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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 where 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 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

A large recent literature argues that misallocation of the factors of production across firms leads to large productivity losses (e.g., Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009). Much of this literature has focused on misallocation of capital due to financial frictions. However, misallocation of labor is potentially severe as well. Wages differ enormously across locations. 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 (Kennan and Walker, 2011; Munshi and Rosenzweig, 2016; Bryan and Morten, 2015).

It is, however, very challenging to identify the size of moving costs. 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 systematic differences in workers across locations with high productivity workers sorting into certain locations. Young (2013) argues that the entire urban-rural wage gap in developing countries can be explained by a simple sorting model with no moving costs. Similarly, Lagakos and Waugh (2013) propose a selection-based interpretation for the low agricultural productivity observed in developing countries.

We overcome this identification problem by exploiting a true natural experiment. On January 23, 1973, a volcanic eruption began in the small Westman Islands off the south coast of Iceland. A volcanic fissure opened only 300 yards from the edge of the island's town. The entire population of the island was evacuated within several hours with only a single casualty. The eruption lasted for 5 months and roughly one third of the houses in the town were destroyed by lava.

After the eruption ended, the population of the Westman Islands quickly rebounded to almost its pre-eruption level. However, the families whose houses were destroyed were substantially less likely to return. The Icelandic government set up a disaster relief fund, which compensated these households for the value of their lost house and land. These funds were unrestricted. Some used them to build or buy new houses on the island, but many relocated to other areas of Iceland permanently.

We therefore interpret the event of having one's house destroyed in the eruption as a quasi-random shock to mobility. We use this quasi-random variation to identify the causal effect of moving away from the Westman Islands on lifetime earnings and education for the subgroup of inhabitants who were induced to move by this shock and their descendants. To do this, we draw on the unusually rich administrative data on income, education, and geneological linkages available for the Icelandic population.

We document a remarkable reversal of fortune for those less than 25 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 equal to roughly 80% of the control group's average earnings, and a causal effect on education of 3.6 years of extra schooling. The educational effect is even larger for the "third generation" (the children of these young cohorts who are born after the eruption). The causal effect on education for this group is 4.7 years of extra schooling. These effects are particularly large for the top of the earnings distribution: the earnings effect is roughly 150% of average control group earnings for the 95th percentile of the earning distribution, while it is roughly 60% for the median.

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 then 25 years old at the time of the eruption (mostly children), the earnings effects for older cohorts is 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, these large intergenerational differences in returns to moving coupled with limits to parental altruism or limits to parents' understanding of the potential gains to their children of moving may create an important barrier to mobility.

An important feature of our setting is that the Westman Islands was (and is) a relatively high income town. Our setting is therefore not one were individuals are induced to move away from a poor place to a richer place as is common in the literature (e.g., Chetty, Hendren, and Katz, 2015). It is, perhaps, a particularly surprising feature of our results that we find such large causal effects of moving, even when the place of origin has slightly higher average income than the destination.

We interpret this finding as evidence of 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.[1] While those who moved away from the Westman Islands did not become rabbit hunters (more likely, they became bankers) they did leave

[1] A more mathematical discussion of these ideas is presented in Acemoglu and Autor (2011).

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. While the Westman Islands might be an ideal place for many workers, it was unlikely to be the highest income match for a future computer whiz or a great legal mind. In a setting like this, the "compliers" for which we estimate a causal effect—i.e., those induced to move by the volcanic eruption—will disproportionately be those who are poorly matched to the Westman Islands in terms of comparative advantage and therefore those for which the causal effect of moving is particularly large.

To illustrate this, we build a Roy model with heterogeneous comparative advantage and moving costs (building on recent work by Lagakos and Waugh (2013), Young (2013), and Adao (2015)). We also provide empirical evidence that the compliers in our experiment were more likely to come from highly educated families who may have been less well suited for the limited, though highly paid, set of occupations available in the Westman Islands. Our findings show that in a world of heterogeneous talents, misallocation across locations may be large even if average income differences are small.[2]

Might compensating differentials explain the large effects of moving we estimate? This is an issue that besets most work on the costs of moving, 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 possibly when it comes to housing) and product variety much more limited. Any compensating benefit 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 descendents, 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 which support the compensating differentials

[2] In a similar vein, Bazzi et al (2015) find that comparative advantage has long term effects on productivity of rice farmers in Indonesia. Farmers randomly reallocated to areas with similar agroclimatic conditions exhibit substantially higher productivity than those reallocated to areas with less similar agroclimatic conditions.

interpretation.

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. What our experiment does identify is the difference in the outcomes of these groups. This is informative about moving costs (broadly defined) since barring such costs, the control group could have mimicked the behavior of the treatment group to take advantage of the higher earnings opportunities on Iceland's mainland.

We calculate the difference in the net present value of life-time earnings for an 18 year old who is induced to move to be roughly $440,000. 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 the costs of moving are very large: the typical worker could roughly double his or her income by moving. Bryan and Morten (2015) estimate a structural model of mobility for Indonesia, and find that large moving costs contribute to substantial productivity losses associated with misallocation. Munshi and Rosenzweig (2016) argue that cultural factors contribute to large barriers to moving in rural India, and develop a structural model to estimate the magnitude of these forces.

Our findings reveal that location has a large, persistent causal effect on long-term income and education. This corroborates recent work on the relationship between location and income. Yagan (2016) 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. Bryan, Chowdhury, and Mobarak (2014) show that inducing individuals to move temporarily from rural to urban areas in Bangladesh (by giving them an $8.50 bus ticket) raises household consumption by 30-35%.

Our work also echos recent work illustrating "reversals of fortune" for large-scale displacements. Deryugina, Kawano, and Levitt (2014) and Sacerdote (2012) show that those displaced by Hurricane Katrina had higher long-run income and educational outcomes. It is worth noting, however, that in this case, workers were likely displaced to locations with substantially higher average incomes. Sarvimäki, Uusitalo, and Jäntti (2016) study the long-term impact of forced migration in Finland after World War II. They estimate a positive long-run effect of displacement on earnings of men working in agriculture prior to displacement: they argue the causal effect of switching to non-agricultural sectors was 70-80% of average income.

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 2-300 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 in the figure, while the residential units that survived are colored green. 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

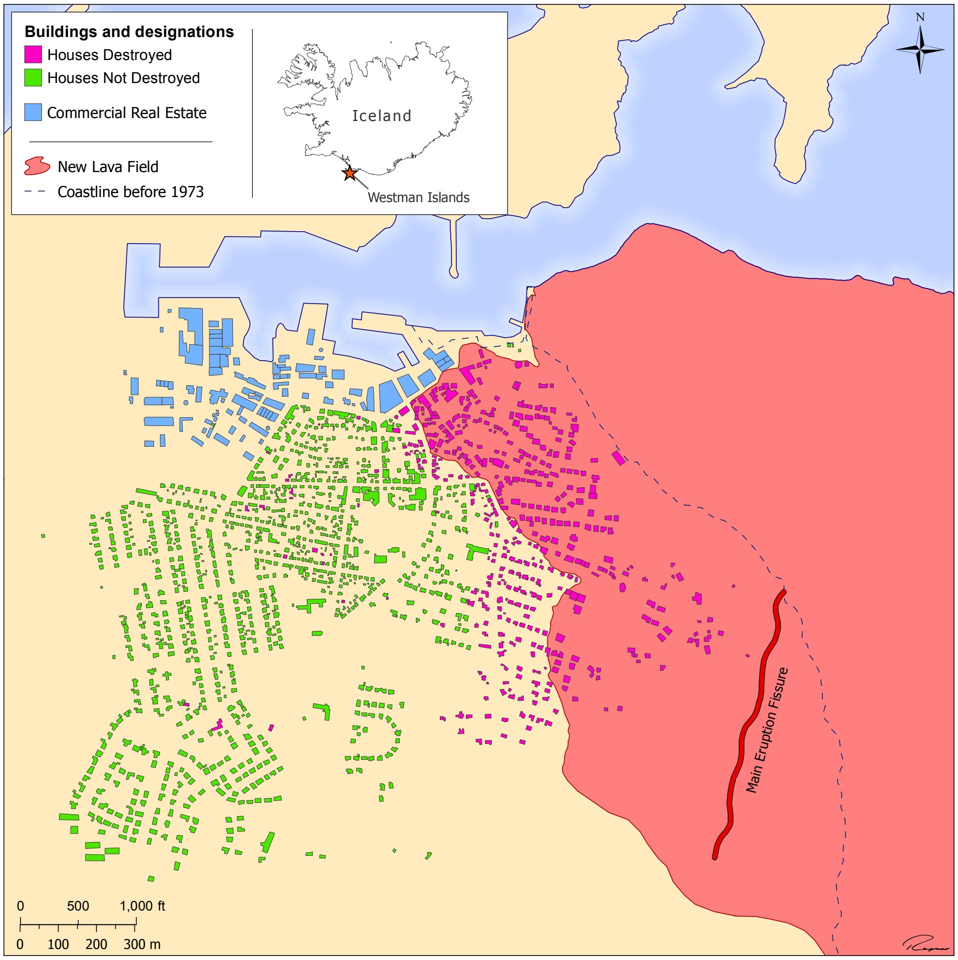


Figure 1: Map of Westman Islands town Post 1973 Eruption

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).

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. 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%

[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.

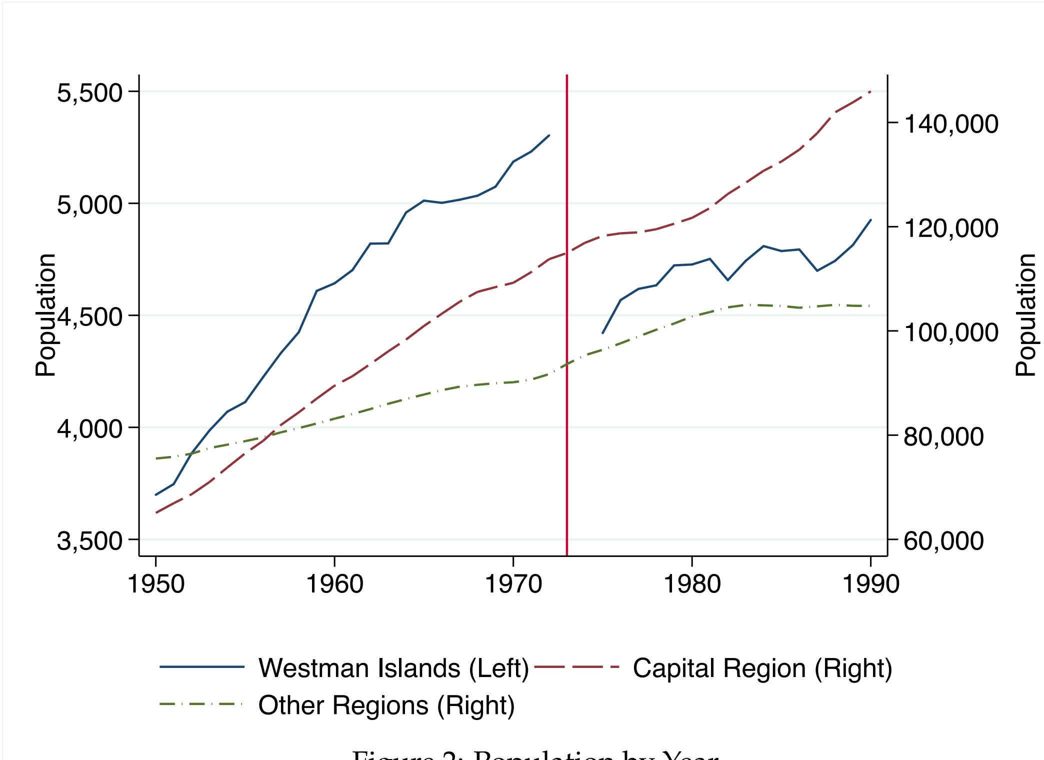


Figure 2: Population by Year

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.

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 were destroyed at the current replacement value of their house. The cash value of houses and land was determined according to annual fire insurance and tax valuations, respectively.[4] 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 contained some error, we believe that they were 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

[4] 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.

could explain the large effects on earning we identify many years later, and the pattern of effects we observe on children versus their parents.

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.[5] 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.

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

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 the earnings data.[6]

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

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

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

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

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

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

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

A recent literature emphasizes the potential heterogeneity of treatment effects across different cohorts of individuals (e.g., Chetty and Hendren, 2015). 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 other formulations for the interaction between age and our mobility shock such as linear exposure effects during childhood.

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 descendants that satisfy two conditions: 1) They are children of those younger than 25 years old at the time of the eruption. 2) They are born after the eruption but before 1997, i.e., they are older than 18 year old in 2014. The main reason for these choices is to avoid including descendants of grown children who had already moved away before the eruption.

For the descendants, the definitions of $Moved_i$ and $Destroyed_i$ are somewhat more subtle, since it is 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

Table 2: Descendant Groups

	Parent's Status ({father, mother})	Size
Treatment	{D, D}, {D, A}, {A, D}	842
Control	{N, N}, {N, A}, {A, N}	2,453
Excluded	{D, N}, {N, D}	277
Total		3,572

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

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). The challenge arises from those that have one parent from a destroyed house and one parent from a non-destroyed house. Should these be members of the treatment group or the control group? We assign these ambiguous members to neither group, i.e., exclude them. The treatment group is then those whose parents' status is one of the following {D,D}, {D,A}, or {A,D}, while the control group is those whose parents' status is one of {N,N}, {N,A}, or {A,N}. Table 2 illustrates this decomposition of descendants into subgroups.

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% level. Living in a house that was destroyed raises the probability of moving by 15% points for the overall population. There is some heterogeneity accross 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 roughly 6 percentage points less likely to live in the Westman Islands when they first appear in our administrative

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***	0.063***	0.063***
	(0.015)	(0.015)	(0.021)	(0.021)	(0.023)	(0.022)	(0.018)	(0.018)
Control Mean	0.269	0.269	0.284	0.284	0.250	0.250	0.680	0.680
Controls	No	Yes	No	Yes	No	Yes	No	Yes
F-statistic	61.3	70.3	28.1	33.9	40.6	43.0	10.7	11.7
N	4,807	4,807	2,609	2,609	2,198	2,198	3,295	3,295

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, dummy for having changed house after 1960, and dummy for being born in the Westman Islands. Robust standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1

records. This difference is statistically significant at the 1% level with a first-stage F-statistic of 11.7.

6 Balance Tests

The Westman Islands is a small and relatively homogenous 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 systemic 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. This data shows 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

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 time of the eruption. *Change house after 1960* is a dummy for having moved houses after 1960. *Missing* is a dummy variable for an individual being missing from the outcome data in 1981. Columns 2 and 4 report results from a covariate balance test. Each row reports a coefficients on regressions of the variable indicated in that row on $Destroyed$, which is an indicator of living in a house that was destroyed by the eruption. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

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). It is hard to know whether this indicates a true difference in the nature of the destroyed neighborhoods or whether this reflects random variation (one out of 20 tests being significant).

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, 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.[7] We have annual earning data for the sample period 1981

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

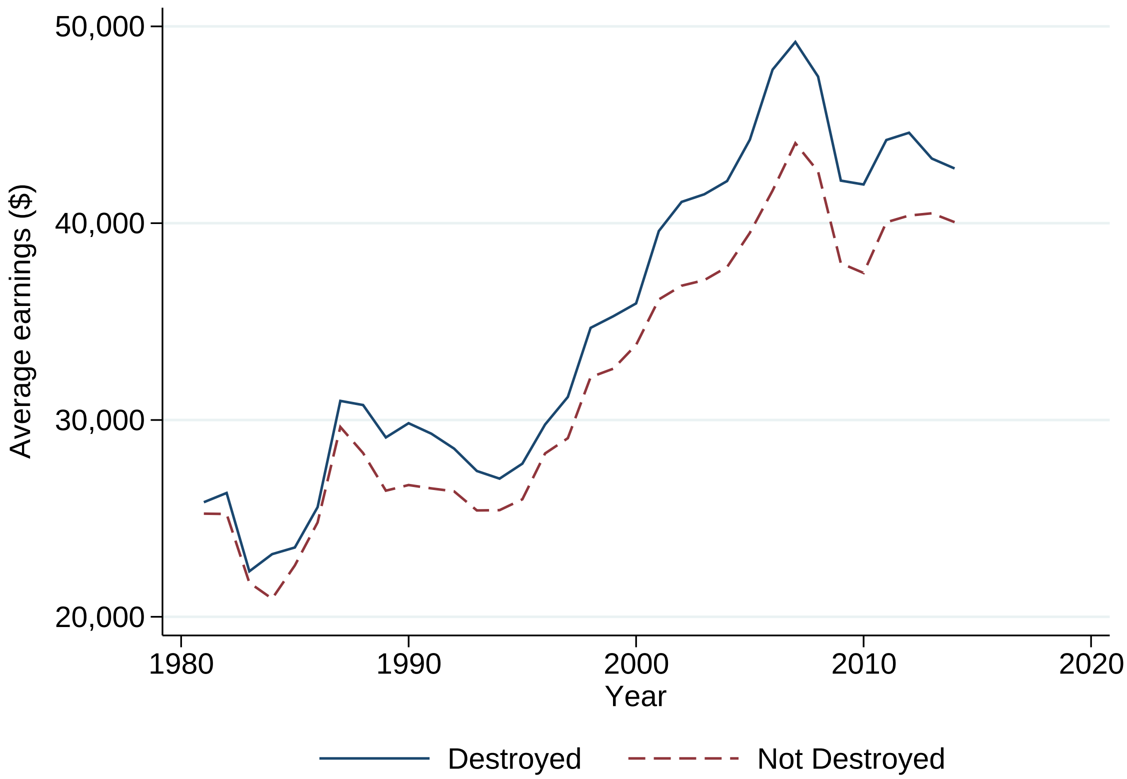


Figure 4: Earnings by Year – Cohorts Younger than 25 at time of Eruption

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 earning 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

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**
			(11,797)	(9,612)	(1,030)	(917)
Destroyed	3,037**	3,408***				
	(1,211)	(1,025)				
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 of whether individual changed house after 1960, and a dummy of whether individual is born in the Westman Islands. Robust standard errors, clustered at the individual level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1

estimate is statistically significant at the 1% level. When constructing standard errors, we cluster observations at the individual level to allow for arbitrary correlation across time. The point estimates are similar with and without controls. The controls we include are age and year fixed effects as well as dummies for 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 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 lifecycle profile of earnings.[8] 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.[9]

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 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 69 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.[10] Figure A.1 in the appendix plots quantile treatment effects when the logarithm of earnings is the dependent variable. When viewed in pro-

[8]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.

[9]Yagan (2016) finds that moving is strongly negatively correlated with employment (conditional on age and other demographics).

[10]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.

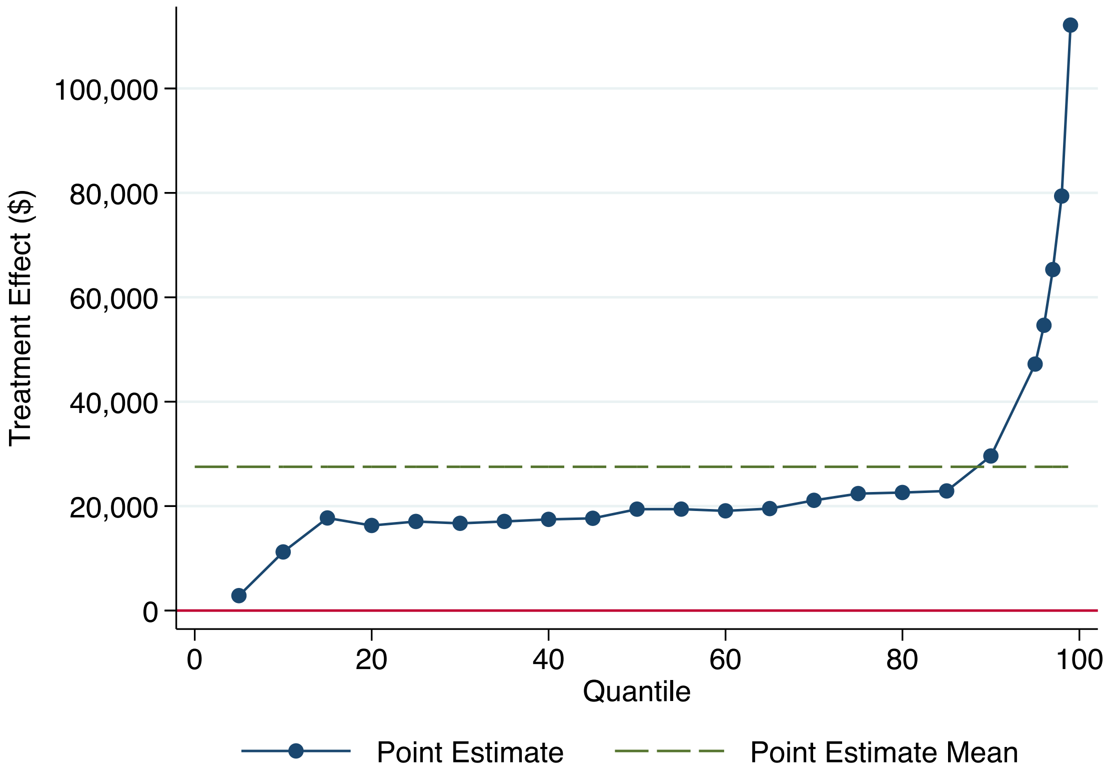


Figure 5: Quantile Treatment Effects on Earnings – Cohorts Younger than 25 at time of Eruption

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.

portional 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 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

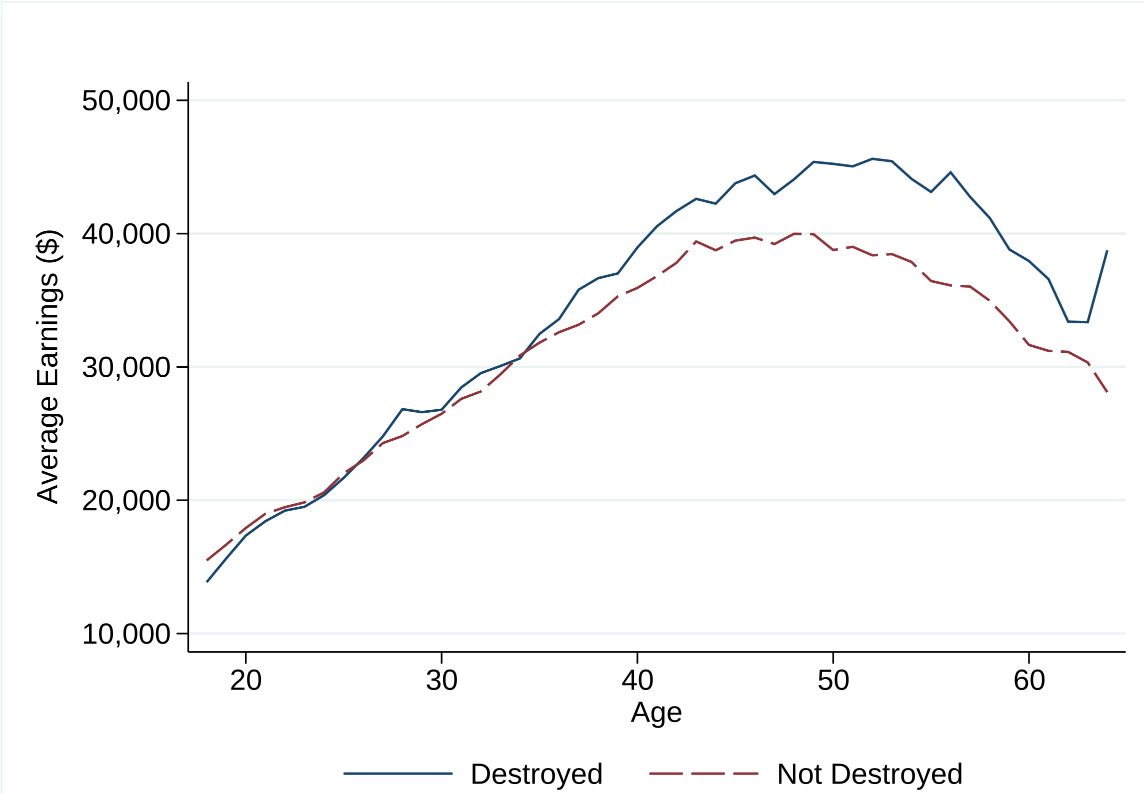


Figure 6: Earnings Effect Over the Life Cycle – Cohorts Younger than 25 at time of Eruption

and grow at least until age 50. At age 50, they are estimated to be roughly $50,000.[11] 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.[12]

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 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

[11] 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).

[12] 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.

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***
			(6,033)	(5,119)	(1,153)	(950)
Destroyed	-1,024	-725				
	(1,175)	(948)				
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 of whether individual changed house after 1960, and a dummy of whether individual is born in the Westman Islands. Robust standard errors, clustered at the individual level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1

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 it is the young that disproportionately benefit from moving is consistent with recent work by Chetty, Hendren, and Katz (2015) and Chetty and Hendren (2015) in other settings. Chetty and Hendren (2015) find evidence for a linear exposure effect—i.e., that the benefits of living 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.[13] Our result, therefore, suggest that the crucial distinction is whether individuals had settled on a carrier at the time of the eruption. Those young enough to shift careers were better able to take advantage of the "opportunity" the lava shock presented them.[14]

[13]We have also run linear specifications similar to those reported by Chetty and Hendren (2015). These do not support the existence of a linear exposure effect in our setting.

[14]Our results also differ from those of Chetty, Hendren, and Katz (2015), who find positive effects only for children who are younger than 13 at the time they move.

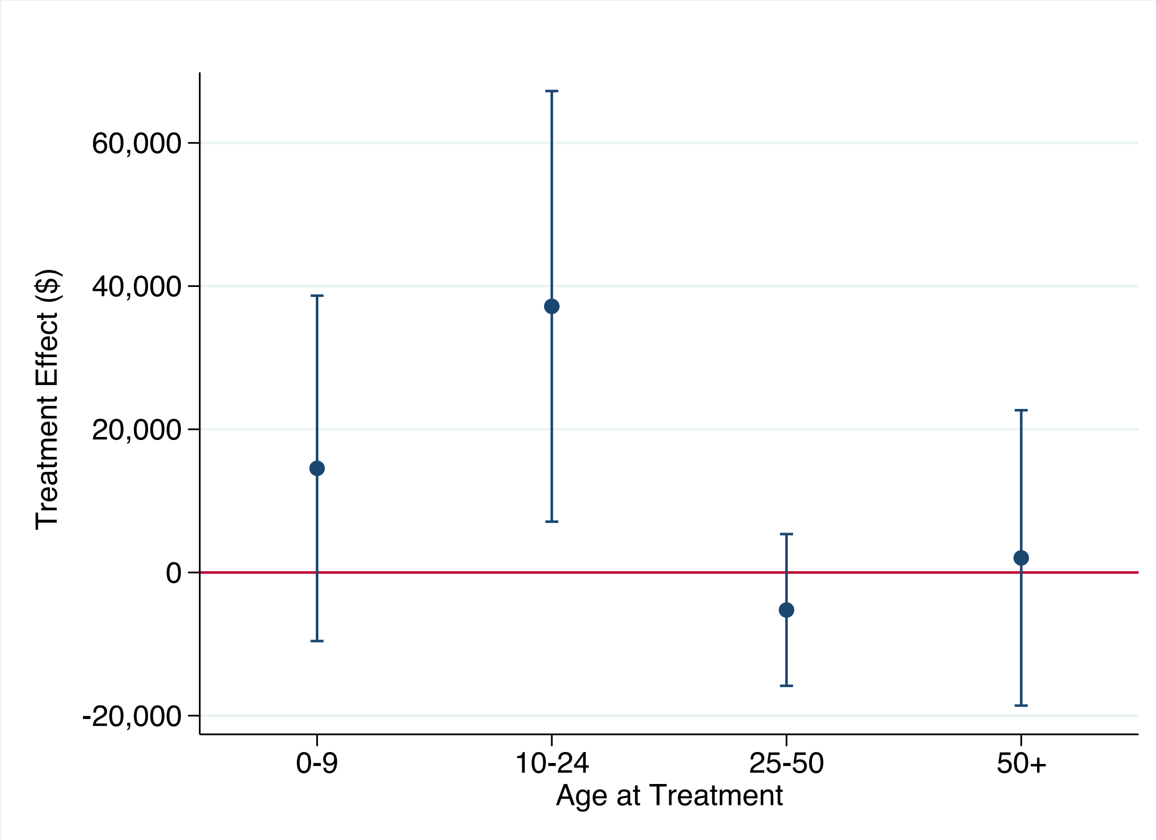


Figure 7: IV Earnings Effect – Four Age Groups

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 3.6 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

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)
$Moved$	3.59**	0.13	0.82	0.13	4.70**	-0.19
	(1.55)	(0.14)	(0.71)	(0.15)	(2.36)	(0.12)
Control group mean	13.51	13.51	11.98	11.98	12.71	12.71
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	2,262	2,262	1,101	1,101	2,826	2,826

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

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 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 3.6 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 4.7 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).

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 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 reflecting this channel.

Empirical work on the returns to education suggests that an additional year of schooling raises

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

	Junior College	University
	(1)	(2)
$Moved$	0.632***	0.224
	(0.223)	(0.175)
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 in parentheses. *** p<0.01, ** p<0.05, * p<0.1

income by roughly 10% (Card, 2001). This corresponds approximately to what one would obtain by comparing average incomes across educational groups in Iceland. During the period 2004-2014, the annual earnings premium for a worker with a junior college degree 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 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.

We think this difference is unsurprising. In part, it likely to reflects comparative advantage (see section 9) since those induced to move in our setting appear likely to have particularly high returns to education. Moreover, an interesting feature of studying educational effects associated with moving is that they reflect an interaction effect between moving and increased educational attainment. The returns to additional years of education may be much smaller than otherwise if an individual still faces large moving costs and is therefore only able to use his or her additional education in his or her original location. In contrast, in our experiment, the increase in educational attainment we observe for movers is an optimizing response to new labor market conditions. This is likely to have larger benefits.

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 by the eruption. Perhaps the simplest possible interpretation of this result is that it is either due to luck, e.g., a (possibly unanticipated) decline in the returns to fishing, or to the Westman Islands being a fundamentally bad place to earn income during our sample period.

These explanations are, however, hard to square with the fact that the Westman Islands was and is a relatively high income place. The population trends in Figure 2 show that the Westman Islands was a booming town at the time of the eruption. Furthermore, Figure 8 shows that average earnings in the Westman Islands have been higher than in Iceland's capital area (Reykjavik and suburbs) except for a few years during the financial boom last decade. Much prior work on mobility has focused on settings where people are deciding whether to move from a poor location to a richer location—e.g., rural-urban migration in developing countries and relocation out of low-income neighborhoods in the US. Our setting is very different in this regard. The people living in the Westman Islands at the time of the 1973 eruption received an opportunity to move away from a high income place to places with lower average income.

This raises the question of how it can possibly be so beneficial to move away from a high income place. In other words, how can both of the following two facts be true simultaneously: 1) average income in the Westman Islands is higher than in the places people move to, and 2) the causal effect of moving away from the Westman Islands is very large.

One logically possible—but perhaps not particularly plausible—explanation is that the people living in the Westman Islands at the time of the eruption are hugely positively selected in terms of their ability to earn income relative to people elsewhere in Iceland. If you combine this assumption with the notion that the Westman Islands are a really bad place to earn income, you can get both high average income in the Westman Islands and a large causal effect of moving away from the Westman Islands. In this case, average income in the Westman Islands can be higher than elsewhere in Iceland because the fact that the people are so much "better" than people elsewhere in Iceland makes up for the Westman Islands being a much "worse" place than other places in Iceland (all from an income earning perspective).

An alternative explanation of our findings—which we find more plausible—is that the causal effect we estimate is heterogeneous. Recall that the IV methodology we use identifies the causal

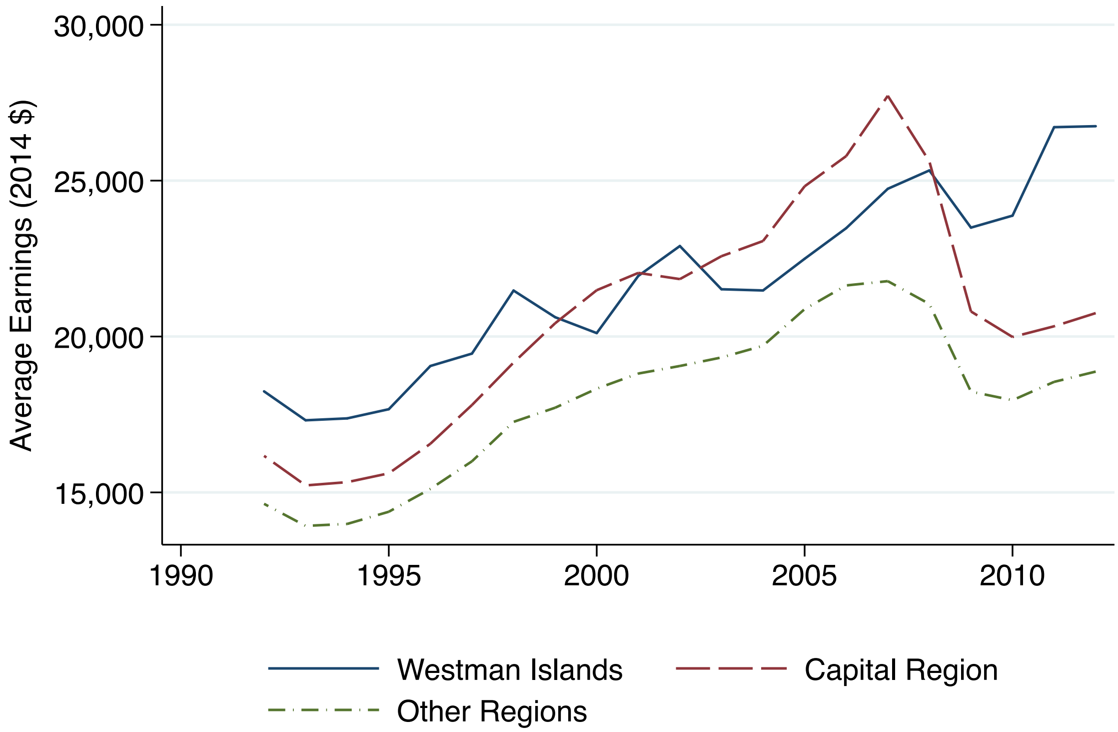


Figure 8: Evolution of Average Earnings Across Locations

effect on the "compliers" in our experiment—i.e., the group of people that are "marginal" in the sense that they only move if their house is destroyed.[15] Perhaps the causal effect on the compliers is much larger than the causal effect on other groups because the compliers are particularly poorly suited to live in the Westman Islands. If this is the case, it may be easier to reconcile the high average income in the Westman Islands with the large causal effect we estimate. We develop this idea below.

9.1 The Importance of Comparative Advantage

While average income in the Westman Islands is high, the range of occupations available is quite limited. Like many small places, the Westman Islands is specialized in a particular industry. In the case of the Westman Islands, this industry happens to be fishing. Table 9 shows that fishing and fish processing make up roughly 70% of the labor market in Westman Islands, as measured by their share of payroll taxes. In contrast, the labor market in Reykjavik is quite a bit more diverse.

[15] Angrist (2004) decomposes the population into four groups. Some individuals would have moved away regardless of whether their house was destroyed ("always-takers"); some did not move despite their house being destroyed ("never-takers"); some moved away because their house was destroyed ("treated compliers"); and some would have moved if their house were destroyed but were unaffected by the lava shock ("untreated compliers").

Table 9: 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.

The highly specialized nature of the labor market in the Westman Islands likely means that this is a good place for some to work but a much worse place for others. The Westman Islands may be the ideal place (in terms of earnings) for those whose comparative advantage lies in skills that are valued in the fishing industry. But the Westman Islands is likely a much worse location for those whose comparative advantage lies elsewhere, e.g., those with a comparative advantage in jobs requiring a large amount of education such as law, computer science, medicine, engineering, etc.

If moving costs were low, people born in the Westman Islands whose comparative advantage lies in occupations not well represented in the Westman Islands would leave and others with a comparative advantage in skills valued in the fishing industry would come instead. However, if moving costs are large, many people may be “stuck” in the Westman Islands who are not a good match for living there. For this group, the causal effect on earnings of being able to move is potentially very high, while the causal effect for those well matched to the islands is likely much lower and may even be negative.

In a setting like this, in which comparative advantage is important and moving costs are large, those induced to move by the volcanic eruption will disproportionately be those who are poorly matched to the Westman Islands in terms of comparative advantage. In other words, the compliers in our experiment are highly selected to be those with a particularly high causal effect of moving.

To illustrate this, it is useful to write down a Roy model with heterogeneous comparative advantage and moving costs. We do this in appendix D. The model we develop is based on the

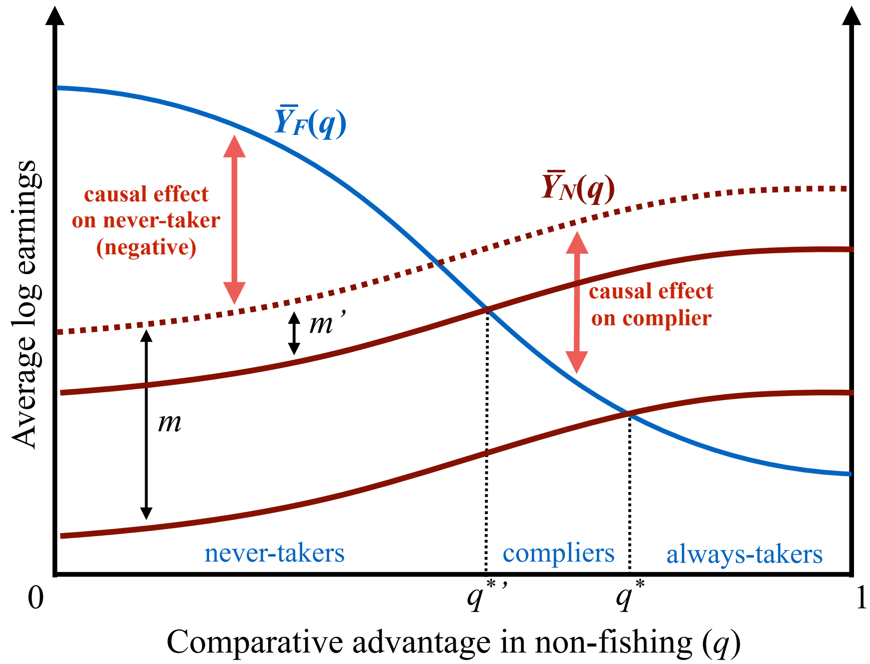


Figure 9: A Shock to Moving Costs

models in Lagakos and Waugh (2013), Young (2013), and Adao (2015). Our model extends these models to include a moving cost and uses it to illustrate that the causal effect on compliers is larger than for the population as a whole.

The key points from the model can be illustrated using Figure 9 which plots the earnings of workers living in the Westman Islands at the time of the eruption as a function of their comparative advantage and occupational choice. Workers are assumed to be endowed with heterogeneous skills in two sectors: fishing and non-fishing. Some workers have a comparative advantage in the fishing sector, while others have a comparative advantage in the non-fishing sector. In the figure, workers are ranked by comparative advantage in non-fishing along the horizontal axis with q indexing the degree of comparative advantage. Workers with a strong comparative advantage in fishing are to the left in the figure (low q), while those with a strong comparative advantage in non-fishing are to the right in the figure (high q). The figure plots average income in the fishing and non-fishing sectors as a function of q. These are denoted $\bar{Y}_F(q)$ and $\bar{Y}_N(q)$, respectively.

Suppose for simplicity that the Westman Islands only employs people in the fishing sector. Workers living in the Westman Islands that would like to work in the non-fishing sector must move to the mainland of Iceland, which is costly. At the time of the eruption, a fraction of the workers in the Westman Islands exogenously face a lower moving cost than the rest of the workers because their houses are destroyed in the eruption. The workers whose houses are destroyed face a moving cost of m', while those whose houses are not destroyed face a moving cost of $m > m'$.

Workers will self-select into the sector in which they earn the most net of moving costs. They therefore choose between $\bar{Y}_F(q)$ and $\bar{Y}_N(q) - m'$ if their house was destroyed and between $\bar{Y}_F(q)$ and $\bar{Y}_N(q) - m$ if there house was not destroyed. We have plotted $\bar{Y}_N(q) - m'$ and $\bar{Y}_N(q) - m$ on Figure 9 for convenience (the two lower upward-sloping lines). Those to the right of q^* will move whether or not their house is destroyed (since $\bar{Y}_N(q) - m' > \bar{Y}_N(q) - m > \bar{Y}_F(q)$ when $q > q^*$). These are the "always-takers" in our experiment. Those to the left of $q^{*\prime}$ will not move even if their house is destroyed (since $\bar{Y}_N(q) - m < \bar{Y}_N(q) - m' < \bar{Y}_F(q)$ when $q < q^{*\prime}$). These are the "never-takers" in our experiment. Those with comparative advantage between $q^{*\prime}$ and q^* will move only if their house is destroyed (since $\bar{Y}_N(q) - m < \bar{Y}_F(q) < \bar{Y}_N(q) - m'$ when $q^{*\prime} < q < q^*$). These are the "compliers" in our experiment, i.e., those induced to move by their house being destroyed.

Our IV estimator estimates the causal effect on the compliers. This causal effect is positive in Figure 9 and can be quite large ($\bar{Y}_N(q)$ lies above $\bar{Y}_F(q)$ between $q^{*\prime}$ and q^*). However, it is also clear from the figure that the causal effect on other groups can be quite different. In particular, the causal effect on the never-takers is smaller than the causal effect on compliers and can easily be negative. Figure 9 depicts a case were the causal effect is negative for most never-takers (all of those to the left of the point where the $\bar{Y}_F(q)$ line crosses the $\bar{Y}_N(q)$ line). These workers 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. We analyse the model in more detail in appendix D.

Figure 9 thus illustrates that the compliers in our experiment are highly selected to be those with a particularly high causal effect of moving. This selection effect helps explain the large magnitude of the causal earnings effects we estimate and how these large causal effects can be consistent with high average income in the Westman Islands. The basic idea is that there is a group of people who are quite poorly matched to the Westman Islands but are "stuck" there due to moving costs. These people move and are much better off for it. But there are other people that are well matched to the Westman Islands and don't move and enjoy high income living there.

Our results suggest 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

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

Variable (X)	$\Pr[X_i = 1]$	$\Pr[X_i = 1 \vert \text{Complier}]$	$\frac{\Pr[X_i=1 \vert \text{Complier}]}{\Pr[X_i=1]}$
Female	0.49	0.34	0.69 (0.19)
Age (> median)	0.51	0.40	0.79 (0.18)
Change house after 1960	0.60	0.75	1.25 (0.16)
Born in Westman Islands	0.80	0.82	1.03 (0.09)
House value (> median)	0.64	0.68	1.06 (0.10)
House year (> median)	0.61	0.72	1.17 (0.19)
Parents education (> cumpulsory)	0.50	0.75	1.51 (0.22)
Parents married	0.88	1.05	1.19 (0.07)

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 are in parentheses.

not well represented in that location may suffer a great deal in terms of life-time earnings even if the location in question is high income on average.

9.2 Evidence on Comparative Advantage

To support this story of comparative advantage, it is useful to investigate who the compliers are in our experiment. Table 10 presents statistics on characteristics of the compliers.[16] The third column reports the frequency of certain characteristics among compliers relative to the overall population (we focus here on cohorts younger than 25 at the time of the eruption). 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 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.

While the fishing industry pays high wages, it requires little formal education. One sign of

[16] Although individual compliers cannot be identified in the data, their average characteristics can be estimated using Bayes theorem, when both the treatment status and the instrumental variable are binary (Angrist, 2004). For further discussion on estimation of treatment effects under imperfect compliance, see Imbens and Angrist (1994) and Angrist and Pischke (2009).

Table 11: 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.

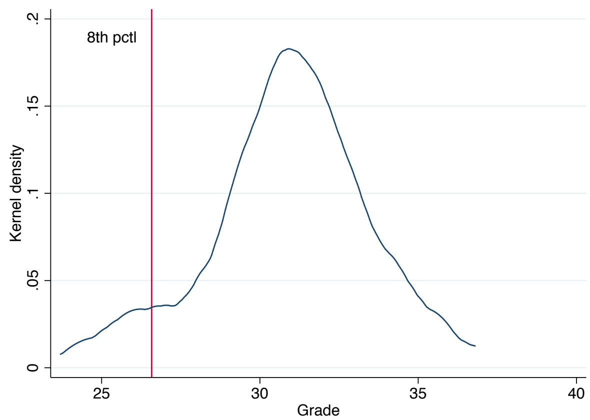
this is that educational attainment in the Westman Islands is low. Table 11 reports educational attainment in the Westman Islands, Iceland's capital area, and other areas in Iceland. Educational attainment is substantially lower in the Westman Islands than in Reykjavik. Only 20% of the working age population has a university degree, compared to 40% in the capital region.

Moreover, students from the Westman Islands perform poorly on standardized tests relative to their peers elsewhere in Iceland. Figure 10 presents the distribution of average test scores by school on standardized tests at the end of compulsory schooling (10th grade) in Reykjavik (left-hand side panels) and other regions (right-hand side panels) in the years 2010-2014. The red vertical line in each panel represents the average score for the Westman Islands. Relative to both schools in Reykjavik and elsewhere in Iceland, standardized test scores in the Westman Islands are quite low in all subjects. This may be because schools in the Westman Islands are poor quality or because the students and their parents do not value education and therefore do not put in as much effort as students and parents elsewhere.

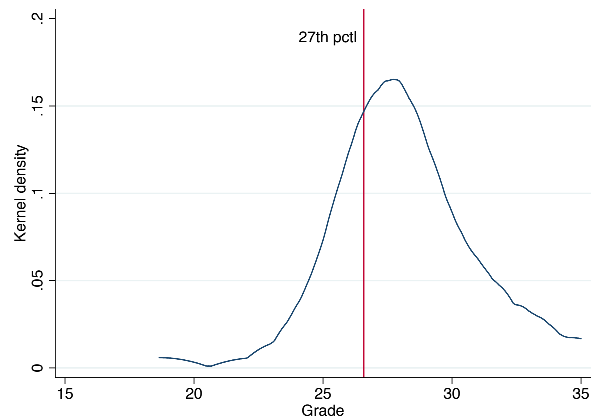
The fact that the Westman Islands is a place that specializes in occupations for which education is not valuable suggests that returns to education in the Westman Islands are low and that the Westman Islands are a poor match for people with a comparative advantage in occupations for which education is important. This supports our interpretation that the mobility shock we identify likely disproportionately induces highly educated households to move and helps us understand the large size of our estimated causal effect of moving since the skills of these highly educated households were more highly valued in other locations than the Westman Islands.

9.3 Compensating Differentials

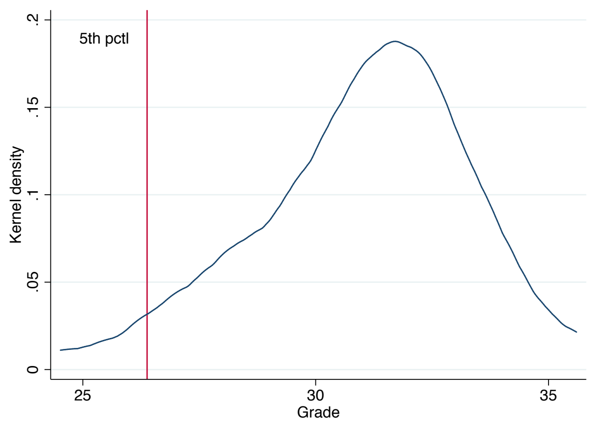
Are the greater earnings obtained by those who move away from the Westman Islands compensation for non-pecuniary costs? This is an issue that besets most work on the costs of moving, but which we believe is relatively unimportant in our setting. Conventional wisdom in Iceland is that the price level in rural towns like the Westman Islands has traditionally been higher than in Reyk-



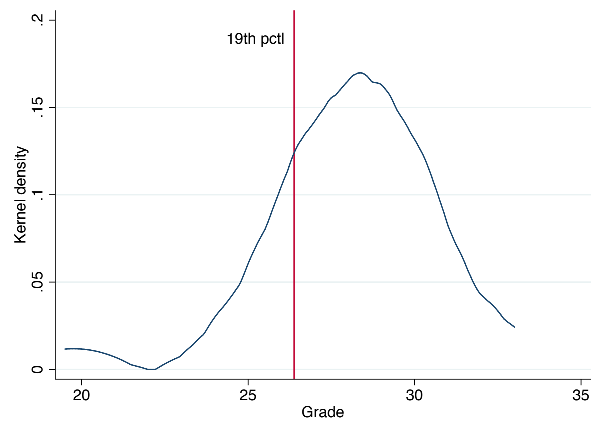
(a) Mathematics – Capital Region



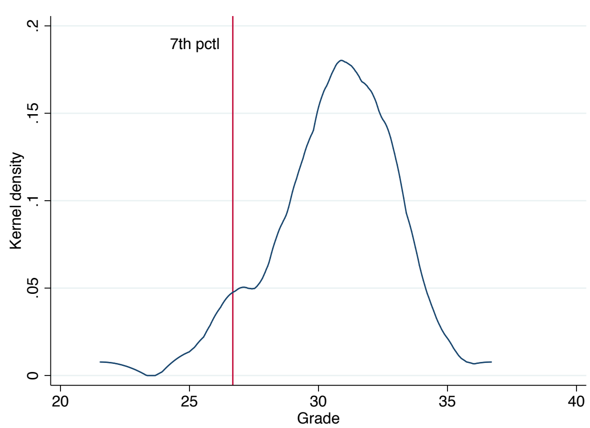
(b) Mathematics – Other Regions



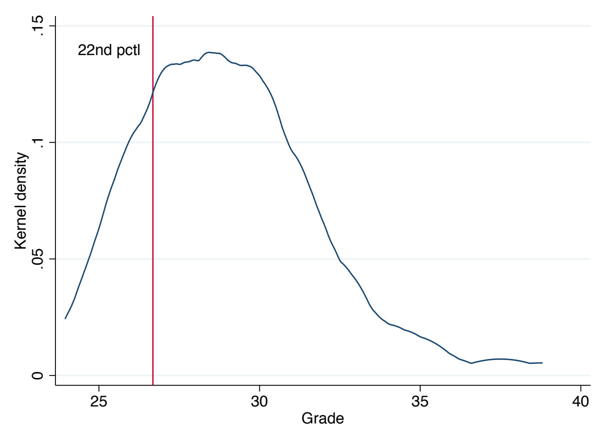
(c) English – Capital Region



(d) English – Other Regions



(e) Icelandic – Capital Region



(f) Icelandic – Other Regions

Figure 10: 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.

javik (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.

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 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 12 reports the causal effect of moving on 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 other outcomes is, according to conventional views, uniformly positive.

Table 12: Other Outcomes – Cohorts Younger than 25 at Time of Eruption

	IV (1)	OLS (2)	Control Mean (3)
Early Pension	-0.084*	0.001	0.081
	(0.045)	(0.006)	
Early Death	-0.112**	-0.005	0.036
	(0.052)	(0.007)	
Married	0.171	-0.038***	0.628
	(0.110)	(0.014)	
Number of Children	0.089	-0.100**	2.30
	(0.390)	(0.050)	

Notes: Each coefficient estimate corresponds to regression of the dependent variable indicated in the top panel on $Moved$. Controls: gender, cohort, change house after 1960, born in the Westman Islands, year dummies and age dummies. *Early Pension* is a dummy variable that takes the value 1 if individual receives pension and is younger than 65, but zero otherwise. *Early Death* is a dummy variable that takes the value 1 if individual dies before age 50, but zero otherwise. *Married* is an indicator of being registered as married in the National Registry. *Number of Children* is number of children born after the eruption, in 1973 or later. Robust standard errors clustered at the individual level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

These estimates suggest that movers are both less likely to die before the age of 50 and less likely to receive pension payments before the retirement age of 65 due to illness or disability. The point estimate suggests they are more likely to get married, although the effect is imprecisely measured; and there is no effect on the number of children. Effects for the older cohorts are qualitatively similar, though they are smaller and none of the coefficients are statistically significant (see Table A.3).

For these reasons, we believe that compensating differentials are not driving our results. Rather we interpret our results as evidence of large barriers to moving of some kind that must have impeded mobility in our setting and generated large amounts of misallocation of labor across space.

9.4 Information Frictions and Intergenerational Bargaining

If individuals have imperfect information about the returns to moving, the decision to move will depend on *perceived* returns rather than actual returns. Informational barriers may therefore hinder mobility in the same way as other more traditional forms of moving costs. This friction has been emphasized in, e.g., the context of returns to education (Manski, 1993). In settings where education and income is low, perceived returns to education are much smaller than actual returns (Jensen, 2010).

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. Moreover, the fact that the returns to moving we estimate accrue to the children, and are largest at the peak of the mid-life earnings profile, long after the initial decision to move was made, may also contribute to it being difficult to assess the returns to moving. Furthermore, the decision to move was likely made by the parents. This no doubt exacerbated the informational frictions. Not only did a future computer genius or great legal mind need to understand that he or she would have higher earnings on the mainland, but this information needed to be communicated to his or her parents, and weighed against the potential negative consequences for the older generation. All of this suggests to us that information frictions may play an important role in explaining the large moving costs we estimate.

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 has 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 descendents of the originally affected population) get 4.7 more years of schooling.

The benefits of moving are very unequally distributed within the family. While the eruption had large positive effects on the earnings of the young, the earnings effects for those older than 25 at the time of the eruption are 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, while the gains accrue to children, potentially many decades later.

A unique feature of our environment, moreover, is that the location of out-migration is affluent relative to the destination locations. This suggests that our results should not be interpreted as the return from escaping a "bad" location. 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 junior college degrees. In junior college, students can choose between traditional tracks that prepare students for university studies and vocational tracks such as carpentry, hair-dressing, plumping, 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

$$Y_{it} = \alpha + \sum_{t=1981}^{2014} \beta_t Moved_i \times period_t + \mathbf{X}_i'\gamma + \delta_t + \varepsilon_{it}, \qquad (3)$$

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

$$Y_{it} = \alpha + \sum_{\tau=18}^{62} \beta_\tau Destroyed_i \times age_\tau + \mathbf{X}_i'\gamma + \delta_t + \varepsilon_{it} \quad (4)$$

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

$$Y_{it} = \alpha + \sum_{\tau=18}^{62} \beta_\tau Moved_i \times age_\tau + \mathbf{X}_i'\gamma + \delta_t + \varepsilon_{it} \quad (5)$$

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 A Model of Heterogeneous Treatment Effects

To demonstrate that comparative advantage can lead the treatment effect we estimate for the compliers in our natural experiment to be larger than the average treatment effect in the population, we present a Roy (1951) model with moving costs. The model we present is based closely on the model presented in Adao (2015). Our model generalizes Adao's model to include moving costs, while simplifying it along several other dimensions.[17]

Consider an economy with two regions and two sectors. The regions are the Westman Islands and the mainland of Iceland. The sectors are fishing and non-fishing. We use the generic index k to denote the sectors and denote fishing by F and non-fishing by N. For simplicity, we assume that the only industry in the Westman Islands is fishing. Workers born in the Westman Islands can therefore work as fishermen freely, but if they would like to work in another industry, they need to move to the mainland. We assume that it is costly to move to the mainland and denote this cost

[17] Adao's model is a generalization of the models developed in Lagakos and Waugh (2013) and Young (2013).

by m. The form that this cost takes is that a fraction $1 - \exp(-m)$ of labor income is lost when a worker moves to the mainland.

Each worker $i \in \mathcal{I}$ is endowed with one unit of time which she supplies inelastically to the labor market. Each worker is also endowed with a bivariate skill vector $(z_F(i), z_N(i))$, where $z_k(i)$ is the number of efficiency units of labor the worker produces if employed in sector k. It is convenient to define individual i's comparative advantage in the non-fishing sector to be

$$s(i) \equiv \ln\left[z_N(i)/z_F(i)\right]$$

and her absolute advantage to be

$$a(i) \equiv \ln[z_F(i)].$$

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

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

$$Y_F = A_F L_F, \quad Y_N = A_N L_N, \tag{6}$$

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, Iceland is a small country 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

$$W_F = P_F A_F, \quad W_N = P_N A_N, \tag{7}$$

respectively.

The workers self select based on their comparative advantage into the sector which gives higher income. The set of workers employed in each sector is given by

$$\mathcal{S}^F = \{i \in \mathcal{I} : w_F z_F(i) \geqslant \exp(-m) w_N z_N(i)\}$$
$$\mathcal{S}^N = \{i \in \mathcal{I} : w_F z_F(i) < \exp(-m) w_N z_N(i)\}$$

Labor income of worker i in sector k is $Y_k(i) = W_k z_k(i)$. Using the definitions of comparative advantage and absolute advantage, we can write the logarithm of labor income of worker i in sector $k \in \{N, F\}$ as

$$y_N(i) = w_N + s(i) + a(i), \quad y_F(i) = w_F + a(i), \tag{8}$$

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

It is convenient to rank workers according to their comparative advantage. 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. Workers at higher quantiles q have a stronger comparative advantage in the non-fishing sector, or equivalently a stronger comparative disadvantage in fishing.

Average log earnings for each quantile q in the non-fishing and fishing sectors are

$$\bar{Y}_N(q) = w_N + \alpha(q) + A(q), \quad \bar{Y}_F(q) = w_F + A(q), \tag{9}$$

respectively. Here $A(q)$ denotes the mean of the absolute advantage conditional distribution $H(a|\alpha(q))$ at quantile q.

Figure D.1 plots average earnings in each sector for workers at different quantiles of comparative advantage in the Westman Islands. If a worker chooses to work in the fishing sector, she will on average earn $\bar{Y}_F(q)$. If she chooses to work in the non-fishing sector, she will need to move away from the Westman Islands, which is costly. Taking account of these moving costs, she will on average earn $\bar{Y}_N(q) - m$ if she chooses to work in the non-fishing sector.

We have drawn Figure D.1 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 relatively more productive at fishing than those that have a comparative advantage at non-fishing. 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 $\bar{Y}_N(q)$ have a larger slope than $\bar{Y}_F(q)$ (i.e., workers differ in their comparative advantage).

Workers will self-select into the sector in which they earn the most net of moving costs. Figure D.1 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. The figure plots a case in which there is "positive

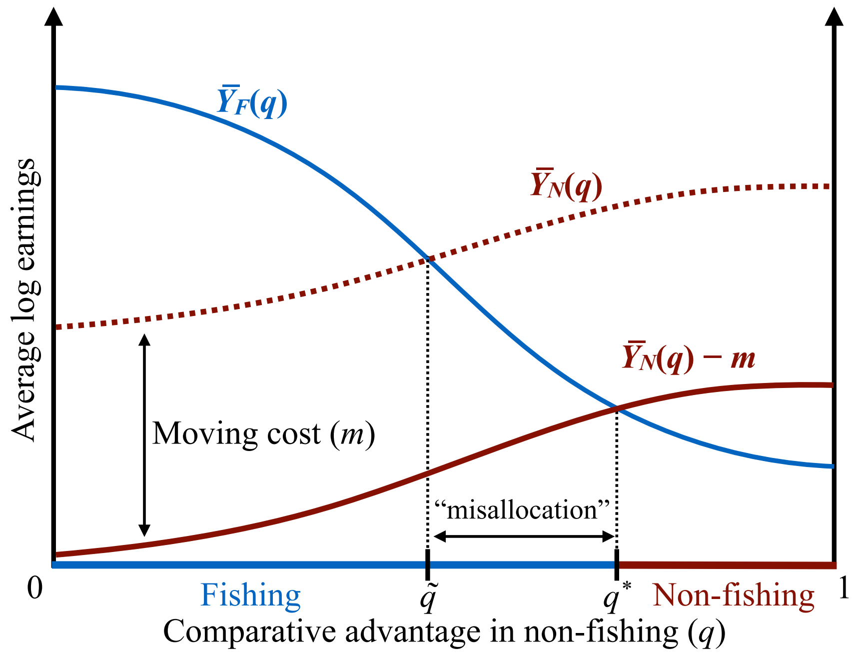


Figure D.1: Sorting by Comparative Advantage

selection" into both sectors in the sense that average earnings in the sector are higher than the earnings of the marginal worker in the sector.

Figure D.1 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 worker between $\tilde{q}$ and q^* are misallocated and are earning less than they would without the moving cost.

The model that we have laid out above is not explicitly dynamic. A simplistic view would be that workers move to their location of comparative advantage at the beginning of time and after that there is no further migration. A more realistic view is that there will be continual migration for at least two reasons. First, each year a new cohort of workers enters the labor market. Some of these new workers have a comparative advantage in the fishing sector, while others don't.[18] Second, workers face shocks to comparative advantage (they loose their good job in the fishing sector, they improve their education, or they simply learn more about their abilities). For these

[18]Here we implicitly assume that children are born with a skill set that may differ from their parents'.

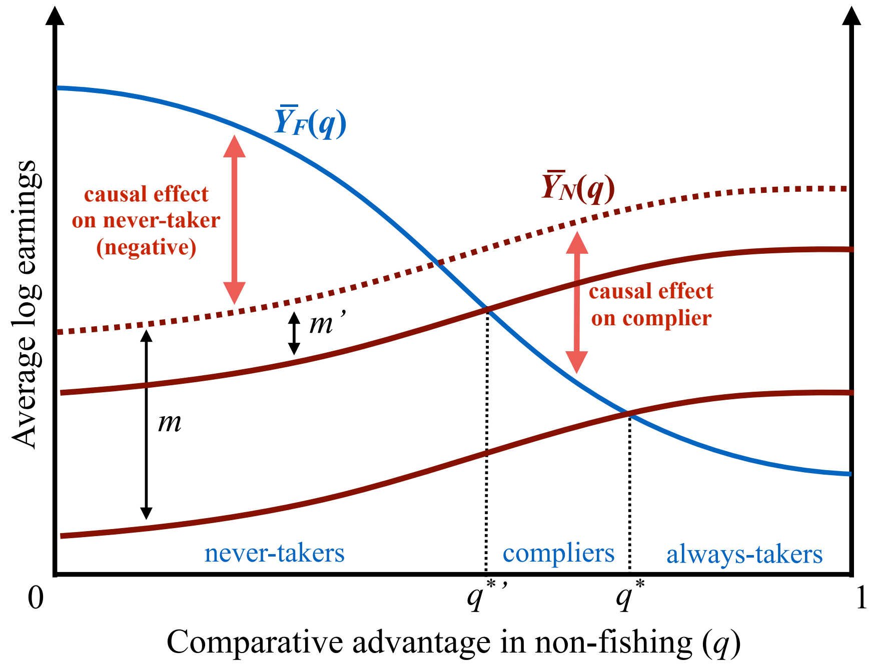


Figure D.2: A Shock to Moving Costs

reasons, the sorting that is depicted in Figure D.1 will repeat itself each period.

Let's now consider the situation at the time of the eruption. At this time, a fraction of the workers in the Westman Islands exogenously face a lower moving cost than the rest of the workers because their houses are destroyed in the eruption. This situation is depicted in Figure D.2. The workers whose houses are destroyed face a moving cost of m', while those whose houses are not destroyed face a moving cost of $m > m'$.[19] Since those whose houses are destroyed face lower moving costs, more of them move away. In Figure D.2, this is reflected in a cutoff quantile for moving $q^{*\prime}$ for those whose houses are destroyed that is to the left of the cutoff quantile q^* for those whose houses are not destroyed.

Using the terminology of Angrist (2004), we can divide workers into three groups. Workers to the left of $q^{*\prime}$ in Figure D.2 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 workers move only if their house is destroyed. Finally, workers to the right of q^* are "always-takers." These workers move even if their house is not destroyed.

Our instrumental variables estimator estimates the causal effect on the compliers. This causal effect is positive and can potentially be quite large. However, it is clear from the figure that the

[19]The moving cost m at the time of the eruption is likely lower than the moving cost in other times. But this is not important for our analysis.

causal effect on other groups can be quite different. In particular, the causal effect on the never-takers is smaller than the causal effect on compliers and can easily be negative. Figure D.2 depicts a case were the causal effect is negative for most never-takers (all of those to the left of the point where the $\bar{Y}_F(q)$ line crosses the $\bar{Y}_N(q)$ line). These workers 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.

Figure D.2 also illustrates how the OLS estimate can be much lower than the IV estimate. The OLS estimator compares all of those that move with all of those that stay. The stayers include the never-takers and the non-treated compliers, while the movers include the always-takers and the treated compliers. The OLS estimate is therefore affected by any difference in average earnings between never-takers and always-takers. We have drawn Figure D.2 such that always-takers have lower average income than never-takers. In this case, OLS will yield a smaller estimate than IV. Notice that this is the case even though the causal effect on the always-takers is larger than the causal effect on the compliers. OLS is not a measure of the causal effect on always-takers.

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
			(0.387)	(0.324)	(0.041)	(0.038)
$Destroyed$	0.094**	0.110***				
	(0.041)	(0.037)				
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 logaritm of life-time labor earnings. The set of controls includes gender, a dummy of whether individual changed house after 1960, and a dummy of whether individual is born in the Westman Islands. Robust standard errors, clustered at the individual level, 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)
$Moved$			42,773	30,732	-6,397***	-5,607***
			(35,227)	(29,204)	(1,467)	(1,347)
$Destroyed$	2,538	1,793				
	(1,595)	(1,433)				
Control group mean	30,650	30,650	30,650	30,650	—	—
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	15,259	15,259	15,259	15,259	15,259	15,259

Notes: Controls: gender. Robust standard errors clustered by individual 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)
Early Pension	-0.013	-0.008	0.054
	(0.027)	(0.005)	
Early Death	-0.025	-0.009**	0.017
	(0.022)	(0.004)	
Married	0.109	0.009	0.700
	(0.085)	(0.018)	
Number of Children	0.131	-0.167***	1.08
	(0.238)	(0.048)	
Earnings > 0	0.011	-0.022**	0.622
	(0.047)	(0.010)	

Notes: Each coefficient estimate corresponds to regression of the dependent variable indicated in the top panel on $Moved$. Controls: gender, cohort, change house after 1960, born in the Westman Islands, year dummies and age dummies. *Early Pension* is a dummy variable that takes the value 1 if individual receives pension and is younger than 65, but zero otherwise. *Early Death* is a dummy variable that takes the value 1 if individual dies before age 50, but zero otherwise. *Married* is an indicator of being registered as married in the National Registry. *Number of Children* is number of children born after the eruption, in 1973 or later. Robust standard errors clustered at the individual level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

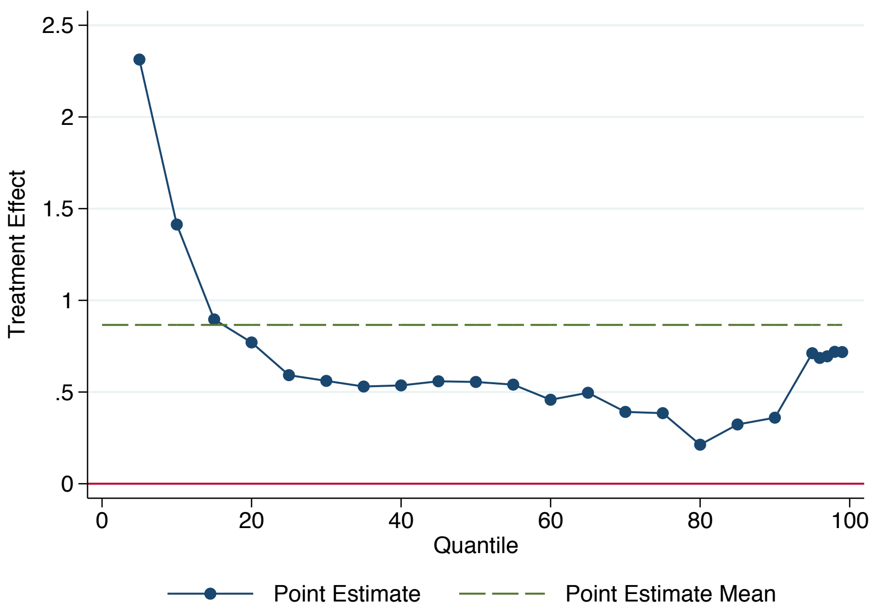
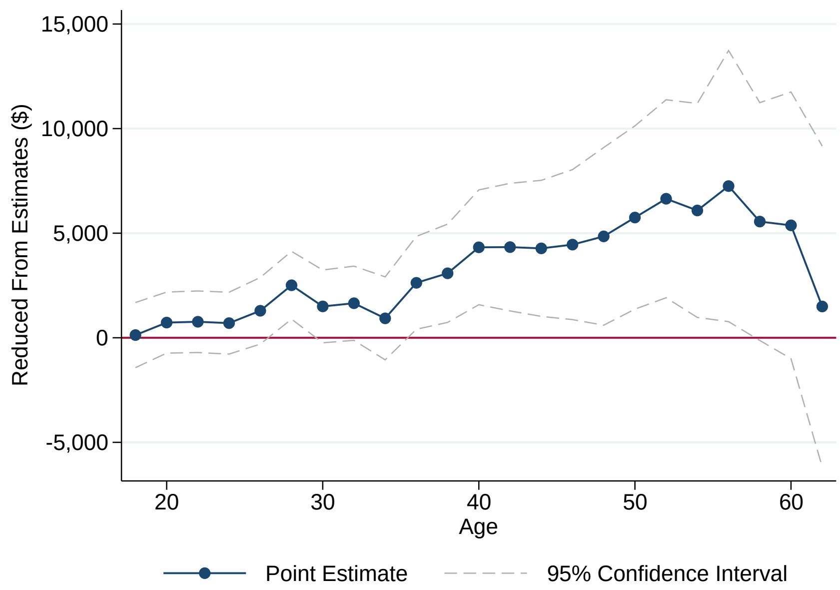
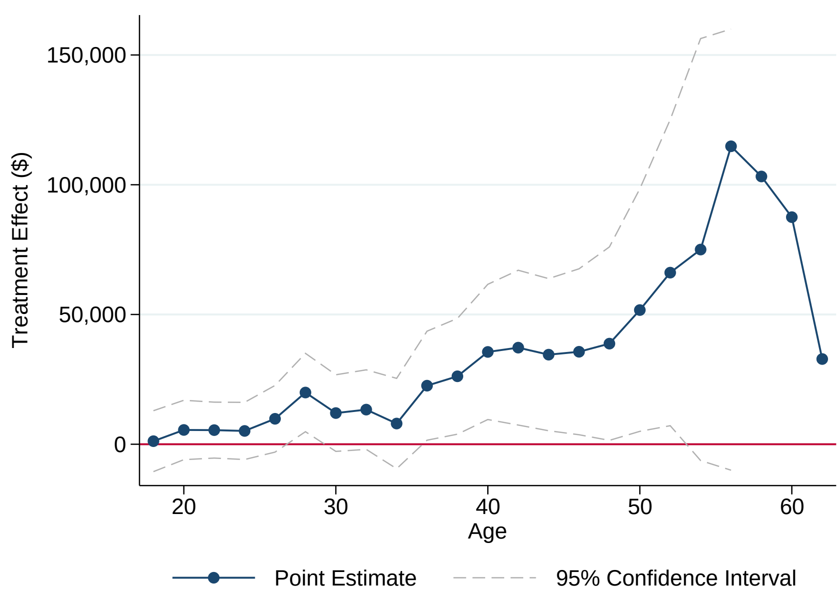


Figure A.1: IV Quantile Effects for Log(Earnings) – Cohorts 25 and Older at time of Eruption

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 individual 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.

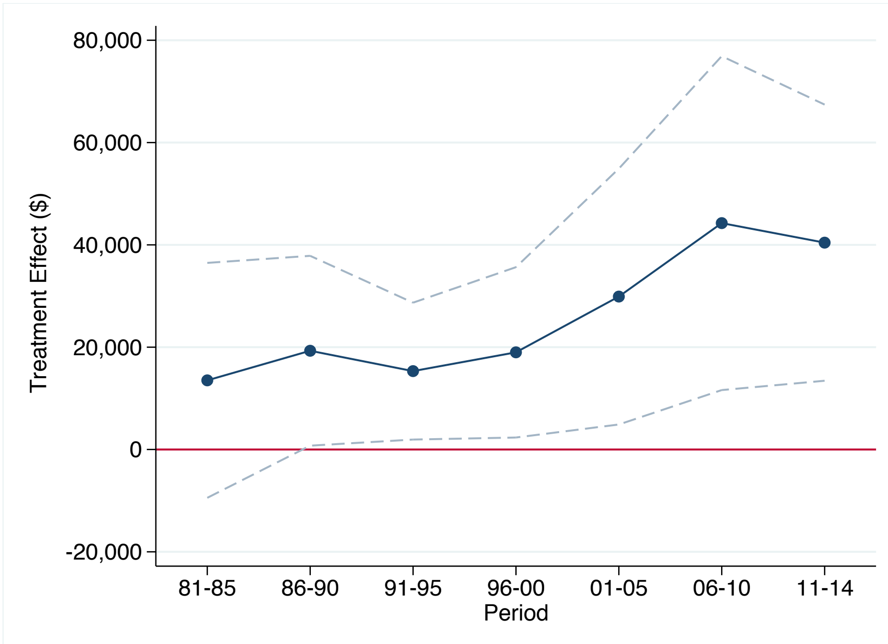


Figure A.3: IV Earnings Effect by Year – Cohorts Younger than 25 at time of Eruption.

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 individual level.

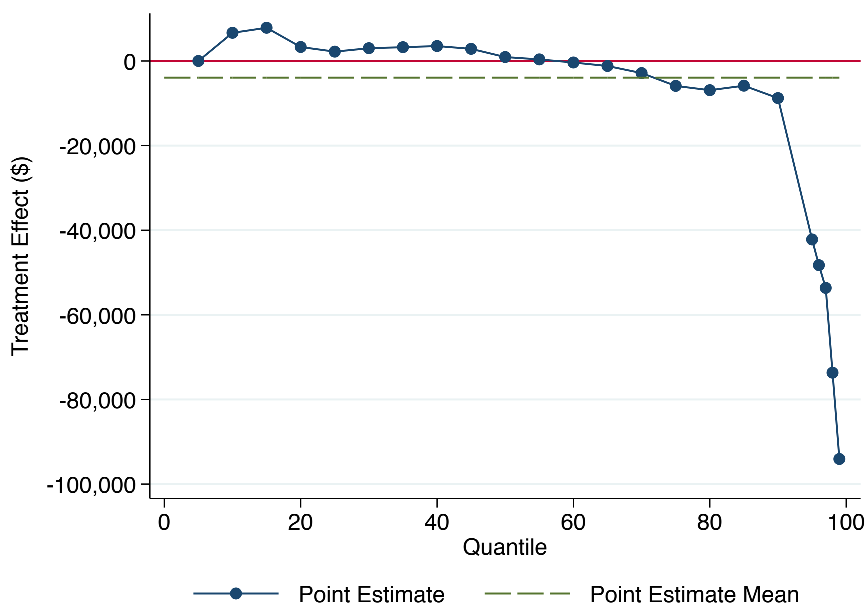


Figure A.4: IV Earnings Quantile Effects – Cohorts 25 and Older at time of Eruption

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.

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