

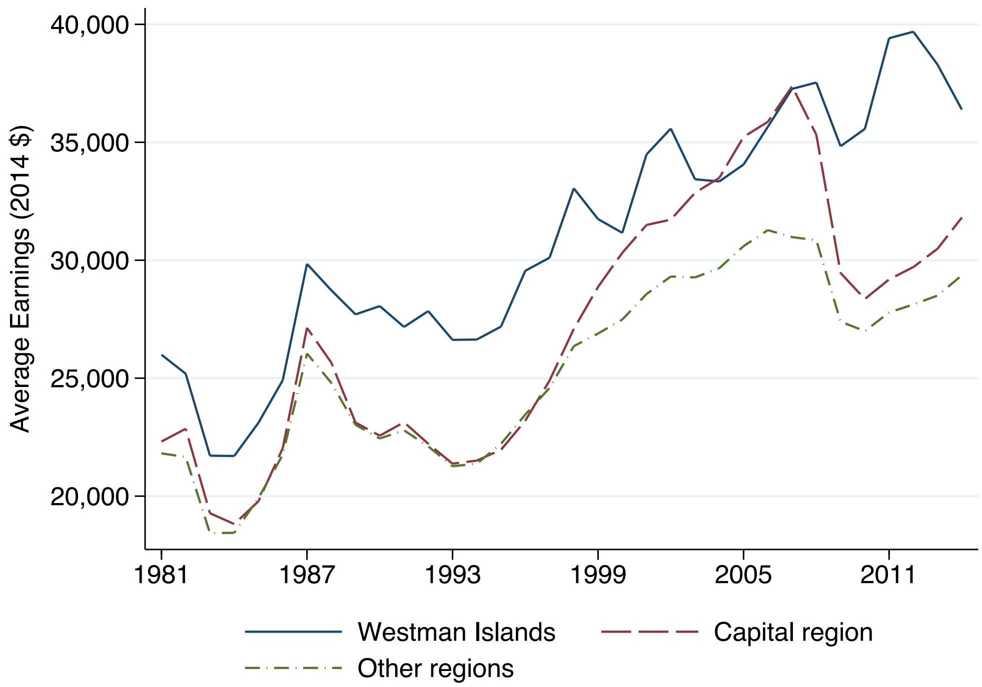


Figure 8: Evolution of Average Earnings Across Locations

Note: The figure plots average earnings across all taxpayers by their municipality of residence in the given year. "Capital region" includes Reykjavik (the capital) and surrounding municipalities. "Other regions" includes all municipalities not included in the groups "Capital region" or Westman Islands.

returns to fishing or by the Westman Islands being a fundamentally bad place to earn income during our sample period. To the contrary, fishing has been highly profitable in Iceland and the Westman Islands has been a very high income place over our sample period. Figure 8 shows that average earnings in the Westman Islands have been substantially higher than in Iceland's capital area (Reykjavik and suburbs) except for a few years during the financial boom last decade. In contrast to much prior work, our setting is, thus, one in which people gain a great deal from moving away from a high income location to locations with lower average income.

This raises the question of how it can possibly be so beneficial to move away from the Westman Islands. The most compelling explanation, in our view, is that the causal effect of moving away is highly heterogeneous across people due to the importance of comparative advantage. Like many small places, the Westman Islands is specialized in a narrow set of industries. In the case of the Westman Islands, these industries happen to be fishing and fish processing. These two industries alone account for roughly 70% of income in the Westman Islands, relative to less than 15% in Iceland as a whole.[18] The highly specialized nature of the labor market in the Westman Islands

[18]See Table A.5 for further details. These statistics combine "Fishing and Agriculture" and "Fish and Food Processing". However, since there is virtually no agriculture in the Westman Islands, the true extent of specialization is even

likely means that this is a good place for some to work—i.e., those whose comparative advantage lies in skills valued in the fishing industry—but a much worse place for others—i.e., those whose comparative advantage lies in jobs requiring a large amount of eduction such as law, computer science, engineering, or medicine.

If moving costs are large, people who are not a good match for living in the Westman Islands may nevertheless remain on the island. For this group, the causal effect on earnings of moving away is potentially very high. At the same time, the causal effect for those well matched to the islands is likely much lower, and may even be negative. In other words, the compliers in our experiment are likely to be selected as those with a particularly high causal effect of moving. Moreover, there are important intergenerational tradeoffs that come into play in the decision to move. The benefits of moving may be quite different for the children (who have yet to complete their education and choose a career) versus their parents, whose choice of education and career is more likely to be fixed, even if they move to a location where the returns have changed.

9.1 A Model of Comparative Advantage

To illustrate these ideas, it is useful to write down a Roy model with heterogeneous comparative advantage, moving costs, and overlapping generations. The model we develop is based on the models in Lagakos and Waugh (2013), Young (2013), Bryan and Morten (2018), and Adao (2015). Our model generalizes Adao's model to include moving costs, an educational choice, and overlapping generations, while simplifying it along several other dimensions.

Consider an economy with two regions: the Westman Islands and the mainland of Iceland. For simplicity, we assume that each region has a single sector. The economy of the Westman Islands is engaged in fishing, while the mainland of Iceland is engaged in non-fishing. We use the generic index k to denote the sectors and denote fishing by F and non-fishing by N.

The Westman Islands is populated by a measure I of families. Each period, family i is made up of two generations: parents and children. For simplicity, we model the parents in each family as a single agent and the children, also, as a single agent. Agents live for two periods. In their first period of life, they are children and in their second period of life they are parents. We denote parents by p and children by c.

Parents inelastically supply one unit of labor to market work. They are endowed with a bivariate skill vector $(z_F^p(i), z_N^p(i))$, where $z_k^p(i)$ is the number of efficiency units of labor that parents

greater than what the statistics suggest.

from family i produce if employed in sector k. It is convenient to define parent i's comparative advantage in the non-fishing sector to be

$$s^p(i) \equiv \log(z_N^p(i)/z_F^p(i))$$

and her absolute advantage to be

$$a^p(i) \equiv \log z_F^p(i).$$

The joint distribution of $(z_F^p(i), z_N^p(i))$ can then be described in terms of a distribution for comparative advantage $s^p(i) \sim F(s)$ and a conditional distribution for absolute advantage $\{a^p(i)|s^p(i) = s\} \sim H(a|s)$.

Children inherit the skills of their parents with some error. We model the inheritability of skills as an intergenerational AR(1) process for comparative and absolute advantage:

$$s^c(i) = \rho_s s^p(i) + \epsilon^s(i),$$

$$a^c(i) = \rho_a a^p(i) + \epsilon^a(i),$$

where $\epsilon^s(i)$ and $\epsilon^a(i)$ are mean zero i.i.d. shocks and the parameters ρ_s and ρ_a take values between zero and one. Childrens' skills are not known until they become adults.

Parents face two choices: whether to move and whether to educate their children. Their choices are made to maximize a utility function given by

$$\log(C^p(i)) + \beta \mathbb{E} \log(C^c(i)),$$

where $C^p(i)$ is the parents' (family) consumption and $C^c(i)$ is their childrens' (family) consumption in adulthood. The parameter β captures the degree of altruism of parents towards their children. We assume that there is no inter-generational borrowing or saving.

We focus our analysis on the decisions of the parents at the time of the volcanic eruption. For simplicity, we abstract from the possibility that their children will want to move when they become adults. We have considered the more general case. Allowing the children to move when they are adults complicates the analysis considerably without yielding further insight. One interpretation of our no-future-moving assumption is that the eruption is a very special event that lowers moving costs (both for those that lose their house and those that don't) and that in other periods moving costs are sufficiently high that few people move.

Moving to the mainland is costly. We denote this cost by $m(i)$. The form that this cost takes is that a fraction $1 - \exp(-m(i))$ of the parents' labor income is lost when they move. The moving costs may differ across households. For example, it may be lower for households whose house is destroyed if this event reduces their attachment to the Westman Islands.

We assume that the returns to education on the mainland are sufficiently high that parents choose to educate their children if they move to the mainland. We denote the cost of education by f. As with moving costs, the form that the education cost takes is that parents lose a fraction $1 - \exp(-f)$ of their income if they educate their children. Being educated increases childrens' non-fishing income by a factor $\exp(\phi(i))$. The benefits of education may also differ across households, i.e., some households may have a comparative advantage when it comes to making use of education. Education is not useful in the fishing industry. In our model, parents that stay in the Westman Islands, therefore, do not educate their children.

Labor is the only factor of production and firms produce using linear production functions

$$Y_F = A_F L_F \quad \text{and} \quad Y_N = A_N L_N,$$

where

$$L_F = \int_{i \in \mathcal{S}^F} z_F(i) di, \quad L_N = \int_{i \in \mathcal{S}^N} z_N(i) di,$$

and S^k denotes the set of workers employed in sector k.

The labor markets in both sectors are perfectly competitive. Furthermore, the Westman Islands is a small place that takes the prices of both fish, denoted P_F, and non-fish, denoted P_N, as given. These assumptions imply that the wages per efficiency unit of labor in fishing and non-fishing are given by

$$W_F = P_F A_F \quad \text{and} \quad W_N = P_N A_N,$$

respectively. The labor income of worker i in sector k before adjustment for education is therefore $Y_k(i) = W_k z_k(i)$, i.e., the wage in that sector times the number of efficiency units of labor the worker can supply.

Using the definitions of comparative advantage and absolute advantage, we can write the logarithm of labor income of parents and children in family i as

$$\begin{aligned}
y_N^p(i) &= w_N + a^p(i) + s^p(i), \\
y_F^p(i) &= w_F + a^p(i), \\
y_N^c(i) &= w_N + a^c(i) + s^c(i) + \phi(i), \\
y_F^c(i) &= w_F + a^c(i),
\end{aligned}$$

where lower case letters refer to the logarithm of upper class letters (i.e., $y_N^p(i) = \log Y_N^p(i)$).

Taking account of moving costs and the costs of education, we can write the logarithm of the consumption of parents and children in family i as

$$c^p(i) = \begin{cases} w_N + a^p(i) + s^p(i) - m(i) - f & \text{if they move,} \\ w_F + a^p(i) & \text{if they stay,} \end{cases}$$

$$c^c(i) = \begin{cases} w_N + a^c(i) + s^c(i) + \phi(i) - f & \text{if parents move,} \\ w_F + a^c(i) & \text{if parents stay.} \end{cases}$$

Notice that if the parents move, they choose to educate their children—this is the f in the first line above—and the children also choose to educate *their* children in the subsequent period—this is the f in the third line above.

It is convenient to rank families according to the comparative advantage of the parents. For each quantile $q \in [0, 1]$, let $\alpha(q) \equiv F^{-1}(q)$ denote the level of comparative advantage at quantile q. By construction, $\alpha(q)$ is increasing in q. Agents at higher quantiles q have a stronger comparative advantage in the non-fishing sector, or equivalently a stronger comparative disadvantage in fishing.

Expected average log earnings for parents and children of quantile q in the non-fishing and fishing sectors are

$$\bar{Y}^p_N(q) = w_N + A(q) + \alpha(q), \tag{3a}$$

$$\bar{Y}^p_F(q) = w_F + A(q), \tag{3b}$$

$$\mathbb{E}\bar{Y}^c_N(q) = w_N + \rho_a A(q) + \rho_s \alpha(q) + \phi(q), \tag{3c}$$

$$\mathbb{E}\bar{Y}^c_F(q) = w_F + \rho_a A(q). \tag{3d}$$

Here $A(q)$ denotes the mean of the absolute advantage conditional distribution $H(a|\alpha(q))$ at quantile q and $\phi(q)$ is the mean benefit of education for workers of quantile q.

9.1.1 Simplified Version Explained Visually

As a stepping stone towards understanding the full model, let's first briefly consider a simplified version of the model where there is a single generation, no education, and all workers face homogeneous moving costs. In this case, average earnings of quantile q in the non-fishing and fishing sectors are

$$\bar{Y}_N(q) = w_N + A(q) + \alpha(q) \quad \text{and} \quad \bar{Y}_F(q) = w_F + A(q),$$

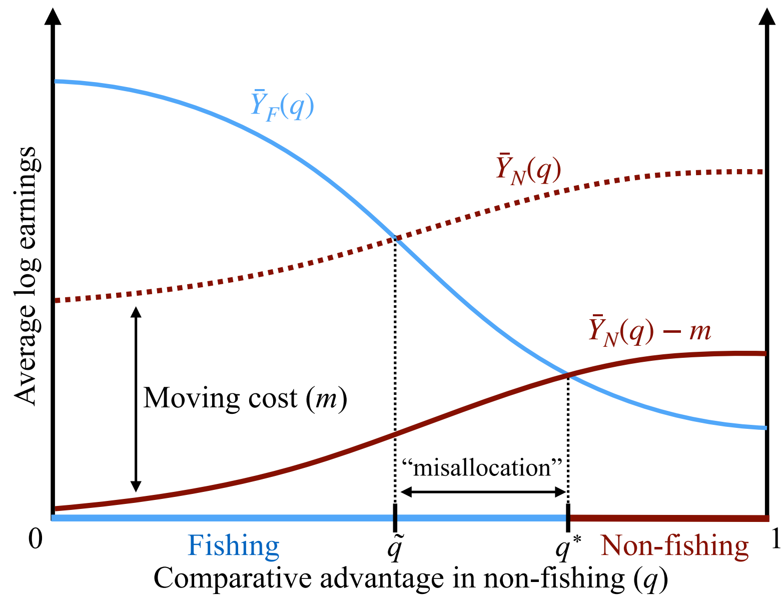


Figure 9: Sorting by Comparative Advantage

respectively. Figure 9 illustrates the economics of the model visually. If a worker chooses to work in the fishing sector, she will on average earn $\bar{Y}_F(q)$ (the light blue line). This will also be her consumption. If she chooses to work in the non-fishing sector, she will earn $\bar{Y}_N(q)$ (the dashed dark red line). In this case, however, she will need to move away from the Westman Islands, which is costly. Taking account of these moving costs, her level of consumption will on average be $\bar{Y}_N(q) - m$ (the solid dark red line).

We have drawn Figure 9 with $\bar{Y}_F(q)$ downward sloping and $\bar{Y}_N(q)$ upward sloping. This means that workers that have a comparative advantage in fishing (i.e., low q workers) are more productive at fishing than those that have a comparative advantage at non-fishing and vice versa. While this may seem like a natural case, the theory we have laid out can accommodate cases in which both $\bar{Y}_F(q)$ and $\bar{Y}_N(q)$ are upward sloping (those with a comparative advantage at non-fishing are also better at fishing) and cases in which both $\bar{Y}_F(q)$ and $\bar{Y}_N(q)$ are downward sloping (those with a comparative advantage at fishing are also better at non-fishing). All that we assume is that $\bar{Y}_N(q)$ has a larger slope than $\bar{Y}_F(q)$ (i.e., workers differ in their comparative advantage).

In equilibrium, workers will self-select into the sector in which they earn the most net of moving costs. Figure 9 shows that this will give rise to a unique cutoff quantile q^* below which all workers choose to be fishermen and above which all workers choose to move away from the Westman Islands and take up employment in the non-fishing sector.

Figure 9 also shows clearly how the moving cost leads to misallocation of labor. If moving were not costly, workers at quantile q would choose between $\bar{Y}_F(q)$ and $\bar{Y}_N(q)$ rather than $\bar{Y}_F(q)$ and $\bar{Y}_N(q) - m$. In this case, a larger fraction of workers would move away from the Westman Islands (and presumably a larger fraction of mainland workers would also move to the Westman Islands). The cutoff quantile in this no-moving-cost case would be $\tilde{q}$. The moving cost implies that workers between $\tilde{q}$ and q^* are misallocated and are earning less than they would without the moving cost.

9.2 The Volcanic Experiment

Let's now consider the situation at the time of the eruption in the full model with parents and children. Our empirical results in sections 5 and 6 indicate that at the time of the eruption a fraction of families in the Westman Islands exogenously faced a lower moving cost than other families because their houses were destroyed in the eruption. We therefore consider a situation where a fraction of families (those whose house was destroyed) face a moving cost of m', while other families face a moving cost of $m > m'$.

The decision to move is made by the parents. They decide whether to move by comparing their expected utility from moving with their expected utility from staying. This comparison implies that a family moves if

$$\bar{Y}_N^p(q) + \beta \mathbb{E} \bar{Y}_N^c(q) - m(i) - (1+\beta)f > \bar{Y}_F^p(q) + \beta \mathbb{E} \bar{Y}_F^c(q),$$

where $m(i)$ is either m or m'. Using equations (3a)-(3d) we can rewrite this condition as

$$(1 + \beta\rho_s)\alpha(q) + \beta\phi(q) > m(i) + (1+\beta)f + (1+\beta)(w_F - w_N). \tag{4}$$

The left-hand side of this condition is the benefit of moving, while the right-hand side is the cost of moving. If we assume that $\phi(q)$ is constant or increasing in q—i.e., that families with a comparative advantage in non-fishing also gain (weakly) more from being educated—the left-hand side of the inequality (4) is increasing in q, while the right-hand side is a constant for each value of $m(i)$. This implies that for families whose house was destroyed there is a unique $q^{*\prime}$ such that among these families, those with $q \in [q^{*\prime}, 1]$ move away from the Islands. The cutoff $q^{*\prime}$ solves the equation

$$(1 + \beta\rho_s)\alpha(q^{*\prime}) + \beta\phi(q^{*\prime}) = m' + (1+\beta)f + (1+\beta)(w_F - w_N).$$

Analogously, for families whose house was not destroyed there is a different unique cutoff $q^* > q^{*\prime}$ such that among these families, those with $q \in [q^*, 1]$ move away from the Islands.

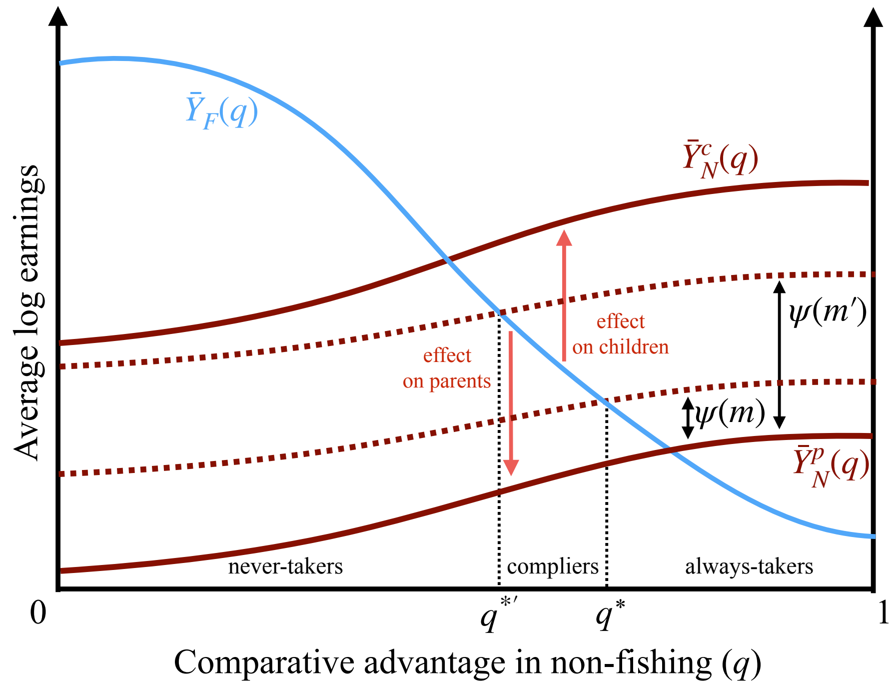


Figure 10: The Volcanic Experiment

This situation is depicted in Figure 10. As before, the downward-sloping light-blue line depicts average earnings of fishermen as a function of comparative advantage in non-fishing $\bar{Y}_F(q)$ (same for both generations). The two solid dark-red lines depict average earning on the mainland for parents and children. The gap between the two lines represents the gains from education in non-fishing. We have drawn the figure for the case where those with a comparative advantage in non-fishing also gain (slightly) more from being educated (the $\bar{Y}_N^c(q)$ line is (slightly) steeper than the $\bar{Y}_N^p(q)$ line). The equation for the cutoff points $q^{*\prime}$ and q^* can be rewritten as

$$\bar{Y}_N^p(q) + \Psi(m(i), q) = \bar{Y}_F^p(q), \tag{5}$$

where

$$\Psi(m(i), q) = \frac{1}{1+\beta}[\beta\phi(q) - m(i) - (1+\beta)f - \beta(1-\rho_s)\alpha(q)]. \tag{6}$$

The two dashed red lines in Figure 10 plot the left-hand side of equation (5) for the two values of moving costs m' and m. The points where these lines cross the $\bar{Y}_F(q)$ line are the two cutoff quantiles $q^{*\prime}$ and q^*.

It is now straightforward to map our various empirical facts into the model. For this purpose, it is useful to begin by dividing the workers into three groups based on the terminology of Angrist

(2004). Workers to the left of $q^{*\prime}$ in Figure 10 are "never-takers." These workers have such a strong comparative advantage in fishing that they don't move even if their house is destroyed. Workers between $q^{*\prime}$ and q^* are "compliers." These are the workers that are induced to move by having their house be destroyed, i.e., they move only if their house is destroyed. Finally, workers to the right of q^* are "always-takers." These workers have such a strong comparative disadvantage in fishing that they move even if their house is not destroyed.

Our IV estimates reflect the causal effect of moving on the compliers (Imbens and Angrist, 1994). Let's start by considering the children. In Figure 10, the causal effect of moving for children at a given level of comparative advantage q is the vertical distance between $\bar{Y}_N^c(q)$ and $\bar{Y}_F(q)$. The figure shows clearly that in our setting the complier children are highly selected to have a large causal effect of moving. This fact helps explain the large magnitude of the causal earnings effects we estimate. Intuitively, the complier children are relatively poorly suited to live in the Westman Islands. This is why their parents can be induced to move away and also why they themselves gain so much from moving.

We can also read the causal effect on the parents—the vertical distance between $\bar{Y}_N^p(q)$ and $\bar{Y}_F(q)$—off of Figure 10. It is much smaller than the causal effect on the children. The reason for this is that the parents are not educated and therefore benefit less from moving to the mainland. We have drawn Figure 10 such that the causal effect on the complier parents is negative as in our empirical estimates. Whether this effect is positive or negative depends on the sign of $\Psi(m(i), q)$. Equation (6) reveals that this depends on the parents' level of altruism toward their children β and the size of the education effect $\phi(q)$. With a large degree of altruism and a large education effect, parents will be induced to move even if the effect on their own earning is negative because the large effect on their children's earnings outweighs their own losses.

It is evident from Figure 10 that the causal effect of moving in our model is highly heterogeneous depending on comparative advantage. In particular, the causal effect on the never-takers is smaller than the causal effect on compliers and can easily be negative even for children. In Figure 10, the causal effect on most never-taker children is negative (all of those to the left of the point where the $\bar{Y}_F(q)$ line crosses the $\bar{Y}_N^c(q)$ line). These families have a strong comparative advantage in the fishing sector. They would be made worse off if they had to move to the non-fishing sector even if there were no direct moving cost. Our model therefore has the property that even though some can be made much better off by moving, this is not true of all. A policy of moving everyone away from the Westman Islands may be a terrible policy even despite our large positive IV estimates because there are these other groups that are well matched to the Westman Islands and would be made worse off by having to move.

Figure 10 provides a natural explanation for the "puzzle" we posed at the start of this section: how can it be that the causal effects of moving are so positive even though people are moving away from a high income location? We have drawn the figure such that the average income across never-takers is high (higher than the average income of compliers and always-takers). This reflects the fact that fishing is very profitable in Iceland, and those with a comparative advantage in this sector therefore earn high income on average (higher than the average of those with a comparative advantage in other sectors). This is in no way inconsistent with the notion that the causal effect of moving away from fishing can be very high for those not well suited to work in this sector. Hence, even though the causal effect on the complier children is large and positive, average income can easily be higher for those who remain in the Westman Islands (a weighted average of $\bar{Y}_F(q)$ for the never-takers and non-treated compliers) than those who move away (a weighted average of $\bar{Y}^c_N(q)$ and $\bar{Y}^p_N(q)$ for the always-takers and treated compliers).

This logic also provide a simple explanation for why the OLS estimate of income on moving is so much lower than the IV estimate for the young in our setting. The OLS estimator compares the income of all of those that move with all of those that stay. The stayers are the never-takers and the non-treated compliers, while the movers are the always-takers and the treated compliers. The OLS estimate therefore takes a difference between the average of $\bar{Y}^c_N(q)$ from $q^{*\prime}$ to 1 and $\bar{Y}_F(q)$ from 0 to q^*.[19] This can easily be negative for both the parents and the children even though the causal effect on the complier children is always large and positive.

An important implication of our model is that barriers to mobility can result in large amounts of misallocation even across locations that have similar levels of average income. Many locations—especially smaller ones—are specialized in terms of their occupational mix. Large moving costs will then imply that people born in these locations who happen to have a strong comparative advantage in occupations not well represented in that location could have substantially higher lifetime earnings were these barriers to mobility eliminated.

A concern regarding the interpretation given above is that the large causal effect of moving for the compliers may have been an ex post fluke due to aggregate shocks after the eruption rather than something that could have been rationally anticipated at the time of the eruption (Rosen-

[19]It is important to remember that there are two types of households at each value of q in Figure 10: those whose house was destroyed and those whose house was not destroyed. In the complier region, those whose house was destroyed move, while those whose house was destroyed do not.

zweig and Udry, 2018). This concern is difficult to rule out completely. However, Figure 4 shows that the relative labor market outcomes for treated and untreated individuals in our sample are quite stable over our 34 year sample period. If we assume that the statistical process that generated this stable process of relative returns during our sample period also applied to the period prior to our sample period running back to the eruption, a large difference between ex-ante and ex-post returns is unlikely. Only a large and extremely persistent shock could result in such a difference, but the stability over our sample period implies that such shocks are uncommon.

9.3 Barriers to Moving

Equation (6) is helpful for understanding what factors impede moving in our setting—i.e., what we have referred to as "moving costs broadly defined." One such factor is imperfect altruism by parents towards their children. The level of altruism of parents towards their children is captured by β in our model. If β is small, parents will place low value on their childrens' gains from education ($\phi(q)$) and consequently be less inclined to move.

Another factor is imperfect information about the returns to moving. The decision to move will depend on perceived returns rather than actual returns. In our model this means the perceived gains from eduction (perceived $\phi(q)$) and perceived earnings in the non-fishing sector (perceived $\bar{Y}_N^p(q)$). If the parents are pessimistic about either of these factors, this will hinder mobility in the same way as traditional moving costs. This type of friction has been emphasized in, e.g., the context of returns to education (Manski, 1993). In settings where education and income are low, perceived returns to education are much smaller than actual returns (Jensen, 2010).

But pessimism is not the only way in which imperfect information can impede mobility. Risk has this effect as well. In appendix F, we extend our model to allow for Epstein-Zin preferences and uncertain returns to education. In this case, the equivalent expression to equation (6) is

$$\Psi(m(i),q) = \frac{1}{1+\beta}\left[\beta\left(\bar{\phi}(q) - \frac{\gamma}{2}\sigma_\phi^2\right) - m(i) - (1+\beta)f - \beta(1-\rho_s)\alpha(q) - \frac{\gamma}{2}\sigma_s^2\right], \tag{7}$$

where σ_ϕ^2 and σ_s^2 denote the variance of the education effect and the variance of the intergenerational shock to comparative advantage, respectively, and γ denotes the coefficient of relative risk aversion. Moving away from log-utility and adding uncertain returns to eduction results in two additional terms in equation (7) relative to equation (6): the σ_ϕ^2 term and the σ_s^2 term. Both terms enter the right-hand side of equation (7) with a negative sign. In this case, therefore, risk about the effect of education and risk regarding future comparative advantage act to hinder mobility in the same way as traditional moving costs.

The returns to moving may be particularly difficult to estimate when the industry structure differs between the location of origin and destination, as in the case of the Westman Islands. Furthermore, the fact that the decision to move is made by the parents no doubt exacerbates the informational frictions. Not only does a future computer genius or great legal mind need to understand that he or she will have higher earnings on the mainland, but this information needs to be communicated to his or her parents. All of this suggests to us that information frictions may play an important role in explaining the large barriers to moving that we estimate.

While the large causal effect of moving that we estimate (present value of roughly $444,000) indicates that barriers to moving are large, this does not provide an estimate of the *difference* in moving costs between those whose houses were destroyed and those whose houses were not destroyed. This difference—i.e., the reduction in moving costs resulting from one's house being destroyed—is potentially much smaller. Figure 10 illustrates this point clearly. The reduction in moving costs resulting from one's house being destroyed is equal to the vertical distance between the two dashed red lines in Figure 10. The size of this difference determines the size of the complier group. It is, however, not related to the size of the causal effect on the children, which is equal to the distance between the light-blue line and the top solid red line.

9.4 Evidence of Comparative Advantage

The model analyzed above illustrates that heterogeneous comparative advantage across workers provides a natural explanation for the large causal effect of moving we estimate. In this section, we support this view by presenting evidence indicating that the Westman Islands is a place that specializes in occupations for which the value of education is low, and is therefore a poor match for people with a comparative advantage in "brainy" occupations.

While the fishing industry pays high wages, it requires little formal education. One sign of this is that educational attainment in the Westman Islands is low. Table 9 reports educational attainment in the Westman Islands, Iceland's capital area, and other areas in Iceland. Educational attainment is substantially lower in the Westman Islands than in Reykjavik. Only 20% of the working age population has a university degree, compared to 40% in the capital region.

Another sign that comparative advantage for "brainy" occupations is an important factor in our results derives from our analysis of the characteristics of the compliers in our natural experiment—i.e., those induced to move by the volcanic eruption. Although individual compliers cannot be identified in the data, their average characteristics can be estimated when the instru-

Table 9: Educational Attainment by Location

	Westman Islands	Capital Region	Other Regions
Compulsory education	40%	25%	41%
Junior college education	39%	36%	36%
University education	20%	39%	22%

Notes: Data from the 2011 Educational Census. People aged 25-64 in 2011. *Source*: Statistics Iceland.

Table 10: Complier characteristics ratios – Cohorts Younger than 25 at Time of Eruption

Variable (X)	$\Pr[X_i = 1]$	$\Pr[X_i = 1\|\text{Complier}]$	$\frac{\Pr[X_i=1\|\text{Complier}]}{\Pr[X_i=1]}$
Female	0.49	0.34	0.69 (0.20)
Age (> median)	0.51	0.40	0.79 (0.18)
Change house after 1960	0.60	0.75	1.25 (0.25)
Born in Westman Islands	0.80	0.82	1.03 (0.13)
House value (> median)	0.64	0.68	1.06 (0.16)
House year (> median)	0.61	0.72	1.17 (0.32)
Parents education (> compulsory)	0.50	0.75	1.51 (0.36)
Parents married	0.88	1.05	1.19 (0.10)

Notes: The first column reports the fraction of the overall population for which the characteristic applies. The second column reports this same statistic only for compliers. The third column reports the relative frequency for compliers relative to the overall population. *Parents education* is a dummy variable that equals 1 if one or both parents have more than compulsory education. Standard errors for the characteristics ratios clustered by address are reported in parentheses.

mental variable is binary (Angrist, 2004). The basic intuition is that we can uncover the statistical characteristics of the always-takers and never-takers in our data by looking at those whose houses were destroyed and did not move (never-takers) and those whose houses were not destroyed and moved anyway (always-takers). The statistical characteristics of the compliers can then be inferred by comparing these groups to the whole sample and using Bayes rule.[20]

Table 10 reports the frequency of a set of characteristics among the cohorts that were younger than 25 years old at the time of the eruption. We report the frequency within this entire group (column 1), among the compliers in this group (column 2), and the ratio of these frequencies (column 3). What stands out is that the compliers are roughly 50% more likely to have parents that had post-compulsory education than the typical Westman Islander.

An extensive literature has documented that parents with higher education levels also have children with higher education levels (see, e.g., Black and Devereux, 2010), and that this partly reflects correlated traits between parents and children (Black, Devereux, and Salvanes, 2005). The

[20] For further discussion on estimation of treatment effects under imperfect compliance, see Imbens and Angrist (1994) and Angrist and Pischke (2009).

fact that the compliers in our experiment come from homes with highly educated parents, thus, suggests that they may be particularly likely to have a comparative advantage in occupations that require relatively large amounts of education.

9.5 A Model of Absolute Advantage

It is useful to contrast our preferred comparative advantage interpretation of our empirical results with an interpretation based only on absolute advantage. In a seminal paper, (Abowd, Kramarz, and Margolis, 1999) (hereafter, AKM) model worker income $y_{i,j}$ as the sum of a worker effect, a firm (or in our case, location) effect, and an error term:

$$y_{i,j} = a_i + b_j + \epsilon_{i,j}. \quad (8)$$

Here a_i is the worker effect and b_j is the location effect. In empirical applications, the location effect in this model is identified by looking at movers. For our application, let's denote the location effect for the Westman Islands by b_W and the location effect for the rest of Iceland by b_I.

Since we estimate a large causal effect of moving away from the Westman Island for the cohorts younger than 25 years old at the time of the eruption, the AKM model implies that the Westman Islands has a worse location effect than the rest of Iceland, i.e., $b_W - b_I < 0$, for these cohorts. In other words, the Westman Islands is a "bad" location from the perspective of earning income for this group.

But as we emphasize above, average income in the Westman Islands is substantially higher than average income in the rest of Iceland. Given that the Westman Islands is a "bad" place, the only way to explain the high average income in the Westman Islands within the context of AKM's model of absolute advantage is that the workers in the Westman Islands have much higher average person effects (a_i's) than their counterparts in the rest of Iceland, In other words, the young people living in the Westman Islands at the time of the eruption must have been hugely positively selected in terms of their ability to earn income relative to young people elsewhere in Iceland.

While this alternative explanation is logically consistent, we do not view it as particularly plausible. One reason for this is that standard measures of human capital accumulation do not support this view. Educational attainment is low in the Westman Islands (Table 9). Students from the Westman Islands also perform poorly on standardized tests relative to their peers elsewhere in Iceland: The average test score for the Westman Islands ranks towards the bottom of the distribution of average test scores across schools in Iceland in all subjects (see Figure A.5 for details).

Of course, Westman Islanders may be particularly well-endowed in the human capital needed to carry out the specific tasks that are done on the Westman Islands. But that suggests the model of comparative advantage we present in sections 9.1-9.2.

To gain a further understanding of what an absolute advantage interpretation of our facts entails, Figure 11 provides a graphical depiction of the AKM model analogous to Figure 9. In this case, workers are ranked on the horizontal axis by absolute advantage as opposed to comparative advantage (i.e., q denotes absolute advantage). Workers further to the right in the figure have higher absolute advantage and are therefore better at both tasks. This is reflected in the fact that both the $\bar{Y}_F(q)$ curve and the $\bar{Y}_N(q)$ curve are upward sloping. Since there is no comparative advantage, these two curves are parallel. We have drawn the figure such that the causal effect of moving is positive ($\bar{Y}_N(q) > \bar{Y}_F(q)$). We have also drawn a third curve in the figure representing the average earning in non-fishing of those living in other regions at the time of the eruption (the green dotted curve). This curve is below the $\bar{Y}_N(q)$ curve reflecting the positive worker effects of the Westman Islanders relative to people elsewhere in Iceland needed to explain lower average income in the rest of Iceland than in the Westman Islands. Finally, in this model, it is not heterogeneity in the causal effect of moving that determines who moves (since this is constant). A simple idea is that there is heterogeneity in moving costs. The final curve in the Figure 11 plots earnings of Westman Islanders in non-fishing net of a heterogeneous moving cost ($\bar{Y}_N(q) - m(q)$). We have drawn this curve such that the moving cost is smaller for people with low absolute advantage. In this case, it will be low absolute advantage people that move. This assumption is needed for the AKM interpretation to be able to explain the low OLS estimate of income on moving we obtain. However, recent empirical evidence suggests that, in fact, low-skilled people are less mobile than high-skilled people (Notowidigdo, 2013).

9.6 Compensating Differentials

Are the greater earnings obtained by those who move away from the Westman Islands compensation for non-pecuniary costs? This is an issue that besets most work on the causal effect of location, but which we believe is relatively unimportant in our setting. Conventional wisdom in Iceland is that the price level in rural towns like the Westman Islands has traditionally been higher than in Reykjavik (except perhaps for housing). We do not have access to a systematic comparison of price levels in the Westman Islands and other areas in Iceland. But we have been able to survey certain product categories to partially verify this conventional wisdom at least for the present time.

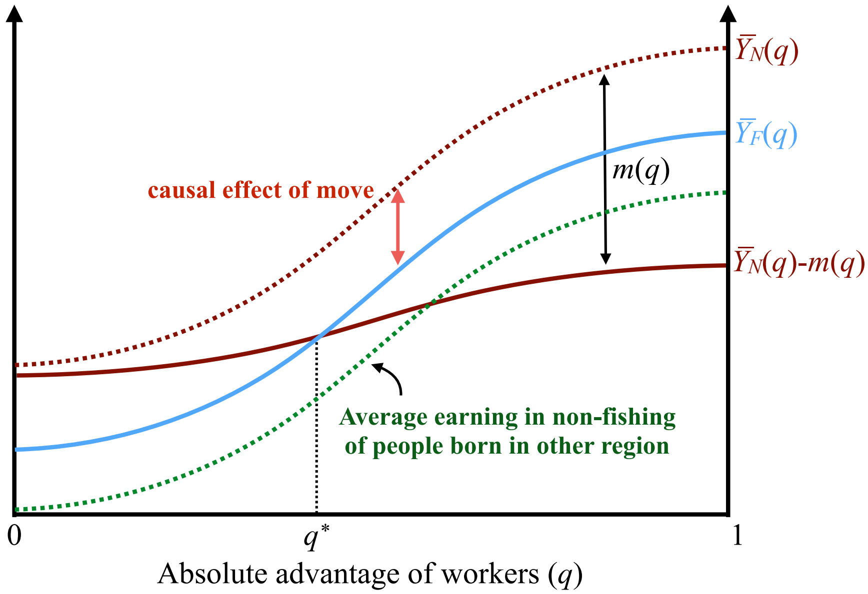


Figure 11: A Model of Absolute Advantage

The Westman Islands has two main supermarkets, and we have verified that currently the price of food in these stores is identical to other outlets of the same chains in Iceland. Product availability is clearly much more limited in the Westman Islands, suggesting that the variety-adjusted price index is higher. The price of gasoline is also the same in the Westman Islands as in rest of Iceland, but the price of electricity and hot water for heating are higher in the Westman Islands than in Reykjavik. Housing has been less expensive per square foot in the Westman Islands than in the Reykjavik in recent years. However, it is difficult to adjust for quality and this difference is presumably associated with greater amenities in Reykjavik.

Since the price level is likely higher in the Westman Islands than in Reykjavik, any non-pecuniary benefits of living in the Westman Islands must arise from other sources. One such source may be differences in preferences (Atkin, 2013). The people living in the Westman Islands may simply have a preference for the particular amenities that exist there. However, this interpretation seems difficult to square with the time pattern of earnings effects which appear to grow across generations. The average earnings effect for the cohorts that were 25 years old and older at the time of the eruption is -$4,000, while it is $27,500 for those younger than 25 years old, and $31,000 for the unborn children of those younger than 25 years old (estimated with large standard errors). Similarly, the education effect also seems to grow across the generations, with the effect being largest for the generation that was unborn at the time of the eruption.

If compensating differentials associated with culture were behind our effects, one would expect them to be smaller for children than parents, and even smaller for descendants born outside of the Westman Islands. Therefore, for compensating differentials to explain our findings, the intergenerational pattern of effect sizes should be the reverse of what we find. Another way to put this is that if the non-pecuniary benefits of living in the Westman Islands were similar for the parents as for the children, then the causal effect estimates for the children would require large moving costs to explain.

We are also able to study the effect of our shock to mobility on a variety of non-monetary outcomes. Table 11 reports results fo a variety of outcomes, aside from earnings, for those less than 25 years of age at the time of the eruption. The causal effect of moving on these outcomes are imprecisely estimated. But the point estimates suggest that those induced to move by the eruption are both less likely to die before the age of 50 and less likely to receive pension payments. Since the young cohorts do not reach the retirement age of 67 during the sample period, pension payments relate to illness, disability, or a deceased spouse or parent.[21] The point estimates also suggest that those induced to move are more likely to get married and have more children. Effects for the older cohorts are qualitatively similar, though they are smaller and apart from being less likely to die before the age of 50, none of the coefficients are statistically significant (see Table A.3). None of these estimates are consistent with non-pecuniary costs of moving, according to conventional views on the consequences of these factors for happiness.

9.7 Returns to Education?

Finally, let us consider how our estimates relate to the literature on the returns to education. Empirical work on the returns to education suggests that an additional year of schooling raises income by roughly 10% (Card, 2001). This corresponds approximately to what one obtains by comparing average incomes across educational groups in Iceland. During the period 2004-2014, the annual earnings premium for workers with junior college degrees in Iceland versus those with only compulsory education was 36%. This suggests a 9% return per additional year of schooling in Iceland (36% / 4 years).

We can compare this with what we would estimate for the returns to education if we were

[21]One might wonder whether the treatment effect on income is, to some extent, driven by the lower propensity of the treatment group to retire early. To investigate this, we reran our empirical analysis setting the earnings observations to missing for all years when individuals are receiving a pension. Table A.4 in the appendix presents results from this case. This approach yields a treatment effect of $24,300, and is highly statistically significant (compared to $27,500 for our baseline specification). Hence, early pensions do not appear to be driving our main results.

Table 11: Other Outcomes – Cohorts Younger than 25 at Time of Eruption

	IV	OLS	Control Mean
	(1)	(2)	(3)
Pension Recipient	-0.087	0.000	0.084
	(0.058)	(0.006)	
Early Death	-0.057	-0.010*	0.033
	(0.040)	(0.006)	
Married	0.171	-0.038**	0.628
	(0.141)	(0.016)	
Number of Children	0.089	-0.100*	2.30
	(0.435)	(0.055)	

Notes: Each coefficient estimate corresponds to a regression of the dependent variable indicated in the top panel on $Moved$. We control for gender, cohort, a dummy for having changed houses after 1960, a dummy for being born in the Westman Islands, year dummies, and age dummies. *Pension Recipient* is a dummy for receiving pension income in a given year. *Early Death* is a dummy for dying before age 50. The regression with *Early Death* as the dependent variable is estimated only for those born before 1965, since this group has reached age 50 by the end of our sample period. *Married* is an indicator of being registered as married in the National Registry. *Number of Children* is number of children born after the eruption, i.e., in 1973 or later. Robust standard errors clustered by address are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

to assume (counterfactually, we think) that the only channel by which moving affects earnings is through educational attainment. Our average estimated earnings effect is 83%, and our average estimated effect on educational attainment is a 3.6 year increase in schooling. Taken together, these estimates would imply a 23% return (0.83/3.6) to each additional year of schooling—much larger than the 10% return suggested by the returns to education literature.

The model we present in sections 9.1-9.2 can help us understand this difference. In this model, the large ratio of causal effects of moving on earning and education arise from several factors. First, it partly reflects comparative advantage: those induced to move have a comparative advantage at non-fishing. Second, it arises from an interaction between moving and increased educational attainment. Additional years of schooling are much more valuable when the individuals can relocate to where the education is most valuable. The returns to additional years of education are smaller in the more standard case where the individual still faces a large moving cost and is therefore only able to use his or her additional education in his or her original location.

10 Conclusion

We exploit a mobility shock generated by a destructive volcanic eruption—a true natural experiment—to estimate the causal effect of location on economic and educational outcomes. For those who were younger than 25 years old at the time of the eruption, we find that having one's house destroyed by the eruption yields a large *positive* causal effect on both earnings and education. The "lava shock" led to an increase in annual earnings of roughly 83% for those younger than 25 years old at the time of the eruption who were induced to move. The earnings effect increased gradually over people's working life and peaked during prime age. Moreover, these young movers got 3.6 more years of schooling than they otherwise would have, and, as a result of the mobility shock, their children (the descendants of the originally affected population) got 4.7 more years of schooling.

Our study shows that the benefits of moving may be very unequally distributed within the family. While the eruption had large positive effects on the earnings of the young, the earnings effects for those 25 and older at the time of the eruption were small and negative. The unequal distribution of the costs and benefits of moving across parents and children may help shed light on why labor does not always flow to where it earns the highest returns: the costs accrue to the parents (who make the decision), while the gains accrue to children, potentially many decades later.

A unique feature of our environment, moreover, is that the workers in our study are moving *away* from opportunity, at least from the perspective of average income. This suggests that our results should not be interpreted as the return from escaping a "bad" location. Instead, we interpret our results as evidence of the importance of comparative advantage. The location we study is, like many small towns, specialized in a particular industry that is unlikely to be the ideal match for everyone. Those who responded to the "lava shock" were more likely to come from highly educated families, who were plausibly poorly matched with the range of job opportunities in this location. Our findings underscore the potential for geographical misallocation of labor even when differences in average incomes across locations are small.

A Constructing Years of Schooling

Our education variable is reported on a five-point scale using the International Standard Classification of Education (ISCED). The first level is compulsory schooling, which is 10 years in Iceland and is completed by most students when they are 16 years old. The second level is a degree from a junior college. In junior college, students can choose between traditional tracks that prepare students for university studies and vocational tracks such as carpentry, hair-dressing, plumbing, etc. Junior college degrees take four years to complete and are completed by most students when they are 20 years old. We therefore convert the second level to 14 years of schooling. The third level is post-secondary, non-tertiary degrees. These include various technical degree programs that in most cases take 6 months to 2 years to complete. We convert this level to 15 years of schooling. The fourth level is university education, both bachelor's and master's degrees. Most bachelor's degrees take three years to complete in Iceland and most masters degree take one to two years to complete. We convert this level to 18 years of schooling, i.e., four additional years over and above junior college. Finally, the fifth level is doctoral degrees. We assume that these take four years to complete after a completion of a bachelor's degree and a one year master's degree. We therefore convert these degrees to 22 years of schooling.

B Earnings Effect over Subsamples

One might worry that the large causal effect of moving we estimate is concentrated in the period of the financial boom Iceland experienced over the period 2002 to 2008. This is not the case. To illustrate this we estimate the following regression

$$Y_{it} = \alpha + \sum_{t=1981}^{2014} \beta_t Moved_i \times period_t + \mathbf{X}_i'\gamma + \delta_t + \varepsilon_{it}, \tag{9}$$

where the variable $period_t$ represents an indicator variable for each non-consecutive 5-year period in sample period of 1981-2014 (i.e., 1981-1985, 1986-1990, ... 2011-2014). The endogenous regressors $Moved_i \times period_t$ are instrumented using interactions of the 5-year period dummies with the instrument $Destroyed_i$. The β_t estimates from this regression are plotted in Figure A.3. The figure shows that the effect of moving is positive throughout the sample period and does not appear to have a systematic relationship with the business cycle. In particular, it is high both before and after the financial crisis.

C Earnings Effects over the Life-Cycle

We can estimate the life-cycle profile of the effect of living in a house that was destroyed on earnings by estimating the following regression

$$Y_{it} = \alpha + \sum_{\tau=18}^{62} \beta_\tau Destroyed_i \times age_\tau + \mathbf{X}_i'\gamma + \delta_t + \varepsilon_{it} \tag{10}$$

where the variable age_τ represents an indicator variable for each 2-year age group from age 18 to 63 (i.e., 18-19, 20-21, ..., 62-63). We include a full set of 2-year age fixed effect, time fixed effects and the same demographic controls as in our main specifications. Panel A of Figure A.2 plots the β_τ coefficients from this specification. These results are slightly different from what one might expect from Figure 4. The difference arises because of the inclusion of the controls.

We can also estimate the life-cycle profile of the causal effect of moving by age by using an instrumental variables procedure where we estimate

$$Y_{it} = \alpha + \sum_{\tau=18}^{62} \beta_\tau Moved_i \times age_\tau + \mathbf{X}_i'\gamma + \delta_t + \varepsilon_{it} \tag{11}$$

and instrument for the endogenous regressors $Moved_i \times age_\tau$ with $Destroyed_i \times age_\tau$. Panel B of Figure A.2 plots the β_τ coefficients from this specification.

D Results for Household Heads versus Dependents

Our baseline empirical results split the sample into those younger than 25 and 25 and older. Our theoretical model and interpretation of our results, however, discusses the difference between the choice faced by children and their parents at the time of the eruption. In this section, we present a set of empirical results that matches more closely the distinction between parents and children.

Rather than splitting the sample by age, we split it into a group we call "household heads" and another that we call "dependents." We classify people into the household heads group if they are: 1) married, "cohabiting" (a legal construct for unmarried couples in Iceland), divorced, or widowed; or 2) the oldest male or female in the household and are older than 25 years old; or 3) between 18 and 25 years old, the oldest male or female, and living with someone older than 25 but less than 15 years older than they are. All others are classified as dependents.

Table D.1 provides basic information about how this grouping compares to the age-based grouping we use in the main body of the paper. The difference is that 217 individuals under

Table D.1: Comparison of Groups

	Younger than 25	25 and Older	
Dependents	2,392	0	2,392
Household Heads	217	2,198	2,415
	2,609	2,198	4,807

Note: The table reports number of individuals by group according to two sample splits: Younger than 25 versus 25 and Older, used in main text, and Dependents versus Household Heads used in this appendix.

the age of 25 are in the household head group. This shows that our simple age based grouping captures the distinction between independent adults and dependents quite well.

Tables D.2-D.5 present our main results for the household head versus dependent grouping. The results are very similar to the results for the age-based grouping in the main body of the paper. We find large positive IV estimates of the effect on both income and education for the dependents. We find zero effects for the household heads on earnings and statistically insignificant positive effects on education. The OLS estimates are also very similar.

The results show directly that it is the dependents (mostly children) that benefit from being induced to move by having their house destroyed, while the parents (household heads) do not gain. The only difference between our grouping and parents versus dependents is that we include childless adults in the household heads category. The reason for this is that we do not think having children will affect the earnings effect of moving for adults. It may affect the decision to move (the first stage). But it should not affect the second stage. We therefore include these childless adults in our head of household regressions.

Table D.2: First Stage Regressions

	All		Dependents		Household Heads		Descendants	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Destroyed$	0.151***	0.160***	0.107***	0.120***	0.195***	0.201***	0.058***	0.059***
	(0.030)	(0.029)	(0.035)	(0.034)	(0.029)	(0.029)	(0.017)	(0.017)
Control Mean	0.269	0.269	0.284	0.284	0.250	0.250	0.621	0.621
Controls	No	Yes	No	Yes	No	Yes	No	Yes
F-statistic	17.9	21.1	8.9	11.4	27.5	29.4	10.4	12.3
N	4,807	4,807	2,392	2,392	2,415	2,415	3,740	3,740

Notes: This table reports coefficients from OLS regressions of $Moved$ on $Destroyed$. For the original inhabitants $Moved$ is an indicator for having moved away as of 1975 and $Destroyed$ is an indicator for living in a house that was destroyed by the eruption. For descendants, $Moved$ is an indicator for living outside the Westman Islands when first observed in the administrative records, while the definition of $Destroyed$ is more involved and is described in section 4. The set of controls includes gender, age, a dummy for having changed houses after 1960, and a dummy for being born in the Westman Islands. Robust standard errors clustered by address are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1

Table D.3: Effect on Earnings – Dependents

	Reduced Form		Wald	2SLS	OLS	
	(1)	(2)	(3)	(4)	(5)	(6)
$Moved$			25,366	28,349**	-2,134*	-1,813*
			(16,905)	(14,425)	(1,224)	(1,099)
$Destroyed$	2,705*	3,314**				
	(1,540)	(1,306)				
Control group mean	34,073	34,073	34,073	34,073	—	—
Controls	No	Yes	No	Yes	No	Yes
Age fixed effects	No	Yes	No	Yes	No	Yes
Year fixed effects	No	Yes	No	Yes	No	Yes
Observations	61,532	61,532	61,532	61,532	61,532	61,532

Notes: The dependent variable in all cases is labor earnings. Coefficient estimates are reported in US dollars as of 2014 (125 ISK = 1 USD). The set of controls includes gender, a dummy for having changed houses after 1960, and a dummy for being born in the Westman Islands. Robust standard errors clustered by address are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table D.4: Effect on Earnings – Household Heads

	Reduced Form		Wald	2SLS	OLS	
	(1)	(2)	(3)	(4)	(5)	(6)
$Moved$			76	835	-3,885***	-3,214***
			(5,730)	(5,769)	(941)	(906)
$Destroyed$	14	153				
	(1,066)	(1,057)				
Control group mean	27,930	27,930	27,930	27,930	—	—
Controls	No	Yes	No	Yes	No	Yes
Age fixed effects	No	Yes	No	Yes	No	Yes
Year fixed effects	No	Yes	No	Yes	No	Yes
Observations	37,868	37,868	37,868	37,868	37,868	37,868

Notes: The dependent variable in all cases is labor earnings. Coefficient estimates are reported in US dollars as of 2014 (125 ISK = 1 USD). The set of controls includes gender, a dummy for having changed houses after 1960, and a dummy for being born in the Westman Islands. Robust standard errors clustered by address are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table D.5: Effect of Moving on Years of Schooling

	Dependents		Household Heads		Descendants	
	IV	OLS	IV	OLS	IV	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
$Moved$	3.56*	0.14	1.16	0.11	5.69**	-0.24**
	(1.86)	(0.17)	(0.77)	(0.15)	(2.49)	(0.11)
Control group mean	13.52	13.52	11.99	11.94	12.71	12.71
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	2,071	2,071	1,292	1,292	3,207	3,207

Notes: The dependent variable is years of schooling for the group listed at the top of each column. Robust standard errors clustered by address are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

E Spatial Correlation

The standard errors in our main analysis are clustered at the address level. This allows for correlation across individuals that lived at the same address at the time of the eruption (in most cases members of the same family). A reasonable concern with our results is that there might be more widespread spatial correlation. For confidentiality reasons, we do not have information about the exact address of the individuals in our sample. Since the Westman Islands is a small place, it is coded as a single geographic unit in our tax data (which identifies location by postal code). Unfortunately, this precludes us from studying spatial correlation in our main outcome variables.

However, since we constructed the house price data we use ourselves by digitizing administrative records, we have the exact address of each house in our sample. We can, therefore, study spatial correlations in house prices prior to the eruption. To do this, we have manually geocoded the location of every house in our dataset. This process was somewhat involved because many of the residential streets in question were subsequently covered with lava and no longer exist. We used a combination of web-based map viewers from the National Land Survey of Iceland and street maps of the Westman Islands pre-eruption to locate houses and to construct a geocoded location for each house.

Using these data we have calculated two measures of spatial correlation of house prices. First, we have calculated Geary's C:

$$C = \frac{N-1}{2W} \frac{\sum_i \sum_j w_{ij}(x_i - x_j)^2}{\sum_i (x_i - \bar{x})^2},$$

where x_i denotes the price of house i, the weight w_{ij} is the inverse distance between house i and j, and W is the sum of all weights w_{ij}. If the price of neighboring houses tends to be positively correlated, this will lead to values of Geary's C that are significantly lower than 1 (negative spatial correlation will lead to values significantly higher than one). A value of one indicates no spatial correlation. For our sample, the value of Geary's C is estimated to be 0.974, which is very closer to 1. We cannot reject the null hypothesis of no spatial correlation (the P-value is 0.128).

The second measure of spatial correlation that we have calculated is Moran's I:

$$I = \frac{N}{W} \frac{\sum_i \sum_j w_{ij}(x_i - \bar{x})(x_j - \bar{x})}{\sum_i (x_i - \bar{x})^2}.$$

Moran's I is analogous to an autocorrelation coefficient, but measures correlations over space (in two dimensions) rather than over time. If adjacent houses tend systematically to have more similar house prices than houses that are further away from each other, this will tend to raise the value of

Moran's I. Values of Moran's I close to 1 suggest strong positive spatial correlation, while values close to -1 suggest strong negative spatial correlation. Moran's I is more sensitive to "global" spatial correlation than Geary's C, since the building blocks involve differences versus the overall mean, as opposed to immediately surrounding houses.

Our estimate of Moran's I is 0.02. This value indicates statistically significantly spatial correlation. However, the economic magnitude of this spatial correlation is extremely small. The test statistic implies that a 1% increase in a given house price is associated with a 0.02% increase in the house prices of its neighbors.

To aid interpretation of Moran's I, Figure E.1 plots a "Moran's I scatter plot." This figure plots the price of each house (on the x-axis) against its "spatial lag." The spatial lag is a "synthetic neighbor," defined as the weighted average of the value of all other houses in the town, weighted by the inverse of their geographic proximity. Hence, closer houses are given higher weights than those that are further away. A positive relationship in Figure E.1 indicates positive spatial correlation. It is clear from the figure that any positive spatial correlation in our house price data is very modest. Moreover, the figure above distinguishes between houses in the destroyed (orange) and non-destroyed (blue) regions. There is no systematic difference in the house prices along this margin, consistent with our balance tests.

Spatial correlation may imply that there are fewer "effective observations" than actual observations in our dataset, which could be biasing downward our standard errors. We can quantify this concern using Moran's I as an indicator of how spatially correlated the observations are likely to be (with the caveat that these spatial correlations apply to house prices, not income or education). To do this, we draw on the literature studying the relationship between Moran's I and the "effective number of observations." Griffith and Zhang (1999) report Monte Carlo calculations that relate Moran's I to the spatial autocorrelation coefficient in a first order spatial autocorrelation model, and then relate the spatial autocorrelation coefficient to an approximate effective sample size. A value of Moran's I of 0.02 implies a spatial autocorrelation of roughly the same numerical value, which implies only a tiny adjustment to the effective sample size (see Figure 3 in their paper). For this reason, we have not pursued further adjustments to our standard errors for spatial correlation. To the extent that spatial correlation of income and education is of a similar order of magnitude to house prices, we expect the required spatial adjustment of our standard errors to be very small.

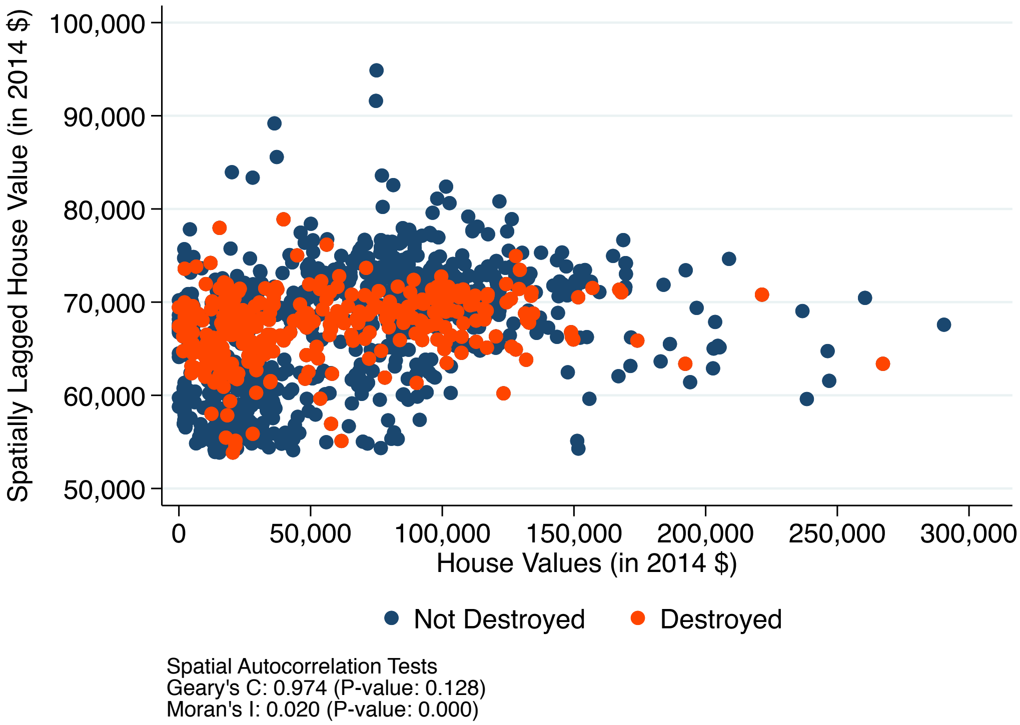


Figure E.1: Moran's I scatter plot

F Uncertain Gains from Education (and Comparative Advantage)

Consider an extension of the model presented in section 9.1 where the gains from education are uncertain and households have Epstein and Zin (1989) preferences. Specifically, assume that the gains from education in the non-fishing sector are stochastic and distributed

$$\phi(i) \sim \mathrm{N}(\bar{\phi}(i) - \sigma_\phi^2/2, \sigma_\phi^2)$$

and the utility function of the parents is

$$\log(C^p(i)) + \beta \log([\mathbb{E}(C^c(i))^{1-\gamma}]^{1/(1-\gamma)}).$$

where γ measures risk aversion and the elasticity of intertemporal substitution (substitution between own consumption and the consumption of the children in this case) is one. We introduce the shorthand notation $U_k^p(q)$ to represent $\log(C^p(i))$ for households of quantile q that are working in sector k and, analogously, $U_k^c(q)$ to represent $\log([\mathbb{E}(C^c(i))^{1-\gamma}]^{1/(1-\gamma)})$ for households of quantile q that are working in sector k.

In this case, we have that

$$U_N^p(q) = w_N + A(q) + \alpha(q), \tag{12a}$$

$$U_F^p(q) = w_F + A(q), \tag{12b}$$

$$U_N^c(q) = w_N + \rho_a A(q) - \frac{\gamma}{2}\sigma_a^2 + \rho_s \alpha(q) - \frac{\gamma}{2}\sigma_s^2 + \bar{\phi}(q) - \frac{\gamma}{2}\sigma_\phi^2, \tag{12c}$$

$$U_F^c(q) = w_F + \rho_a A(q) - \frac{\gamma}{2}\sigma_a^2. \tag{12d}$$

The right-hand sides of these expressions differ from those in equations (3a)-(3d) due to the variance terms $\frac{\gamma}{2}\sigma_a^2$, $\frac{\gamma}{2}\sigma_s^2$, and $\frac{\gamma}{2}\sigma_\phi^2$. Here σ_a^2 and σ_s^2 are the variances of the intergenerational shocks to absolute and comparative advantage, respectively, i.e., the variances of $\epsilon^a(i)$ and $\epsilon^s(i)$. In our earlier model, the three variance terms did not appear because of two simplifying assumptions: log-utility and non-stochastic education. Analogous algebra to that in section 9.2 yields an equation for the cutoff points for moving $q^{*\prime}$ and q^* that can be written

$$U_N^p(q) + \Psi(m(i), q) = U_F(q), \tag{13}$$

where

$$\Psi(m(i), q) = \frac{1}{1+\beta}\left[\beta\left(\bar{\phi}(q) - \frac{\gamma}{2}\sigma_\phi^2\right) - m(i) - (1+\beta)f - \beta(1-\rho_s)\alpha(q) - \frac{\gamma}{2}\sigma_s^2\right]. \tag{14}$$

Relative to the expression for $\Psi(m(i), q)$ in our baseline model, there are two additional terms $-\frac{\gamma}{2}\sigma_s^2$ and $-\frac{\gamma}{2}\sigma_\phi^2$. In this model, risk is a source of "moving costs" in the sense that it makes people more reluctant to move for a given expected return to moving.

Table A.1: Effects on the Logarithm Earnings – Cohorts Younger than 25 at Time of Eruption

	Reduced Form		IV		OLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Moved			0.812*	0.866***	-0.060	-0.031
			(0.484)	(0.421)	(0.046)	(0.043)
Destroyed	0.094*	0.110**				
	(0.048)	(0.044)				
Controls	No	Yes	No	Yes	No	Yes
Observations	2,570	2,570	2,570	2,570	2,570	2,570

Notes: The dependent variable in all cases is the natural logarithm of life-time labor earnings. The set of controls includes gender, a dummy for having changed houses after 1960, and a dummy for being born in the Westman Islands. Robust standard errors clustered by address are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.2: Effects of Moving on Earnings – Descendants

	Reduced Form		IV		OLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Moved			29,070	27,672	-7,038***	-5,708***
			(25,205)	(23,119)	(1,262)	(1,156)
Destroyed	1,833	1,798				
	(1,355)	(1,210)				
Control group mean	31,681	31,681	31,681	31,681	—	—
Controls	No	Yes	No	Yes	No	Yes
Age fixed effects	No	Yes	No	Yes	No	Yes
Year fixed effects	No	Yes	No	Yes	No	Yes
Observations	20,192	20,192	20,192	20,192	20,192	20,192

Notes: We control for gender. Robust standard errors clustered by individual are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.3: Other Outcomes – Cohorts 25 and Older at Time of Eruption

	IV	OLS	Control Mean
	(1)	(2)	(3)
Pension Recipient	0.003	-0.020**	0.40
	(0.049)	(0.010)	
Early Death	-0.024*	0.000	0.008
	(0.013)	(0.002)	
Married	0.109	0.009	0.700
	(0.103)	(0.021)	
Number of Children	0.131	-0.167**	1.08
	(0.301)	(0.059)	
Earnings > 0	0.011	-0.022**	0.622
	(0.050)	(0.011)	

Notes: Each coefficient estimate corresponds to a regression of the dependent variable indicated in the top panel on $Moved$. Controls include gender, cohort, a dummy for having changed houses after 1960, a dummy for being born in the Westman Islands, year dummies, and age dummies. *Pension Recipient* is a dummy for receiving pension income in a given year. *Early Death* is a dummy for dying before age 50. The regression with *Early Death* as the dependent variable is estimated only for those born before 1965, since this group has reached age 50 by the end of our sample period. *Married* is an indicator of being registered as married in the National Registry. *Number of Children* is number of children born after the eruption, i.e., in 1973 or later. Robust standard errors clustered by address are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.4: Effect of Pension on Earnings Estimates – Cohorts Younger than 25 at Time of Eruption

	Reduced Form		Wald	2SLS	OLS	
	(1)	(2)	(3)	(4)	(5)	(6)
$Moved$			22,535	24,298**	-2,528**	-1,879*
			(14,645)	(12,256)	(1,131)	(1,015)
$Destroyed$	2,561*	2,997**				
	(1,445)	(1,227)				
Control group mean	34,297	34,297	34,297	34,297	—	—
Controls	No	Yes	No	Yes	No	Yes
Age fixed effects	No	Yes	No	Yes	No	Yes
Year fixed effects	No	Yes	No	Yes	No	Yes
Observations	62,172	62,172	62,172	62,172	62,172	62,172

Notes: The dependent variable in all cases is labor earnings, which is set to missing in all years when individuals receive pension payments. Coefficient estimates are reported in US dollars as of 2014 (125 ISK = 1 USD). The set of controls includes gender, a dummy for having changed houses after 1960, and a dummy for being born in the Westman Islands. Robust standard errors clustered by address are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.5: Payroll Taxes by Industry

	Westman Islands	Capital Region	Other Regions
Fishing and Agriculture	23.2%	1.2%	13.7%
Fish and Food Processing	46.5%	3.4%	15.6%
Construction	2.5%	4.2%	8.5%
Manufacturing	3.7%	6.2%	10.8%
Trade and Transport	5.4%	18.3%	10.7%
Hospitality and Recreation	1.7%	3.6%	5.0%
Information Services	0.3%	6.6%	0.7%
Professional Services	1.0%	8.9%	0.4%
Finance	2.0%	10.7%	2.3%
Government	12.8%	34.4%	26.5%
Other	0.9%	2.4%	4.4%

Notes: Average share of payroll taxes by industry, 2008-2014. *Source*: Directorate of Internal Revenue, Iceland.

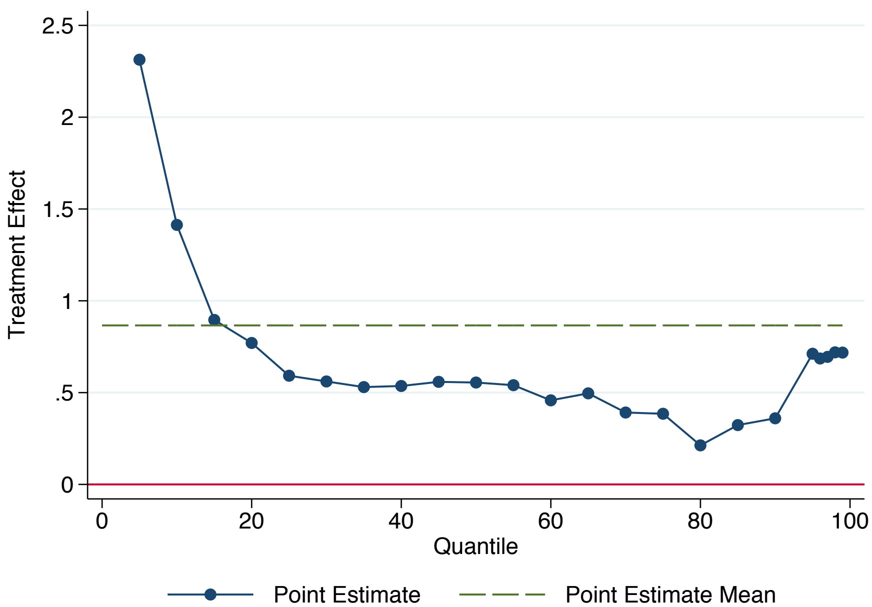
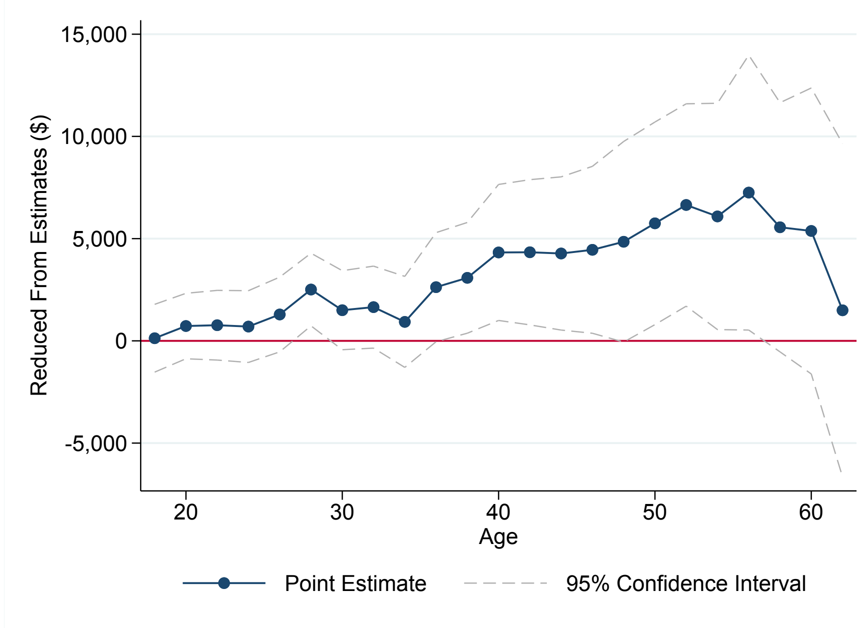
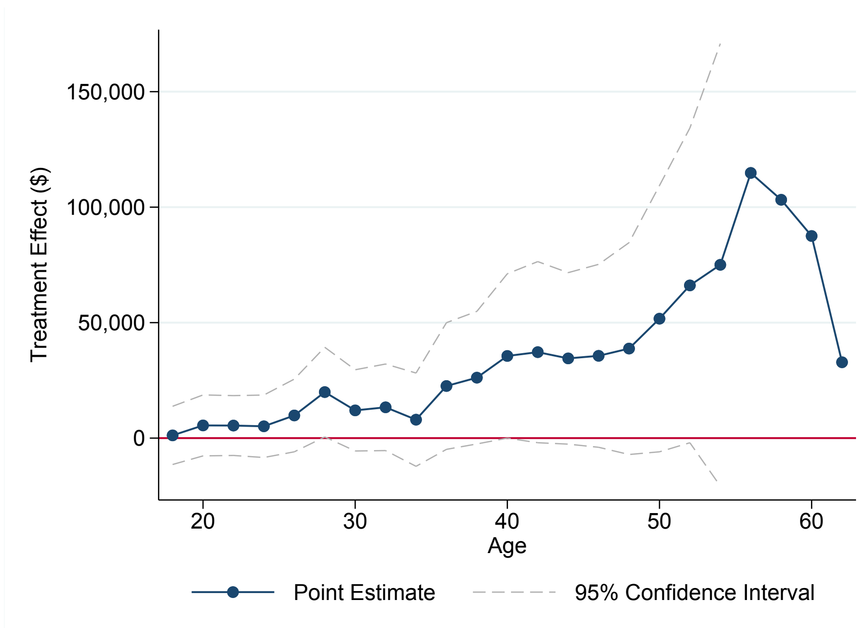


Figure A.1: IV Quantile Effects for Log(Earnings) – Cohorts 25 and Older at time of Eruption

Note: The figure plots quantile treatment effects using the estimator proposed by Abadie, Angrist, and Imbens (2002) for the 5th to the 99th percentile. The effects are estimated in 5 percentile increments up to the 95th percentile, and in 1 percentile increments for 96th to 99th percentile. The green horizontal dashed-line plots the mean effect (2SLS) for comparison.



(a) Reduced Form by Age



(b) Treatment Effect by Age

Figure A.2: Earnings Effect Over the Life Cycle – Cohorts Younger than 25 at time of Eruption

Note: Panel (a) plots the reduced form earnings effect by age. Panel (b) plots the causal effect of moving by age. Robust standard errors are clustered at the house level. To aid visibility in panel (b), we only plot the 95% confidence intervals out to age 56. The confidence intervals for the older age groups are even wider.

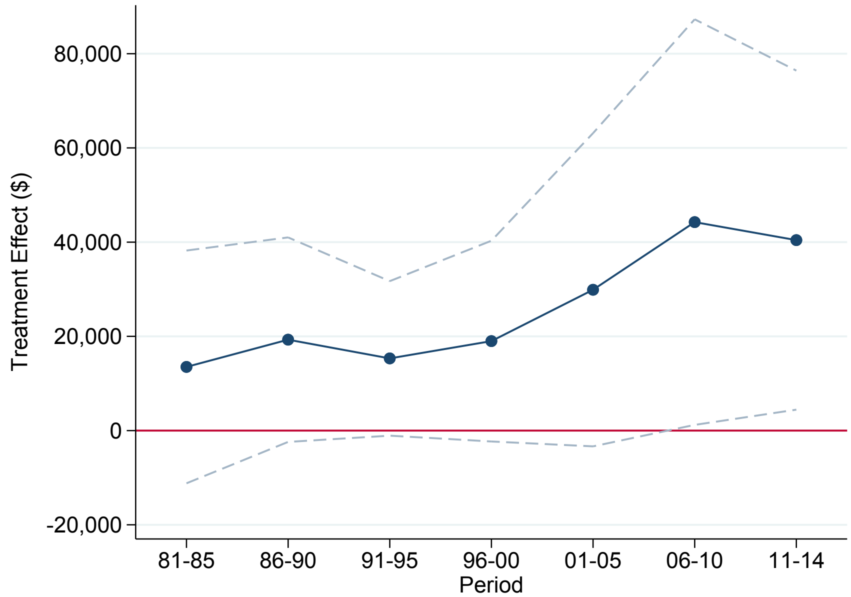


Figure A.3: IV Earnings Effect by Year – Cohorts Younger than 25 at time of Eruption.

Note: The figure displays the evolution of the treatment effect over time. The dashed lines plot the 95-percent confidence interval. Robust standard errors are clustered at the house level.

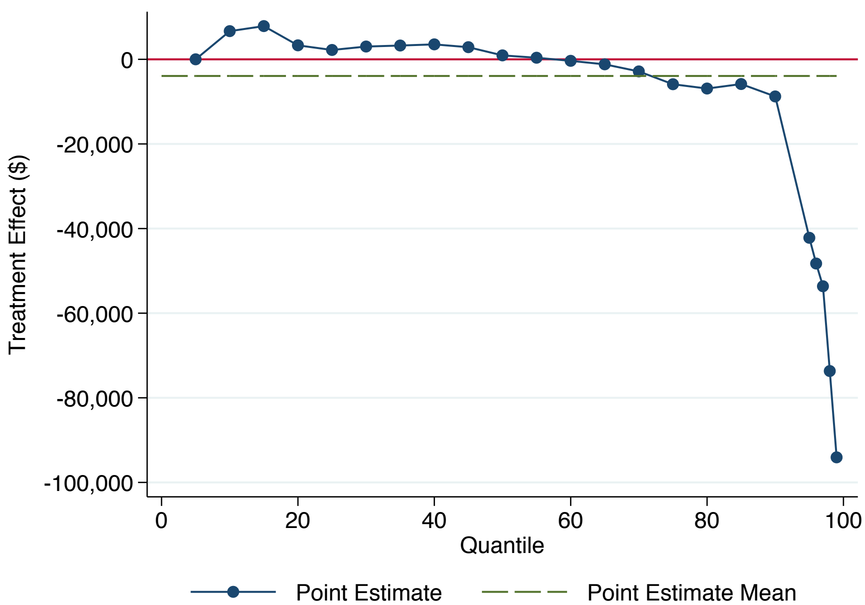
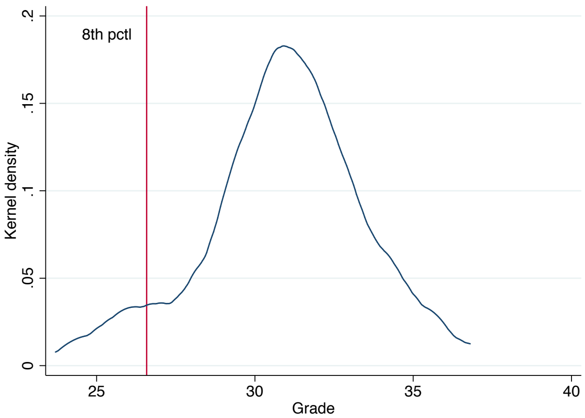
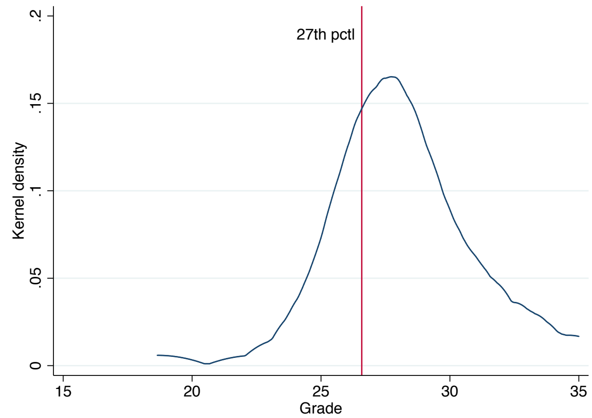


Figure A.4: IV Earnings Quantile Effects – Cohorts 25 and Older at time of Eruption

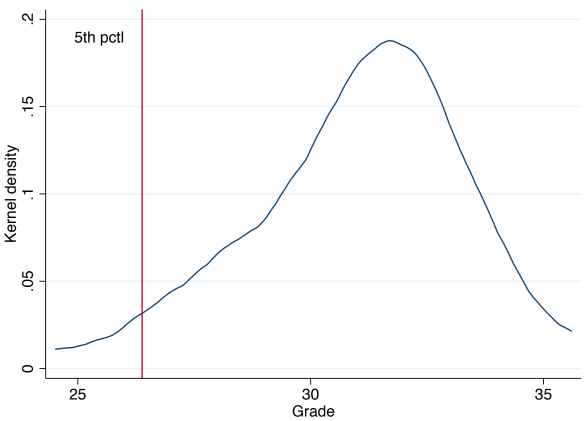
Note: The figure plots quantile treatment effects using the estimator proposed by Abadie, Angrist, and Imbens (2002) for the 5th to the 99th percentile. The effects are estimated in 5 percentile increments up to the 95th percentile, and in 1 percentile increments for 96th to 99th percentile. The green horizontal dashed-line plots the mean effect (2SLS) for comparison.



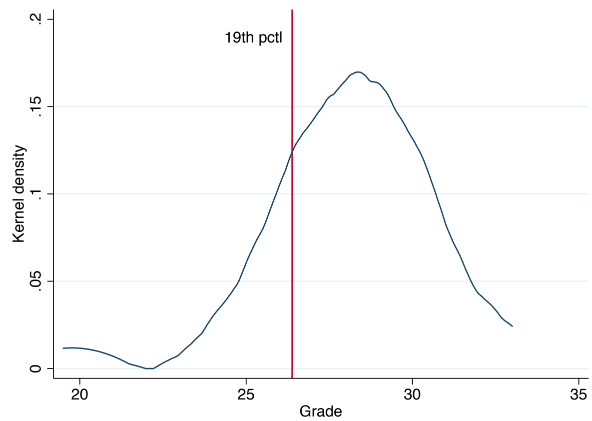
(a) Mathematics – Capital Region



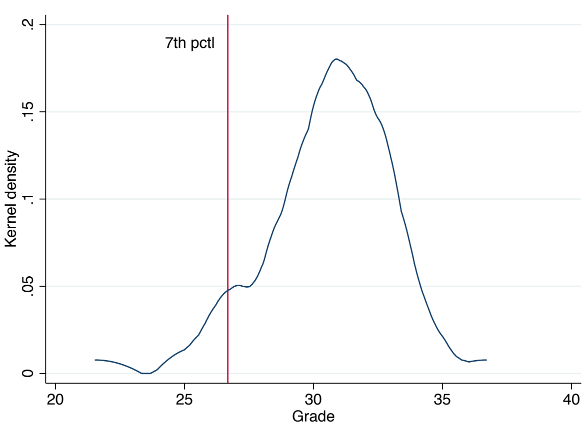
(b) Mathematics – Other Regions



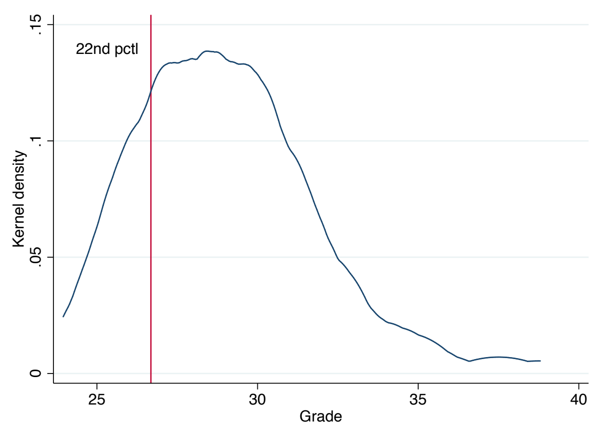
(c) English – Capital Region



(d) English – Other Regions



(e) Icelandic – Capital Region



(f) Icelandic – Other Regions

Figure A.5: Results from Standardized Tests

Notes: Distribution of average grade by school for 2010-2014 on 10th grade standardized tests in Mathematics, English and Icelandic. National average score is 30. The red vertical line represents the average test scores in the Westman Islands in the respective distribution. *Source*: Directorate of Education, Iceland.

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