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. In our experiment, a third of the houses in a town were covered by lava. People living in these houses were much more likely to move away permanently. For the dependents in a household (children), we estimate that being induced to move by the "lava shock" dramatically raised lifetime earnings and education. Yet, the benefits of moving were very unequally distributed across generations: the household heads (parents) were made slightly worse off by the shock. These results suggest large barriers to moving for the children, which imply that labor does not flow to locations where it earns the highest returns. The large gains from moving for the young are surprising in light of the fact that the town affected by our volcanic experiment was (and is) a relatively high income town. We interpret our findings as evidence of the importance of comparative advantage: the gains to moving may be very large for those badly matched to the location they happened to be born in, even if differences in average income are small.

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1 Introduction

Wages differ enormously across space. One interpretation of such differentials is the presence of large barriers to moving, arising from informational, cultural, legal, and economic obstacles that impede labor from flowing to its highest return activity (Munshi and Rosenzweig, 2016; Bryan and Morten, 2019). However, just because the inhabitants of some locations have higher incomes than others does not mean there is a large causal effect of moving to these locations. The variation in average income across locations may be due to selection effects, whereby high productivity workers sort into certain locations, as opposed to the location having a direct causal effect on earnings (e.g., Lagakos and Waugh, 2013; Young, 2013).

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

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

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

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

We document a remarkable reversal of fortune for the young dependents (children) at the time of the eruption. Being "unlucky" enough to be induced to move away from the Westman Islands due to the destruction of one's house is associated with a large *increase* in long-run labor earnings and education for this group. Specifically, we estimate a causal effect of moving of $27,000 per year, or close to a doubling of the control group's average earnings, using the destruction of one's house as an instrument for moving. We calculate that for an 18 year old who is induced to move, the difference in the net present value of life-time earnings is roughly $440,000.[1] The causal effect on education for young dependents is almost four years of additional schooling (and their children's education responded even more).

The benefits of moving are, however, very unequally distributed within the family. In contrast to the effects for the dependents in the household, the earnings effects for the household heads are somewhat negative (but statistically insignificant). In other words, the economic costs of moving fall disproportionately on the parents in a family, while the economic gains accrue to the children. This implies that moving can be an immensely valuable but also somewhat costly gift that parents can give to their children. Conversely, the large intergenerational differences in the returns to moving are a potential important source of barriers to moving for the dependents in the family. After all, it is the parents that decide whether to move. As we describe below, limited parental altruism and understanding of the potential gains to their children of moving, as well as uncertainty about outcomes in other locations, may constitute important barriers to mobility.

[1] This estimate actually lines up quite well with existing structural estimates. Kennan and Walker (2011) estimate a structural model of migration decisions for young men within the United States, and find that the typical worker could roughly double his or her income by moving.

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

To explain these facts, we present a Roy model with heterogeneous comparative advantage and moving costs (building on recent work by Lagakos and Waugh (2013), Young (2013), Bryan and Morten (2019), Adao (2015)). Our model features an overlapping generations structure and an education choice. This allows the model to capture the large difference in causal effects we estimate between younger and older individuals in our sample: the young can reoptimize their education and career choice when they move, while this is more difficult for older individuals.

The second key idea in our model is the importance of comparative advantage. Roy's (1951) classic paper studied the matching between workers and tasks for the case of fishermen and rabbit hunters: those with greater relative prowess in fishing sorted into that industry, and the same for rabbit hunting. In our setting, those that were induced to move away from the Westman Islands did not become rabbit hunters, but they did move to places with very different returns to skill and education. Consequently, the incentives to move differ a great deal depending on a person's comparative advantage.

Our natural experiment—the "lava shock"—induces a subset of individuals to move, who would otherwise have stayed. The individuals who move if and only if their houses are destroyed are the "compliers" in our experiment, in the language of Imbens and Angrist (1994). Importantly, the complier group includes both individuals whose houses were destroyed (these move but would have stayed had their house been spared) and individuals whose houses were not destroyed (these stay but would have moved if their house had been destroyed). Our instrumental variables estimates identify the local average treatment effect for this complier group.

Who are the compliers in our experiment? Intuitively, while the Westman Islands—with its high-paying fishing jobs—may be an ideal place for workers with certain skills, it is unlikely to be the best match for a future computer whiz or a great legal mind. This implies that the subset of individuals induced to move by the lava shock are likely to be those that obtain particularly large gains from moving away—or equivalently, represent particularly severe cases of misallocation.[2] One piece of corroborating evidence for this that we document is that the compliers in our analysis come disproportionately from families with higher than average education. The children in these families are likely to benefit disproportionately from moving to a location where the returns to education are larger.

Our model provides a natural interpretation for how the benefits of moving we estimate can be so large, despite the fact that the individuals are moving away from a high income location. In the model, those induced to move have a stronger comparative advantage in activities other than fishing, which implies that they have large gains from moving. At the same time, the Westman Islands can have high average income because there are *other* individuals whose skills are well suited for the town's industrial structure. The combination of a large causal effect of moving away for the compliers and high average income is a natural outcome in a model like ours which features highly heterogeneous comparative advantage. In contrast, this is awkward to explain in a model of pure absolute advantage, such the widely used AKM model (Abowd et al., 1999).

While our setting is no doubt special in a number of ways, it is also important to recognize its parallels to other settings. Many smaller communities are, like the Westman Islands, specialize in a particular industry that is unlikely to be suitable for everyone. In such a setting, potential gains from moving may be large for some individuals who are "stuck" in locations in which the occupational mix is not well suited for their talents. Barriers to moving—including those that arise from intergenerational tradeoffs—may results in large amounts of misallocation even when differences in average incomes are small.

Might compensating differentials explain the large effects of moving we estimate? While any pattern of results can be explained by a sufficiently flexible model of (unobserved) compensating differentials, this is not a likely explanation in our case. Conventional wisdom in Iceland is that the price level in rural towns like the Westman Islands is and has always been higher than in Reykjavik. We confirm empirically, in Appendix F, that the prices of grocery products are indeed higher in the Westman Islands than in the Capital Region (which includes Reykjavik).

An alternative interpretation is that the compensating differentials arise from differences in preferences (Atkin, 2013). Perhaps people who grew up in the Westman Islands simply like the amenities it provides (a small, island community). However, this interpretation seems difficult to square with the time pattern of earnings effects which appear to grow across generations. If com-

[2] These findings echo and extend earlier results by Borjas et al. (1992).

pensating 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. However, the pattern in the data is the reverse: the earnings gains are highest for the young and their descendants, and much smaller for the parents. We also estimate causal effects of moving on a number of non-monetary outcomes and find that movers are less likely to die before the age of 50, less likely to receive pension payments before the retirement age of 65 due to illness or disability, and more likely to marry. None of these support the compensating differentials interpretation.

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

The paper proceeds as follows. Section 2 provides a short description of the volcanic eruption and its aftermath. Section 3 describes our data. In Section 4 outlines our empirical strategy, presents pre-treatment balance test and estimates of the effects of the lava shock on mobility. Section 5 presents our results on the effects on earnings and educational attainment. Section 6 presents a model of comparative advantage and analysis of the consequences of the volcanic eruption. Section 7 discusses our interpretation that the empirical results imply that moving costs are large and comparative advantage important. Section 8 concludes the paper. Additional background material and auxiliary analyses are relegated to an appendix.

2 A Volcanic Experiment

Just before 2:00am on January 23 1973 a volcanic eruption began in the Westman Islands, a tiny cluster of islands off the southern coast of Iceland. A 1500m long fissue opened only about 200-300 meters from the easternmost part of the island's prosperous town of 5,200 inhabitants (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

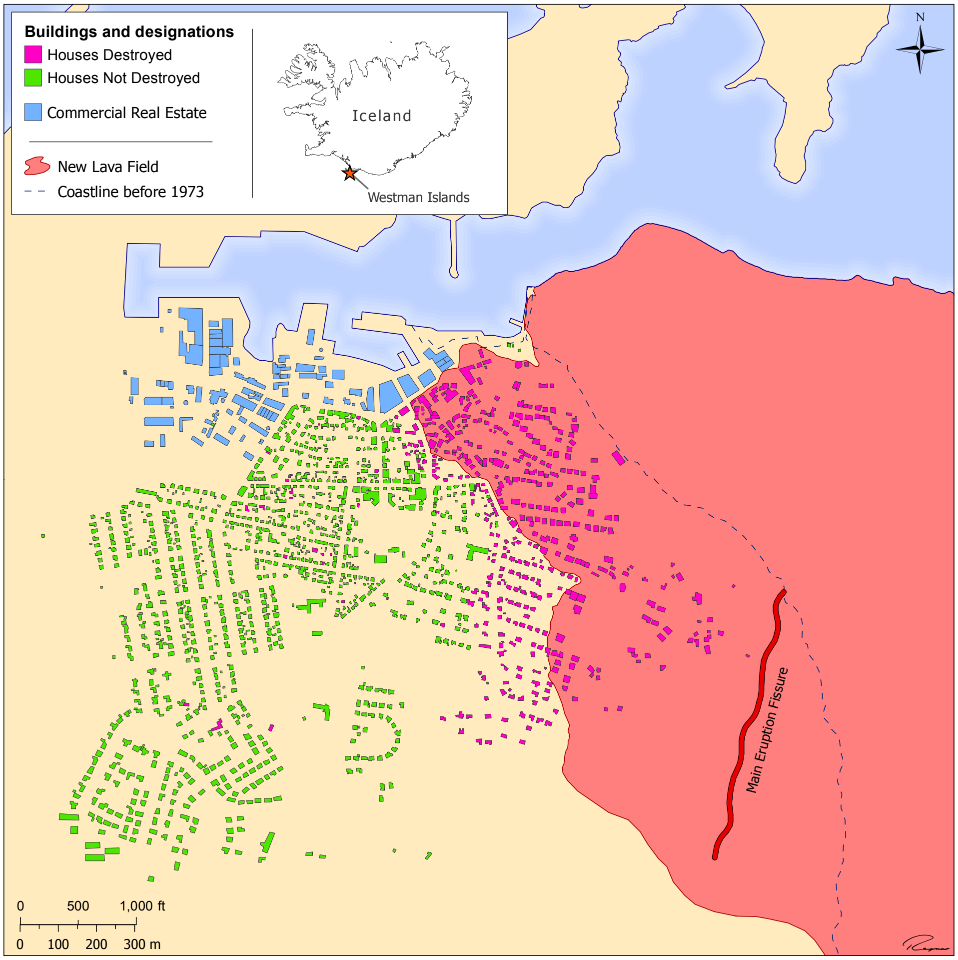

Figure 1: Map of Westman Islands town Post 1973 Eruption

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).

crucial in the evacuation. Within 4 hours, the evacuation was complete, with only a single fatality.

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

People began moving back to the Westman Islands in the summer and fall of 1973. Figure 2 shows that by the end of 1975, the population of the Westman Islands had returned to roughly

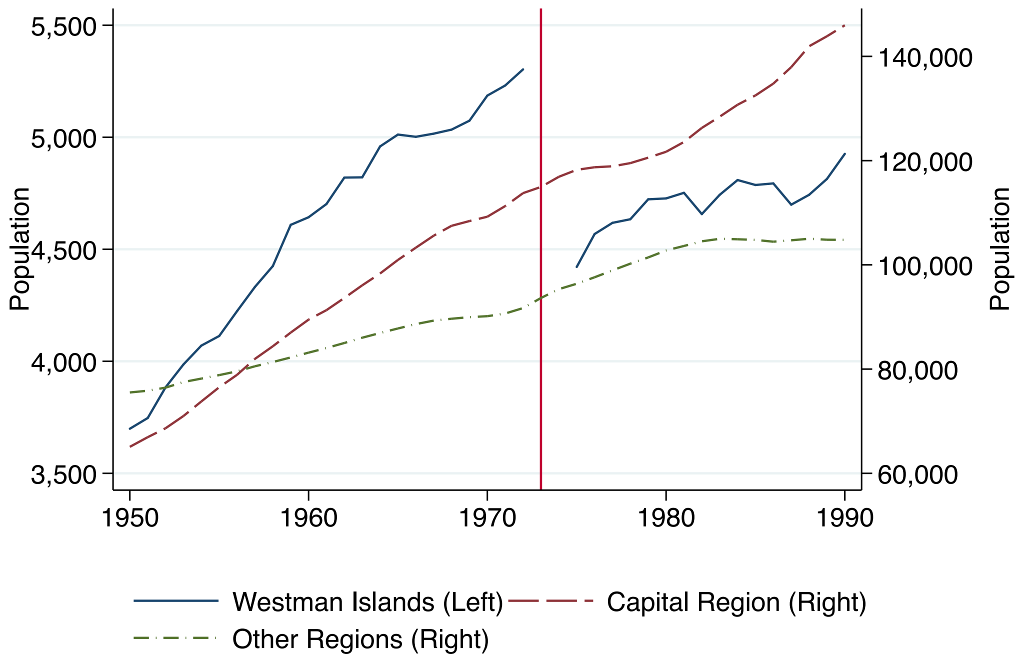

Figure 2: Population by Year

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.

85% of its pre-eruption level. The rapid recovery reflected the great economic importance of the Westman Islands to the Icelandic fishing industry since the town's location is the only viable harbor over a several hundred mile stretch on the southern coast of Iceland.[3] The fishing industry barely skipped a beat and by 1974 the fishing companies in the Westman Islands were back to normal production levels (see Figure A.1).

The Icelandic government set up a Disaster Relief Fund (Viðlagasjóður Íslands) to compensate those whose houses were destroyed. The compensation was paid out in four equal payments over the period October 1973 to July 1974. The goal was to "cash-out" households for the replacement value of their house and land, based on the annual fire insurance and tax valuations, indexed to inflation.[4] The land was also deemed to be "destroyed" since it could not be built on again for at least several decades after the eruption. The Disaster Relief Fund immediately took ownership of the destroyed real estate (and any associated mortgages) after the first payment was made. The

[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. In the end, the lava field created by the eruption actually improved the town's harbor, which meant that the economic fundamentals of the Westman Islands were, if anything, improved by the eruption.

[4]Such valuations are based on characteristics such as house size, age, etc., and are indexed to the construction cost index in Iceland.

Fund 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 additional features that were not included in the original assessments.

One important question is how errors in the valuation of houses—despite the government's considerable efforts—might affect our analysis. The most natural way such errors may affect our results is through wealth effects. Discrepancies in the payouts versus true housing valuations imply shocks to household wealth. While it is inevitable that these valuations contain some error, we believe that they are likely modest in relation to overall household wealth. It is unlikely that these wealth effects are behind our main results. We focus on the causal effect of moving on the young, occurring more than a decade after the eruption and persisting over their lifetime. It is hard to see how the modest wealth shocks on labor supply induced by measurement errors in the valuation of households could explain the large effects on earning we identify many years later.

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 (unreported) results are very similar.

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

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

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

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

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

where Y_{it} denotes earnings or education for individual i in year t. The variable $Moved_i$ is an indicator for having moved from the Westman Islands as of 1975. The causal effect of moving is

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

denoted by β. $\mathbf{X}_i$ is a vector of demographic characteristics, including a set of age fixed effects, with coefficient γ, and δ_t is a set of year fixed effects. Finally, ε_{it} is an error term that captures other determinants of income and education.

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

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

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

where $Destroyed_i$ is an indicator for individual i having lived in a house that was destroyed by 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 natural experiment Imbens and Angrist (1994). The compliers are the group of people that move if and only if their house is destroyed. Importantly, this group includes both individuals whose houses were destroyed (these move but would have stayed had their house been spared) and individuals whose houses were not destroyed (these stay but would have moved if their house had been destroyed). As we discuss in section 7, the causal effect on compliers—the local average treatment effect we estimate—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.[7] It is, of course, not unlikely that having one's house destroyed by lava affects earnings through other channels than whether one moves. However, it seems likely that these other channels negatively affect earnings, making the earnings

[7]As with all IV identification strategies, our empirical strategy requires a monotonicity assumption to be valid. In our context, this assumption rules out the existence of individuals that would have moved away after the eruption if their house had not been destroyed but were induced to stay by the fact that their house was destroyed. While it is possible that the monotonicity assumption is violated in our setting, we think it is unlikely. A reaction of defiance is likely to be strongest among those with the strongest attachment to the Westman Islands. But these are "never-takers" in our experiment.

Table 1: Descendant Groups

	Parent's Status ({father, mother})	Size
Destroyed	{D, D}, {D, A}, {A, D}	965
Not Destroyed	{N, N}, {N, A}, {A, N}	2,775
Excluded	{D, N}, {N, D}	282
Total		4,022

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

benefits of moving away even larger than those implied by our (already large) estimates.

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

For the descendants, the definitions of $Moved_i$ and $Destroyed_i$ are somewhat more subtle, since it was not the individuals themselves that moved due to the eruption or lived in houses that were destroyed, rather it was their parents that were directly affected by the eruption. For the descendants, $Moved_i$ is, therefore, an indicator for whether the *descendant* lived outside the Westman Islands when first observed in the administrative records. For $Destroyed_i$, there is the additional issue that each descendant has two parents, who may each have come from a destroyed (D) or non-destroyed (N) house in the Westman Islands, or may have come from another location in Iceland (A).

Table 1 illustrates how we code the indicator variable $Destroyed_i$ for descendants. The group of descendants that we code $Destroyed_i = 1$ is those descendants whose parents' status is one of the following {D,D}, {D,A}, or {A,D}, where the first entry is the father and the second entry is the mother. The group of descendants that we code $Destroyed_i = 0$ is those descendants whose parents' status is one of {N,N}, {N,A}, or {A,N}. We choose to exclude those that have one parent from a destroyed house and one parent from a non-destroyed house, i.e., the {D,N}, {N,D} groups. We

Table 2: Comparison of Groups

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

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

could alternatively have added these groups to both the 'destroyed' and 'not destroyed' groups. This would not have affected our point estimates (since their presence in both groups would mean that they would cancel out) but would have complicated the calculation of the standard errors.

We present results for those less than 25 years old at the time of the eruption, and those who were 25 years old and older. This breakpoint is meant to distinguish between people who had settled on a career at the time of the eruption and those that had not yet settled on a career. The idea is that the returns to moving may be different for those that can more easily reoptimize their carrier choice. We also present results for a finer age breakdown motivated by the recent literature that has emphasized heterogeneity in the benefits of moving for children as a function of age (e.g., Chetty and Hendren, 2018).

In addition to the distinction by age, we also present our main results in Section 5 distinguishing between "household heads" and "dependents." Individuals—both men and women—are classified as household heads if they are: 1) married, "cohabiting" (a legal construct for unmarried couples in Iceland), divorced, or widowed; or 2) the oldest male or female in the household and are older than 25 years old; or 3) between 18 and 25 years old, the oldest male or female, and living with someone older than 25 but less than 15 years older than they are. All others are classified as dependents.

This distinction is likely to be important, since household heads are likely to play a dominant role in making the decision to move, while their dependents are not. Hence, this distinction matches more closely the distinction between parents and children, which we make in our model in Section 6. However, as Table 2 shows, the two ways of defining the sub-groups are closely related. All of the dependents in our sample are less than 25, and the vast majority of household heads are over 25. The difference in the two definitions is that 9% of household heads are younger than 25.

4.1 Balance Tests

A basic requirement for our instrumental variables strategy to work is that observable pre-eruption features of the people and the houses in the destroyed and non-destroyed areas should be similar. Anecdotally, the Westman Islands is a small and relatively homogeneous community. Our discussions with locals who lived in the Westman Islands at the time indicate that the neighborhoods destroyed by the volcanic eruption were essentially similar to those that were not destroyed. In this section, we investigate these claims quantitatively.

Table 3 presents balance tests for various pre-eruption characteristics that are available in our data. While we have limited data on pre-treatment economic characteristics, importantly, we do have data on housing values prior to the eruption (from tax valuations). There are no systematic differences in values of houses between the destroyed and non-destroyed neighborhoods. As housing wealth is likely to be correlated both with total wealth and income, this test confirms the perceptions of the locals we have spoken with that the destroyed neighborhoods were neither richer nor poorer than neighborhoods that were not destroyed.

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

We have information on several pre-treatment demographic characteristics. Among those 25 years old and older at the time of the eruption, about half of the population was female, the average age was 46 years, 76% were married, 47% were born in the Westman Islands, they had on average 12 years of education, and had slightly less than 2 children on average. When we test for differences in these characteristics (as well as the rate of divorce and widowhood and the probability of moving houses after 1960), we find that in all cases the differences are small and statistically insignificant. The last row of Table 3 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

Table 3: 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)		(2,420)
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)
Nr. of children	0.14	-0.030	1.86	-0.018
		(0.018)		(0.077)
Widowed (%)	0.00	0.00	0.08	-0.01
		(0.00)		(0.01)
Divorced (%)	0.00	-0.00	0.03	-0.01
		(0.00)		(0.01)
Years of schooling	–	–	11.95	0.17
		–		(0.17)
Change house after 1960 (%)	0.61	-0.02	0.46	0.01
		(0.02)		(0.02)
Born in the Westman Islands (%)	0.78	0.05***	0.47	0.03
		(0.02)		(0.02)
Missing (%)	0.02	-0.01	0.12	0.02
		(0.01)		(0.02)
N	1,935		1,782	

Notes: Columns 1 and 3 report sample means by age at the time of the eruption. Columns 2 and 4 report results from a covariate balance test. We regress the variable in question on $Destroyed$ and report the coefficient and robust standard errors in parentheses. *Move house after 1960* is a dummy for having moved houses after 1960. *Missing* is a dummy for being missing from the outcome data in 1981. *Years of schooling* is based on educational attainment as of 2011. We only report a balance test on this variable for those 25 and older. The validity of this balance test relies on the assumption that this group has already completed their education by the time of the eruption. We verify this assumption by showing no significant effect on education for this group in Table 9. We do not, however, report a test of balance in years of schooling for the younger cohorts, who have not completed their education by the time of the eruption. *** p<0.01, ** p<0.05, * p<0.1

group). To assess whether these results indicate a true difference in the nature of the destroyed neighborhoods or random variation (one out of 20 tests being significant), we carried out two additional tests. We performed a test of the omnibus null hypothesis that *all* the balance test coefficients are zero and are not able to reject that hypothesis. We also used a Bonferroni adjustment to assess whether *any* of the coefficients are non-zero taking account of multiple hypothesis testing. We are not able to reject zero for any coefficients with this adjustment.

We should also note that, to the extent that the destroyed neighborhoods *were* different from the non-destroyed neighborhoods in ways that were correlated with long-term outcomes, one would expect these selection effects to run primarily through the *adults* who lived in the affected neighborhoods and only secondarily through their children. Yet our results illustrate a large, positive effect of moving as a result 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.

4.2 Propensity to Move

A second basic requirement for our IV strategy to work 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 4 reports estimates of the first-stage regression, described by equation (2), where $Moved_i$ is regressed on $Destroyed_i$ as well as controls. 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 percentage points for the overall population. There is some heterogeneity across the age groups. The effect is about 12 percentage points for those younger than 25, while it is roughly 20 percentage points for those 25 and older. The first-stage F-statistic ranges from 11 to 28.[8] Table A.1 in the appendix presents an analogous set of results for household heads and dependents. The results are extremely similar.

Table 4 also reports first stage estimates for the descendants. These estimates show that individuals that have parents that lived in houses destroyed by the eruption are about 6 percentage points less likely to live in the Westman Islands when they first appear in our administrative

[8] The largest share of movers from the Westman Islands move to the capital area (Reykjavik and suburbs). Among the treated compliers, 65% moved to the capital area while the remaining 35% moved to other places in Iceland, mostly to the largest towns outside the capital area located in Southern and Northern Iceland.

Table 4: First Stage Regressions

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

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

records. This difference is statistically significant at the 1% level with a first-stage F-statistic of 12.3.

5 Empirical Results

The main outcome variables we focus on are labor earnings and education. In this section, we consider the causal effect of moving on these outcomes using the empirical strategy outlined above. We then discuss evidence relating to compensating differentials and evidence pointing to a role for comparative advantage in explaining our earnings and education results.

5.1 Earnings Effects

Our measure of labor earnings includes wage income and proprietors' labor income, but excludes pension income, transfers, and capital income.[9] We have annual earnings data for the sample period 1981 to 2014. We restrict attention to earnings in years when individuals are "prime age," which we define as being between the ages of 25 and 64 years old. For ease of exposition, we first convert all monetary variables to 2014 prices using the Icelandic CPI and then convert them into US dollars (USD) using an exchange rate of 125 Icelandic króna (ISK) per USD. We present results with and without controls. The controls 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

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

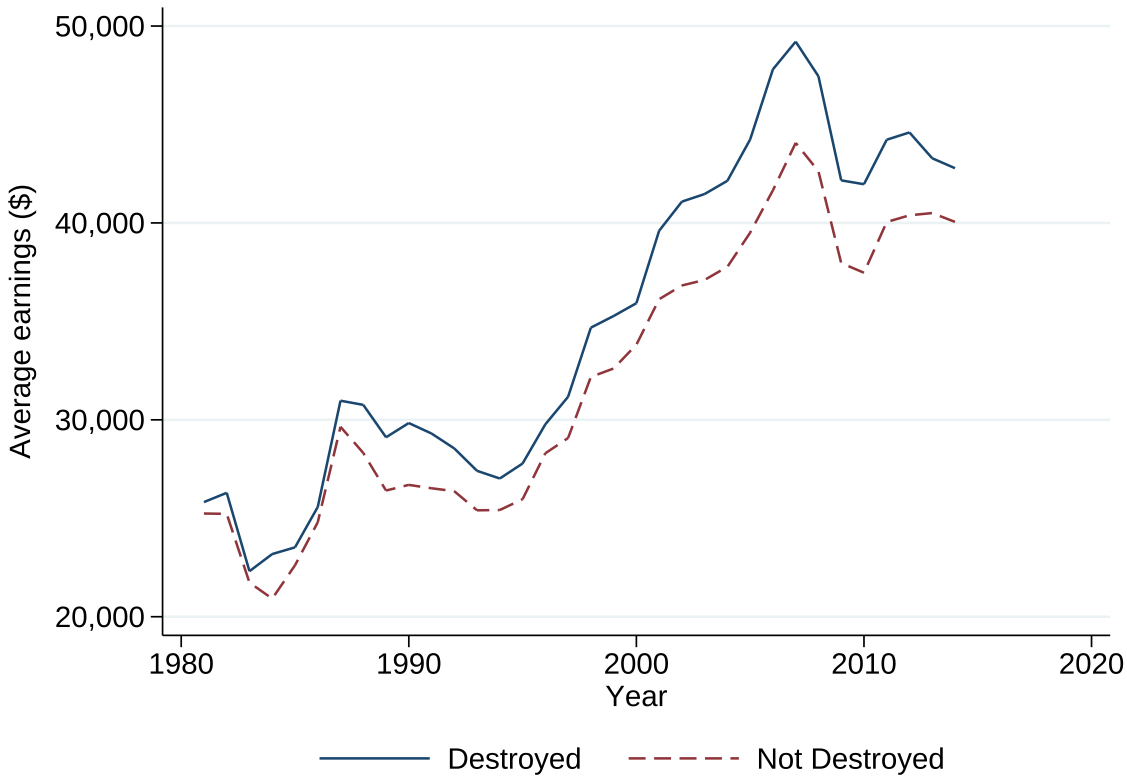

Figure 3: Earnings by Year – Cohorts Younger than 25 at time of Eruption

(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). When constructing standard errors, we cluster observations by address to allow for arbitrary correlation across time and across individuals that live at the same address at the time of the eruption.[10]

Figure 3 compares the average labor earnings by year of those whose houses were destroyed versus not destroyed, for individuals that were younger than 25 years old at the time of the eruption. The figure illustrates a remarkable reversal of fortune for these younger cohorts. The "bad luck" of having their houses destroyed in the 1973 eruption was associated with persistently higher average earnings over the next 35 years. It is worth noting that this difference in earnings does not seem to be driven by the financial boom that Iceland experienced between 2002 and 2008. The gap opens up long before this and persists after the financial crisis.

The first two columns of Table 5 report estimates of the reduced form effect of the instrument on earnings. Those who lived in houses that were destroyed on average earned roughly $3,400

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

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

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

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

more per year than those who lived in houses that were not destroyed, conditional on controls. This difference is statistically significant at the 1% level.

The third column in Table 5 reports a simple Wald estimate of the causal effect of moving on earnings. This estimate is constructed by dividing the difference in average earnings between the destroyed and non-destroyed samples (roughly $3,000 without controls) by the corresponding difference in the probability of moving (11.4 percentage points). This yields an estimate of the causal effect of moving of $26,628.

The fourth column of Table 5 reports a two-stage least squares (2SLS) estimate of equation (1). This estimate differs from the Wald estimate discussed above in that it includes a set of controls. The 2SLS estimate of the causal effect of moving is $27,532, which is equal to 83 percent of the average earnings of those whose houses were not destroyed. This estimate is more precise than the Wald estimate, making it statistically significant at the 5% level. As Figure 3 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.[11]

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

[11] An alternative specification is to use the log (as opposed to the level) of earnings as a dependent variable. Table A.2 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.2, this difference is driven partly by very large proportional increases for the lower tail of the earnings distribution.

Table 6: Effect on Earnings – Dependents

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

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

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 move if and only if their house is destroyed). This finding seems natural in light of the fact that the Westman Islands is a relatively affluent place in Iceland. People moving away from the Westman Islands are likely to do so because of adverse events such as job loss that signal weak unobserved characteristics.[12]

Table 6 presents our estimates of the earnings effects for dependents as opposed to cohorts younger than 25 at the time of the eruption. As we show in Table 2, the sample of individuals younger than 25 at the time of the eruption is extremely similar to the sample of "dependents." Indeed, all of the dependents in our sample are less than 25 (the only difference in the definitions is that 9% of household heads are younger than 25.) It is not, therefore, surprising that the results in Table 6 are very similar to those in Table 5. This analysis shows directly that it is the dependents (mostly children) that benefit from being induced to move by having their house destroyed.[13]

The average treatment effects we estimate in Tables 5 and 6 are very large. Do these large average treatment effects 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 for individuals younger than 25 at the time of the eruption

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

[13] Estimates of the first state regression for dependents are reports in Table A.1 in the appendix.

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 4 plots the resulting quantile treatment effects. We find that the treatment effect for the median and for all quantiles between the 15th percentile and the 85th percentile are roughly $20,000, which is roughly 60 percent of the average earnings of those whose houses were not destroyed. 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.[14] Figure A.2 in the appendix plots quantile treatment effects when the logarithm of earnings is the dependent variable. When viewed in proportional terms, it is the lower tail of the distribution of earning that moves the most. However, movements at the top of the distribution are also substantial at roughly 100 percent (0.7 log points).

Figure 5 plots the raw data on average earnings by age separately for those whose houses were and were not destroyed in the eruption. This figure shows how the earnings effects of the lava shock differs over the life-cycle. This simple comparison indicates negative earnings effects early in adulthood—from ages 18 to roughly 25. This likely reflects the fact that those whose houses were destroyed attend school for longer (see section 5.2). 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 specification we use for this and Panel B of Figure A.3 presents the earnings effects by age. The resulting estimates start off small and grow at least until age 50. At age 50, they are estimated to be roughly $50,000.[15] 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.[16]

The large positive causal effects of moving we estimate for those younger than 25 years old

[14]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.

[15]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).

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

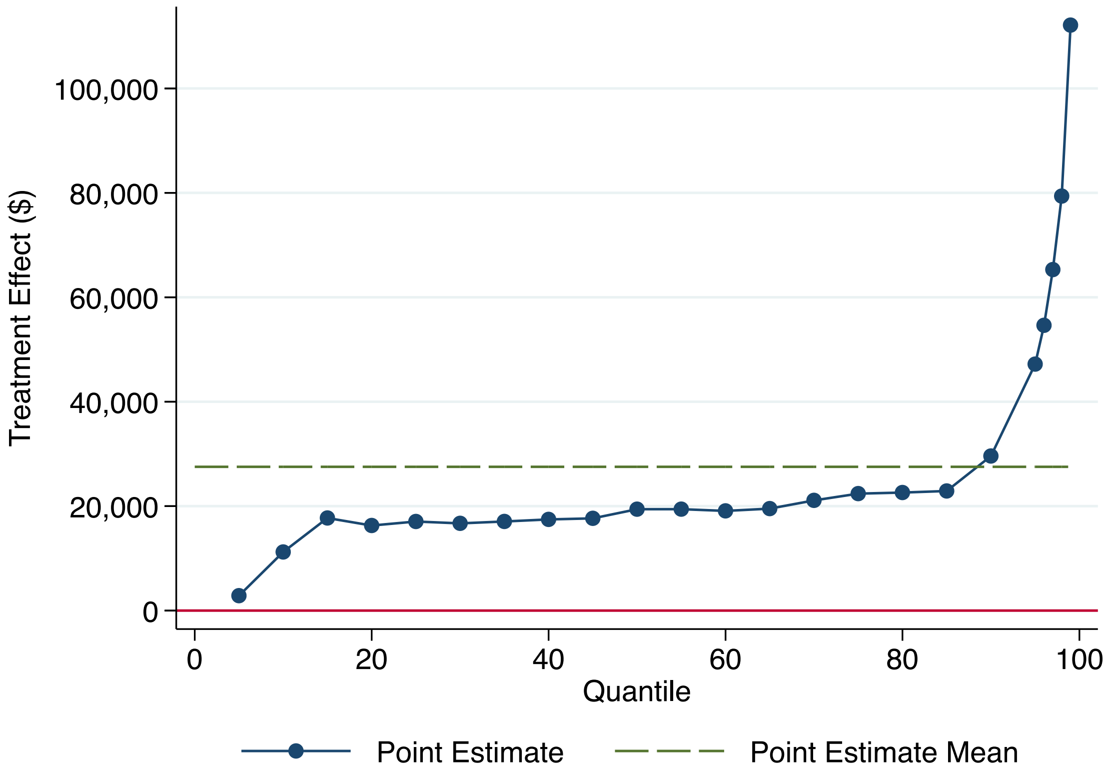

Figure 4: Quantile Treatment Effects on Earnings – Cohorts Younger than 25 at time of Eruption

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.

at the time of the eruption and for dependents contrast sharply with our estimates of the causal effects of moving for those 25 years old and older and for household heads. Table 7 presents results for those 25 years old and older. For these cohorts, we estimate the causal effect of moving to be a small negative number that is not statistically significantly different from zero. Table 8 presents an analogous set of results for household heads. Again, the causal effect of moving is estimated to be a small negative number that is not statistically significantly different from zero. This analysis—combined with the analysis above for dependents—shows directly 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.3 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

Table 7: Effects of Moving on Earnings – Cohorts 25 and Older at Time of Eruption

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

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

Table 8: Effect on Earnings – Household Heads

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

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

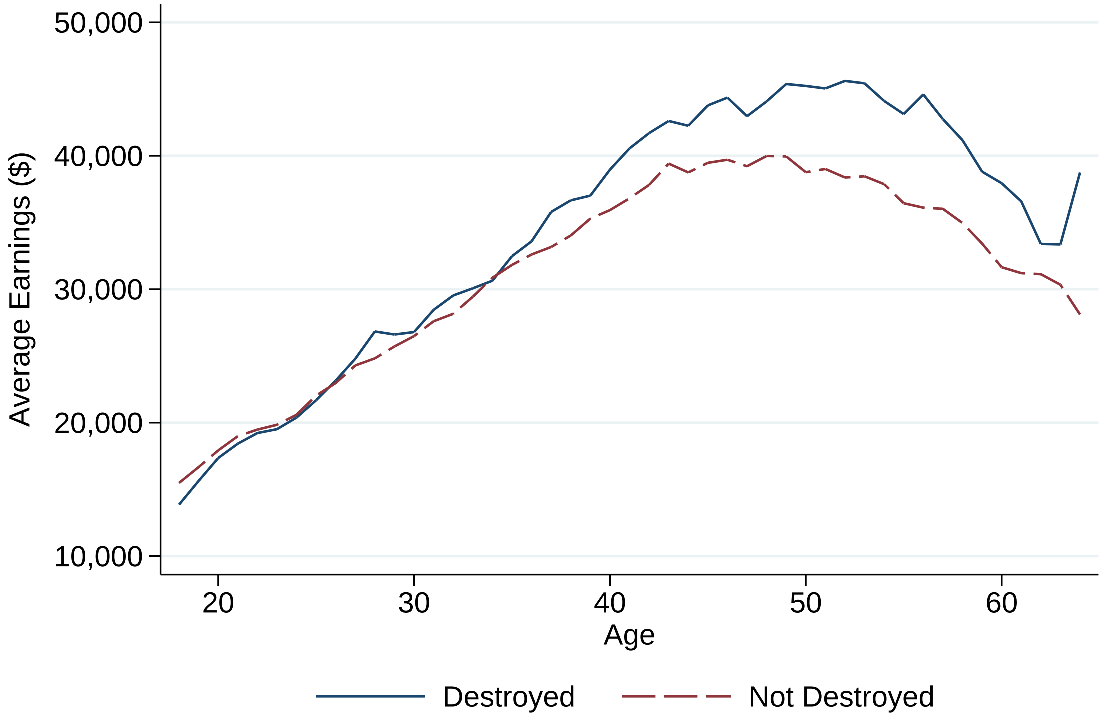

Figure 5: Earnings Effect Over the Life Cycle – Cohorts Younger than 25 at time of Eruption

effect of the descendant group will be possible after a decade or two.

Our result that the young disproportionately benefit from moving is consistent with recent work by Chetty, Hendren, and Katz (2016), Chetty and Hendren (2018) and Chyn (2018) in other settings. Chetty and Hendren (2018) 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 6 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.[17] Our results, therefore, suggest that the crucial distinction is whether individuals had finished their education and settled on a career at the time of the eruption. Those young enough to make changes to the educational choice and shift careers were better able to take advantage of the "opportunity" the lava shock presented them.[18]

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

[18]Our results also differ from those of Chetty, Hendren, and Katz (2016), who find positive effects only for children

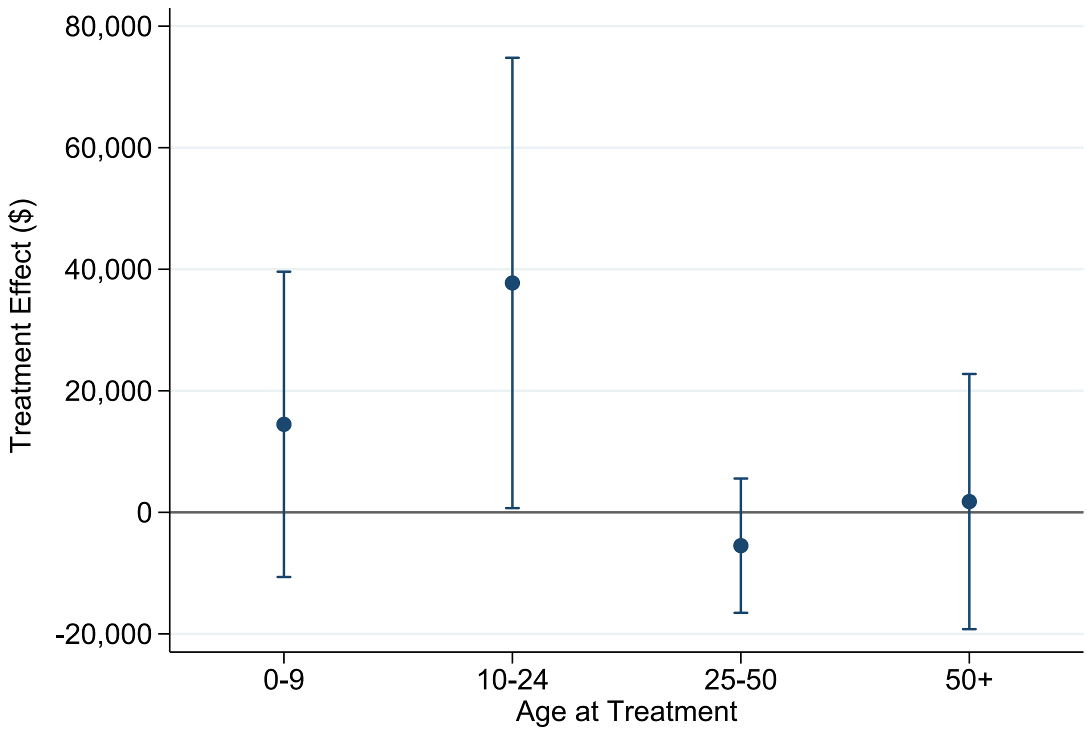

Figure 6: IV Earnings Effect – Four Age Groups

5.2 Education Effects

We next estimate the causal effect of moving on educational attainment. Table 9 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. The table presents both OLS and IV estimates. 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.

The causal effect of moving on education for those younger than 25 was 3.5 years. To interpret this large estimate, it is useful to understand the structure of the Icelandic educational system. Iceland has 10 years of compulsory schooling from ages 6 to 16. The next stage in the Icelandic educational system is a four-year junior college degree (usually done from ages 16 to 20). Junior college has traditional academic tracks required for university enrollment, as well as vocational tracks such as carpentry and hairdressing. Table 10 shows that the causal effect of moving on the

who are younger than 13 at the time they move.

Table 9: Effect of Moving on Years of Schooling

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

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

probability of getting a junior college degree was 63 percentage points, and the causal effect of moving on getting a university degree was 23 percentage points (not statistically significant). The causal effect of moving on education presented in Table 9 therefore mostly reflects a large increase in the rate of attending junior college.

Table 10: Effects on Post-Compulsory Education
Cohorts Younger than 25 at Time of Eruption

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

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

Our estimate of the causal effect of moving on the educational attainment of the descendants of those living in the Westman Islands at the time of the eruption is even larger than on the inhabitants themselves. We estimate the causal effect on the descendants to be 5.7 years of extra schooling. This estimate, though large, may be somewhat downward biased. The youngest cohort in the descendant group was only 15 years old in 2011 (the year for which we have data on educational attainment). Many individuals in the youngest cohorts of the descendant group had

Table 11: Effect of Moving on Years of Schooling

	Dependents		Household Heads	
	IV	OLS	IV	OLS
	(1)	(2)	(3)	(4)
$Moved$	3.56*	0.14	1.16	0.11
	(1.86)	(0.17)	(0.77)	(0.15)
Control group mean	13.52	13.52	11.99	11.94
Controls	Yes	Yes	Yes	Yes
N	2,071	2,071	1,292	1,292

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

therefore not yet finished their educational attainment.

In contrast, the causal effect of moving 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 be reflecting this channel.

Table 11 reports estimates of the causal effect of moving on education for household heads and dependents. As with our results for earnings, the results based on this decomposition are very similar to the results for the age-based grouping discussed above: The education effects are large for dependents, but modest for heads of households.

5.3 Moving Away from Opportunity?

The large causal effects of moving on earnings and education that we document above cannot be explained simply by low returns to fishing in the Westman Islands during our sample period. To the contrary, fishing has been highly profitable in Iceland and the Westman Islands has been a very high income place over our sample period. Figure 7 shows that average earnings in the Westman Islands have been substantially higher than in Iceland's capital area (Reykjavik and suburbs) except for a few years during the financial boom prior to 2008. In contrast to much prior work, our setting is, thus, one in which people gain a great deal from moving away from a high income location to locations with lower average income. This raises the question: how can it possibly be

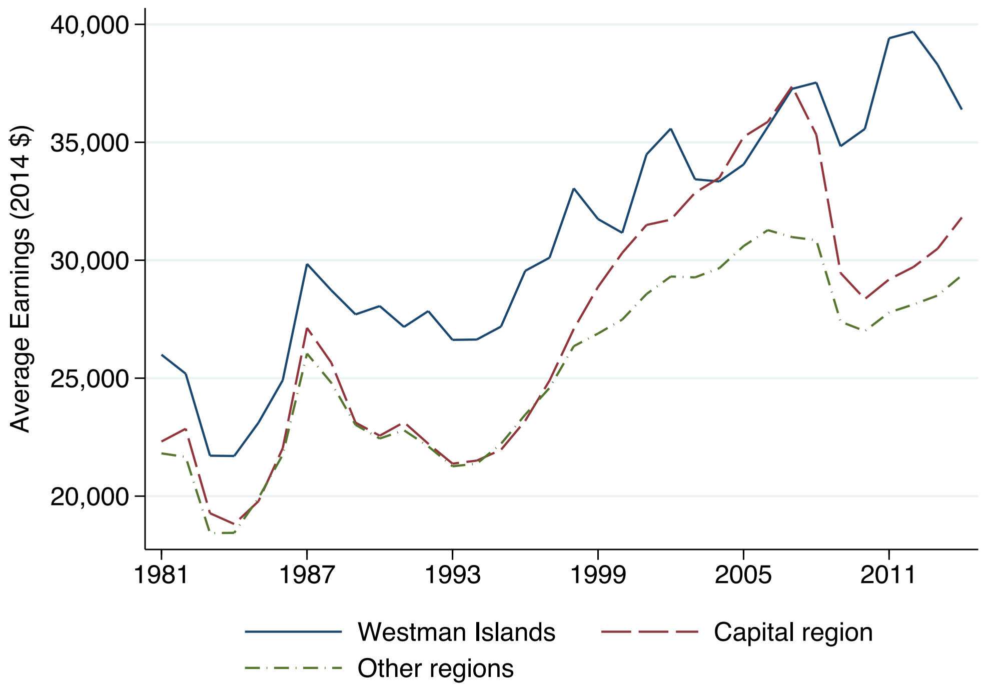

Figure 7: Evolution of Average Earnings Across Locations

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

so beneficial to move away from the Westman Islands? This question is a primary focus of our theoretical analysis below.[19]

We believe that the most compelling interpretation of the facts we have presented so far—large causal effects of moving away from a high income place for young dependents—is one of comparative advantage. The highly specialized nature of the labor market in the Westman Islands likely means that it is not an ideal place for certain workers—i.e., those whose comparative advantage lies in jobs requiring a large amount of education such as law, computer science, engineering, or medicine. At the same time, there are other workers for which the Westman Islands with its high paying fishing jobs is an ideal place to work—i.e., those whose comparative advantage lies in skills valued in the fishing industry. We show in the next section that a model featuring heterogeneous comparative advantage provides a natural explanation for the otherwise rather puzzling

[19]One concern is that the large causal effect of moving for the compliers may have been an ex post fluke due to aggregate shocks after the eruption rather than something that could have been rationally anticipated at the time of the eruption (Rosenzweig and Udry, 2020). This concern is difficult to rule out completely. However, Figure 3 shows that the relative labor market outcomes for treated and untreated individuals in our sample are quite stable over our 34 year sample period. If this is representative of the statistical process of returns more generally, then a difference between ex-ante and ex-post returns is unlikely. Only a large and extremely persistent shock could result in such a difference, but the stability of returns over our sample period implies that such shocks are uncommon.

Table 12: 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.

combination of facts we have documented.

In this subsection, we support this view by presenting evidence indicating that the Westman Islands is a place that specializes in occupations for which the value of education is low, and is therefore a poor match for people with a comparative advantage in "brainy" occupations. The dominant industries in the Westman Islands are fishing and fish processing. These two industries alone account for roughly 70% of income in the Westman Islands, relative to less than 15% in Iceland as a whole.[20] While the fishing industry pays high wages, it requires little formal education. One sign of this is that educational attainment in the Westman Islands is low. Table 12 reports educational attainment in the Westman Islands, Iceland's capital area, and other areas in Iceland. Educational attainment is substantially lower in the Westman Islands than in Reykjavik. Only 20% of the working age population has a university degree, compared to 40% in the Capital Region.

Another sign that comparative advantage for "brainy" occupations is an important factor in our results derives from our analysis of the characteristics of the compliers in our natural experiment—i.e., those that move if and only if their house is destroyed. Although individual compliers cannot be identified in the data, their average characteristics can be estimated when the instrumental variable is binary (Angrist, 2004). The basic intuition is that we can uncover the statistical characteristics of the "always-takers" (those who move no matter what) and "never-takers" (those who stay no matter what) in our data by looking at those whose houses were destroyed and did not move (never-takers) and those whose houses were not destroyed and moved anyway (always-takers). The statistical characteristics of the compliers can then be inferred by comparing these groups to the whole sample and making use of Bayes rule.[21]

Table 13 reports the frequency of a set of characteristics among the cohorts that were younger than 25 years old at the time of the eruption. We report the frequency within this entire group

[20]See Table A.6 in the appendix for further details. These statistics combine "Fishing and Agriculture" and "Fish and Food Processing". However, since there is virtually no agriculture in the Westman Islands, the true extent of specialization is even greater than what the statistics suggest.

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

Table 13: Complier characteristics ratios – Cohorts Younger than 25 at Time of Eruption

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

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

(column 1), among the compliers in this group (column 2), and the ratio of these frequencies (column 3). What stands out is that the compliers are roughly 50% more likely to have parents that had post-compulsory education than the typical Westman Islander.

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

5.4 Compensating Differentials

Are the greater earnings obtained by those who move away from the Westman Islands compensation for non-pecuniary costs? One potential source of non-pecuniary benefits of living in the Westman Islands 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. Perhaps those who grew up living in a small fishing town have become accustomed to that lifestyle, and prefer it to other alternatives.

However, if compensating differentials associated with culture were behind our effects, one would expect them to be larger for the parents—who have spent longer in the Westman Islands—than for their children who are only starting off in life, and even smaller for descendants born outside of the Westman Islands. The time pattern of earnings effects is exactly the opposite, as it appears 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.

We can directly rule out non-pecuniary benefits of living in the Westman Islands associated with differences in prices. In Appendix F, we document empirically that the prices of food and beverages are higher in the Westman Islands than in most other regions in Iceland. The higher cost-of-living for food likely arises from a combination of high transportation costs and markups, as we discuss in the appendix. Product availability is also clearly much more limited in the Westman Islands than in many other places in Iceland, the Capital Region in particular. We document that Westman Islanders have traditionally bought the majority of their food locally. In addition, we have been able to verify, based on our own observations, that the price of electricity and hot water for house heating are higher in the Westman Islands than in Reykjavik.

To provide additional insights into the more holistic effect of moving on quality of life, we also study its effects on a variety of non-monetary outcomes. Table 14 reports results for a variety of outcomes, aside from earnings, for those less than 25 years of age at the time of the eruption. The causal effect of moving on these outcomes are imprecisely estimated. But the point estimates suggest that those induced to move by the eruption are both less likely to die before the age of 50 and less likely to receive pension payments. Since the young cohorts do not reach the retirement age of 67 during the sample period, pension payments relate to illness, disability, or a deceased spouse or parent.[22] The point estimates also suggest that those induced to move are more likely to get married and have more children. Effects for the older cohorts are qualitatively similar, though they are smaller and apart from being less likely to die before the age of 50, none of the coefficients are statistically significant (see Table A.4 in the appendix). None of these estimates are consistent with non-pecuniary costs of moving, according to conventional views on the consequences of these factors for happiness.

[22]One might wonder whether the treatment effect on income is, to some extent, driven by the lower propensity of the treatment group to retire early. To investigate this, we reran our empirical analysis setting the earnings observations to missing for all years when individuals are receiving a pension. Table A.5 in the appendix presents results from this case. This approach yields a treatment effect of $24,300, and is highly statistically significant (compared to $27,500 for our baseline specification). Hence, early pensions do not appear to be driving our main results.

Table 14: Other Outcomes – Cohorts Younger than 25 at Time of Eruption

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

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

6 A Model of Comparative Advantage

We next present a Roy model with heterogeneous comparative advantage, moving costs, and overlapping generations to help interpret the empirical results we have presented so far. The model we develop is based on the models in Lagakos and Waugh (2013), Young (2013), Adao (2015) and Bryan and Morten (2019). Our model generalizes Adao's model to include moving costs, an educational choice, and overlapping generations, while simplifying it along several other dimensions.

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

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

Parents inelastically supply one unit of labor to market work. They are endowed with a bivariate skill vector $(z^p_F(i), z^p_N(i))$, where $z^p_k(i)$ is the number of efficiency units of labor that parents from family i produce if employed in sector k. It is convenient to define parent i's comparative advantage in the non-fishing sector to be

$$s^p(i) \equiv \log(z^p_N(i)/z^p_F(i))$$

and her absolute advantage to be

$$a^p(i) \equiv \log z^p_F(i).$$

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

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

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

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

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

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

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

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

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

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

We assume that the returns to education on the mainland are sufficiently high that parents choose to educate their children if they move to the mainland.[23] We denote the cost of education by f. As with moving costs, the form that the education cost takes is that parents lose a fraction $1 - \exp(-f)$ of their income if they educate their children. Being educated increases childrens' non-fishing income by a factor $\exp(\phi(i))$. The benefits of education may also differ across households, i.e., some households may have a comparative advantage when it comes to making use of education. Education is not useful in the fishing industry. In our model, parents that stay in the Westman Islands, therefore, do not educate their children.

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

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

where

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

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

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

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

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

Using the definitions of comparative advantage and absolute advantage, we can write the

[23]Specifically, we make the simplifying assumption that the cut-off for choosing to educate a family's children is below the cut-off for choosing to moving to the city. For this reason, all people that move to the city decide to educate their children. We have also analyzed the case where the cut-off for education lies above the cut-off for moving to the city. This leads to a more complicated model, but does not change any of the key insights. The main change is simply that this leads to three (as opposed to two) regions in the type space, because there are some individuals who choose to move, but do not educate their children.

logarithm of labor income of parents and children in family i as

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

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

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

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

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

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

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

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

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

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

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

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

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

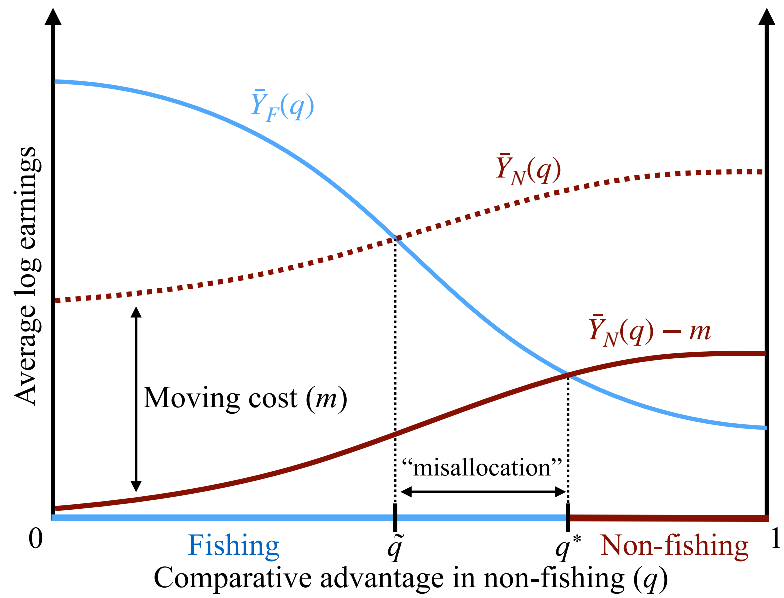

Figure 8: Sorting by Comparative Advantage

6.1 Simple Version Explained Visually

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

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

respectively. Figure 8 illustrates the economics of the model visually. If a worker chooses to work in the fishing sector, she will on average earn $\bar{Y}_F(q)$ (the light blue line). This will also be her consumption. If she chooses to work in the non-fishing sector, she will earn $\bar{Y}_N(q)$ (the dashed dark red line). In this case, however, she will need to move away from the Westman Islands, which is costly. Taking account of these moving costs, her level of consumption will on average be $\bar{Y}_N(q) - m$ (the solid dark red line).

We have drawn Figure 8 with $\bar{Y}_F(q)$ downward sloping and $\bar{Y}_N(q)$ upward sloping. This means that workers that have a comparative advantage in fishing (i.e., low q workers) are more productive at fishing than those that have a comparative advantage at non-fishing and vice versa. While this may seem like a natural case, the theory we have laid out can accommodate cases in which both $\bar{Y}_F(q)$ and $\bar{Y}_N(q)$ are upward sloping (those with a comparative advantage at non-fishing are also better at fishing) and cases in which both $\bar{Y}_F(q)$ and $\bar{Y}_N(q)$ are downward sloping (those with a comparative advantage at fishing are also better at non-fishing). All that we assume is that $\bar{Y}_N(q)$ has a larger slope than $\bar{Y}_F(q)$ (i.e., workers differ in their comparative advantage).

In equilibrium, workers will self-select into the sector in which they earn the most net of moving costs. Figure 8 shows that this will give rise to a unique cutoff quantile q^* below which all workers choose to be fishermen and above which all workers choose to move away from the Westman Islands and take up employment in the non-fishing sector.

Figure 8 also shows clearly how the moving cost leads to misallocation of labor. If moving were not costly, workers at quantile q would choose between $\bar{Y}_F(q)$ and $\bar{Y}_N(q)$ rather than $\bar{Y}_F(q)$ and $\bar{Y}_N(q) - m$. In this case, a larger fraction of workers would move away from the Westman Islands (and presumably a larger fraction of mainland workers would also move to the Westman Islands). The cutoff quantile in this no-moving-cost case would be $\tilde{q}$. The moving cost implies that workers between $\tilde{q}$ and q^* are misallocated and are earning less than they would without the moving cost.

6.2 Model-Based Interpretation of the Volcanic Experiment

Let's now consider the situation at the time of the eruption in the full model with parents and children. Our empirical results in sections 4.1 and 4.2 suggest that at the time of the eruption a fraction of families in the Westman Islands exogenously faced lower barriers to moving than other families because their houses were destroyed in the eruption. We therefore consider a situation where a fraction of families (those whose houses were destroyed) face a moving cost of m', while other families face a moving cost of $m > m'$.

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

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

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

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

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

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

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

This situation is depicted in Figure 9. As before, the downward-sloping light-blue line depicts average earnings of fishermen as a function of comparative advantage in non-fishing $\bar{Y}_F(q)$ (same for both generations). The two solid dark-red lines depict average earning on the mainland for parents and children. The gap between the two lines represents the gains from education in non-fishing. We have drawn the figure for the case where those with a comparative advantage in non-fishing also gain (slightly) more from being educated (the $\bar{Y}_N^c(q)$ line is (slightly) steeper than the $\bar{Y}_N^p(q)$ line). The equation for the cutoff points $q^{*\prime}$ and q^* can be rewritten as

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

where

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

The two dashed red lines in Figure 9 plot the left-hand side of equation (5) for the two values of moving costs m' and m. The points where these lines cross the $\bar{Y}_F(q)$ line are the two cutoff quantiles $q^{*\prime}$ and q^*.

6.3 Barriers to Moving

Equation (6) demonstrates how several features that we have included in the model constitute barriers to moving. One such feature is imperfect altruism by parents towards their children. The level of altruism of parents towards their children is captured by β in our model. If β is small, parents will place low value on their childrens' gains from education ($\phi(q)$) and consequently be less inclined to move.

Another factor is imperfect information about the returns to moving. The decision to move will depend on perceived returns rather than actual returns. In our model this means the perceived gains from eduction (perceived $\phi(q)$) and perceived earnings in the non-fishing sector (perceived $\bar{Y}_N^p(q)$). If the parents are pessimistic about either of these factors, this will hinder mobility in

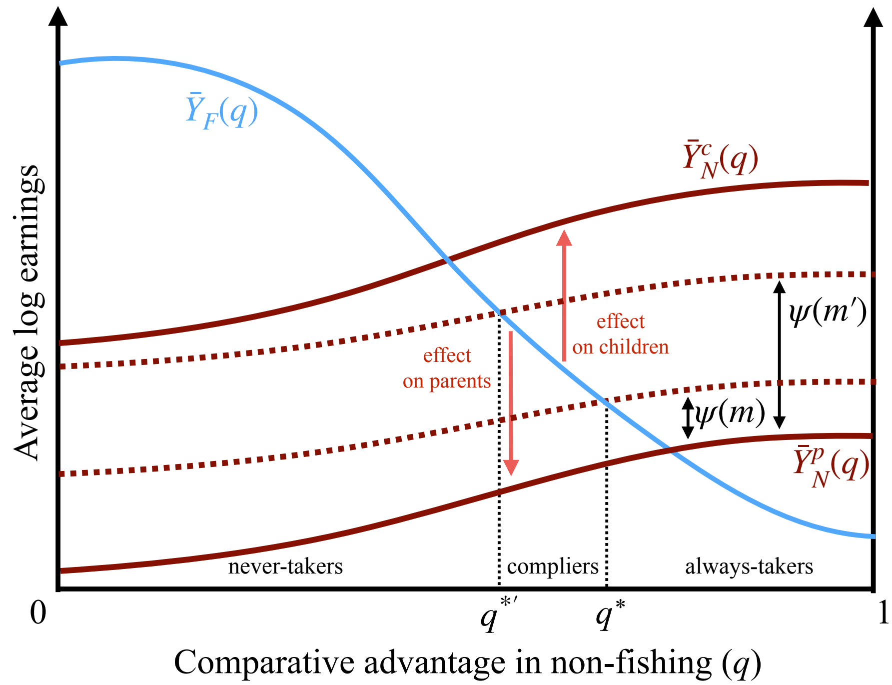

Figure 9: The Volcanic Experiment

the same way as traditional moving costs. This type of friction has been emphasized in, e.g., the context of returns to education (Manski, 1993). In settings where education and income are low, perceived returns to education are much smaller than actual returns (Jensen, 2010).

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

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

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

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

7 Interpreting our Empirical Estimates

It is now straightforward to show how the model we present in the preceding section can explain the various empirical facts we have documented earlier in the paper. For this purpose, it is useful to begin by dividing the workers into three groups based on the terminology of Angrist (2004). Workers to the left of $q^{*\prime}$ in Figure 9 are "never-takers." These workers have such a strong comparative advantage in fishing that they don't move even if their house is destroyed. Workers between $q^{*\prime}$ and q^* are "compliers." These are the workers that move if and only if their house is destroyed. Finally, workers to the right of q^* are "always-takers." These workers have such a strong comparative disadvantage in fishing that they move even if their house is not destroyed.

Our IV estimates reflect the causal effect of moving on the compliers, i.e. the local average treatment effect for this group (Imbens and Angrist, 1994). Let's start by considering the children. In Figure 9, the causal effect of moving for children at a given level of comparative advantage q is the vertical distance between $\bar{Y}_N^c(q)$ and $\bar{Y}_F(q)$. The figure shows clearly that in our setting the complier children are highly selected to have a large causal effect of moving. This fact helps explain the large magnitude of the causal earnings effects we estimate. Intuitively, the complier children are relatively poorly suited to live in the Westman Islands. This is why their parents can be induced to move away and also why they themselves gain so much from moving.

We can also read the causal effect on the parents — the vertical distance between $\bar{Y}_N^p(q)$ and $\bar{Y}_F(q)$ — off of Figure 9. It is much smaller than the causal effect on the children. The reason for this is that the parents are not educated and therefore benefit less from moving to the mainland. We have drawn Figure 9 such that the causal effect on the complier parents is negative as in our empirical estimates. Whether this effect is positive or negative depends on the sign of $\Psi(m(i), q)$. Equation (6) reveals that this depends on the parents' level of altruism toward their children β and the size of the education effect $\phi(q)$. With a large degree of altruism and a large education effect, parents will be induced to move even if the effect on their own earning is negative because the large effect on their children's earnings outweighs their own losses.

It is evident from Figure 9 that the causal effect of moving in our model is highly heterogeneous depending on comparative advantage. In particular, the causal effect on the never-takers is smaller than the causal effect on compliers and can easily be negative even for the children. In Figure 9, the causal effect on most never-taker children is negative (all of those to the left of the point where the $\bar{Y}_F(q)$ line crosses the $\bar{Y}^c_N(q)$ line). These families have a strong comparative advantage in the fishing sector. They would be made worse off if they had to move to the non-fishing sector even if there were no direct moving cost. Our model therefore has the property that even though some can be made much better off by moving, this is not true of all. A policy of moving everyone away from the Westman Islands may be a terrible policy even despite our large positive IV estimates because there are these other groups that are well matched to the Westman Islands and would be made worse off by having to move.

Figure 9 provides a natural explanation for the "puzzle" we posed earlier: how can it be that the causal effect of moving is so positive even though people are moving away from a high income location? We have drawn the figure such that the average income across never-takers is high (higher than the average income of compliers and always-takers). This reflects the fact that fishing is very profitable in Iceland, and those with a comparative advantage in this sector therefore earn high income on average (higher than the average of those with a comparative advantage in other sectors). This is in no way inconsistent with the notion that the causal effect of moving away from fishing can be very high for those not well suited to work in this sector. Hence, even though the causal effect on the complier children is large and positive, average income can easily be higher for those who remain in the Westman Islands (a weighted average of $\bar{Y}_F(q)$ for the never-takers and non-treated compliers) than those who move away (a weighted average of $\bar{Y}^c_N(q)$ and $\bar{Y}^p_N(q)$ for the always-takers and treated compliers).

This logic also provides a simple explanation for why the OLS estimate of income on moving is so much lower than the IV estimate for the young in our setting. The OLS estimator compares the income of all of those that move with all of those that stay. The stayers are the never-takers and the non-treated compliers, while the movers are the always-takers and the treated compliers. The OLS estimate therefore takes a difference between the average of $\bar{Y}^c_N(q)$ from $q^{*\prime}$ to 1 and $\bar{Y}_F(q)$ from 0 to q^*.[24] This can easily be negative for both the parents and the children even though the causal effect on the complier children is always large and positive.[25]

[24] Remember that there are two types of households at each value of q in Figure 9: those whose house was destroyed and those whose house was spared. In the complier region, those whose house was destroyed move, while those whose house was spared do not.

[25] It is important to recognize that while we estimate large causal effects of moving on income (present value of

7.1 Returns to Education

We can also consider how our empirical estimates relate to the literature on returns to education. Empirical work on the returns to education suggests that an additional year of schooling raises income by roughly 10% (Card, 2001). This corresponds approximately to what one obtains by comparing average incomes across educational groups in Iceland. During the period 2004-2014, the annual earnings premium for workers with junior college degrees in Iceland versus those with only compulsory education was 36%. This suggests a 9% return per additional year of schooling in Iceland (36% / 4 years). In comparison, our average estimated earnings effect is 83%, and our average estimated effect on educational attainment is a 3.6 year increase in schooling. A naive interpretation of these facts is that our estimates imply a 23% return (0.83/3.6) to each additional year of schooling — much larger than the 10% return suggested by the returns to education literature.

The model we present above provides a simple interpretation of this discrepancy. In the model, the large apparent returns to education arise partly from comparative advantage: those induced to move have a comparative advantage at non-fishing. Our model implies, moreover, that there is an important interaction between location and increased educational attainment. Additional years of schooling are much more valuable when the individuals can relocate to where the education is most valuable. The returns to additional years of education are smaller in the more standard case where the individual still faces large barriers to moving and is therefore only able to use his or her additional education in his or her original location.

7.2 A Model of Absolute Advantage

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

$$y_{ij} = a_i + b_j + \varepsilon_{ij}. \tag{8}$$

roughly $444,000), this does not provide an estimate of the *difference* in moving costs between those whose houses were destroyed and those whose houses were not destroyed. This difference — i.e., the reduction in moving costs resulting from one's house being destroyed — is potentially much smaller. Figure 9 illustrates this point clearly. The reduction in moving costs resulting from one's house being destroyed is equal to the vertical distance between the two dashed red lines in Figure 9. The size of this difference determines the size of the complier group. It is, however, not related to the size of the causal effect on the children, which is equal to the distance between the light-blue line and the top solid red line.

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

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

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

While this alternative explanation is logically consistent, we do not view it as particularly plausible. One reason for this is that standard measures of human capital accumulation do not support this view. Educational attainment is low in the Westman Islands (Table 12). Students from the Westman Islands also perform poorly on standardized tests relative to their peers elsewhere in Iceland: The average test score for the Westman Islands ranks towards the bottom of the distribution of average test scores across schools in Iceland in all subjects (see Figure A.6 in the appendix for details). Of course, Westman Islanders may be particularly well-endowed in the human capital needed to carry out the specific tasks that are done on the Westman Islands. But that suggests the model of comparative advantage we present in section 6.

To gain a further understanding of what an absolute advantage interpretation of our facts entails, Figure 10 provides a graphical depiction of the AKM model analogous to Figure 8. In this case, workers are ranked on the horizontal axis by absolute advantage as opposed to comparative advantage (i.e., q denotes absolute advantage). Workers further to the right in the figure have higher absolute advantage and are therefore better at both tasks. This is reflected in the fact that both the $\bar{Y}_F(q)$ curve and the $\bar{Y}_N(q)$ curve are upward sloping. Since there is no comparative advantage, these two curves are parallel. We have drawn the figure such that the causal effect of moving is positive ($\bar{Y}_N(q) > \bar{Y}_F(q)$). We have also drawn a third curve in the figure representing

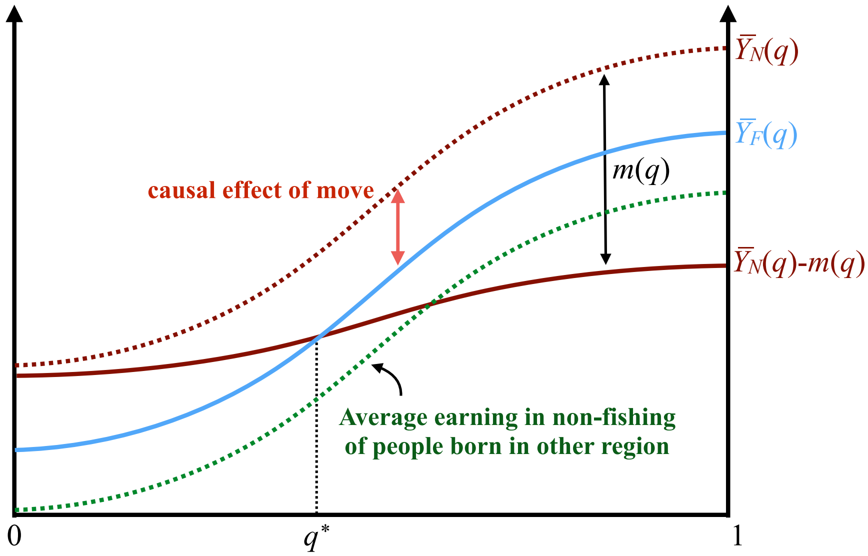

Figure 10: A Model of Absolute Advantage

the average earning in non-fishing of those living in other regions at the time of the eruption (the green dotted curve). This curve is below the $\bar{Y}_N(q)$ curve reflecting the positive worker effects of the Westman Islanders relative to people elsewhere in Iceland needed to explain lower average income in the rest of Iceland than in the Westman Islands. Finally, in this model, it is not heterogeneity in the causal effect of moving that determines who moves (since this is constant). A simple idea is that there is heterogeneity in moving costs. The final curve in the Figure 10 plots earnings of Westman Islanders in non-fishing net of a heterogeneous moving cost ($\bar{Y}_N(q) - m(q)$). We have drawn this curve such that the moving cost is smaller for people with low absolute advantage. In this case, it will be low absolute advantage people that move. This assumption is needed for the AKM interpretation to be able to explain the low OLS estimate of income on moving we obtain. However, recent empirical evidence suggests that, in fact, low-skilled people are less mobile than high-skilled people (Notowidigdo, 2019).

8 Conclusion

We exploit a mobility shock generated by a destructive volcanic eruption—a true natural experiment—to estimate the causal effect of moving on economic and educational outcomes. For the young dependents in our sample (i.e. the children), we estimate a causal effect of moving on annual earnings of roughly 83%. The earnings effect increased gradually over people's working life and peaked during prime age. Moreover, these young movers got 3.5 more years of schooling than they otherwise would have, and, as a result of the mobility shock, their children (the descendants of the originally affected population) got 5.7 more years of schooling.

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

A unique feature of our environment, moreover, is that the workers in our study are moving *away* from opportunity, at least from the perspective of average income. This suggests that our results should not be interpreted as the returns to escaping a "bad" location. Instead, we interpret our results as evidence of the importance of comparative advantage. The location we study is, like many small towns, specialized in a particular industry that is unlikely to be the ideal match for everyone. Our findings underscore the potential for geographical misallocation of labor even when differences in average incomes across locations are small.

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Appendix

A Constructing Years of Schooling

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

B Earnings Effect over Subsamples

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

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

where the variable $period_t$ represents an indicator variable for each non-consecutive 5-year period in sample period of 1981-2014 (i.e., 1981-1985, 1986-1990, ... 2011-2014). The endogenous regressors $Moved_i \times period_t$ are instrumented using interactions of the 5-year period dummies with the instrument $Destroyed_i$. The β_t estimates from this regression are plotted in Figure A.4. The figure shows that the effect of moving is positive throughout the sample period and does not appear to have a systematic relationship with the business cycle. In particular, it is high both before and after the financial crisis.

C Earnings Effects over the Life-Cycle

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

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

where the variable age_τ represents an indicator variable for each 2-year age group from age 18 to 63 (i.e., 18-19, 20-21, ..., 62-63). We include a full set of 2-year age fixed effects, time fixed effects and the same demographic controls as in our main specifications. Panel A of Figure A.3 plots the β_τ coefficients from this specification. These results are slightly different from what one might expect from Figure 3. The difference arises because of the inclusion of the controls.

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

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

and instrument for the endogenous regressors $Moved_i \times age_\tau$ with $Destroyed_i \times age_\tau$. Panel B of Figure A.3 plots the β_τ coefficients from this specification.

D Spatial Correlation

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

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

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

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

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

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

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

Moran's I is analogous to an autocorrelation coefficient, but measures correlations over space (in two dimensions) rather than over time. If adjacent houses tend systematically to have more similar house prices than houses that are further away from each other, this will tend to raise the value of Moran's I. Values of Moran's I close to 1 suggest strong positive spatial correlation, while values close to -1 suggest strong negative spatial correlation. Moran's I is more sensitive to "global" spatial correlation than Geary's C, since the building blocks involve differences versus the overall mean, as opposed to immediately surrounding houses.

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

To aid interpretation of Moran's I, Figure D.1 plots a "Moran's I scatter plot." This figure plots the price of each house (on the x-axis) against its "spatial lag." The spatial lag is a "synthetic neighbor," defined as the weighted average of the value of all other houses in the town, weighted

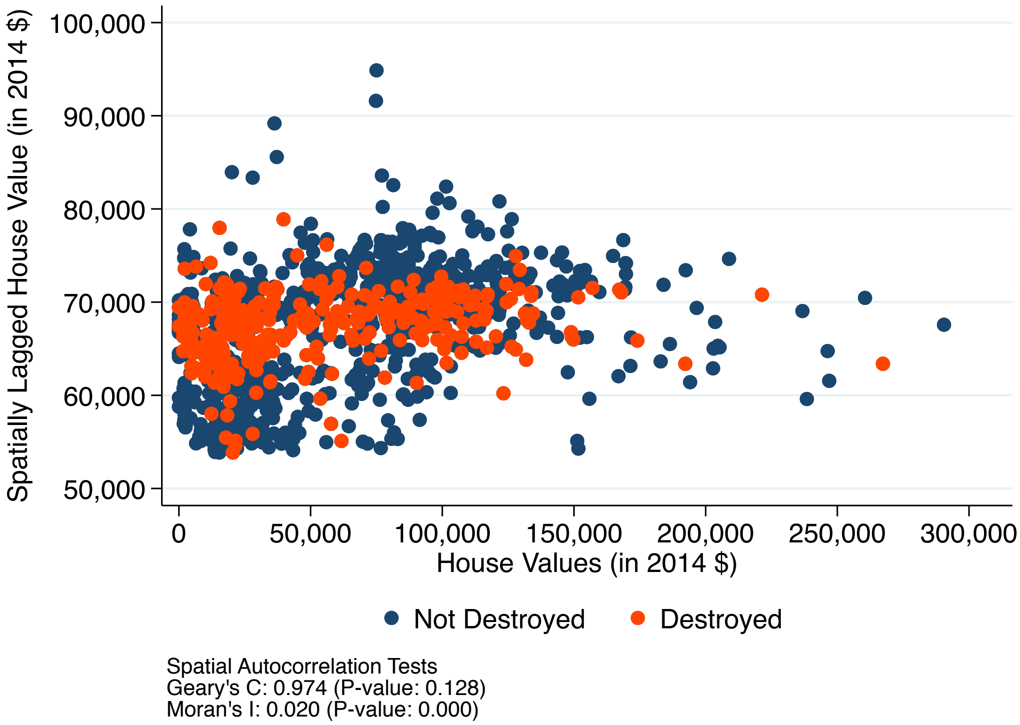


Figure D.1: Moran's I scatter plot

by the inverse of their geographic proximity. Hence, closer houses are given higher weights than those that are further away. A positive relationship in Figure D.1 indicates positive spatial correlation. It is clear from the figure that any positive spatial correlation in our house price data is very modest. Moreover, the figure above distinguishes between houses in the destroyed (orange) and non-destroyed (blue) regions. There is no systematic difference in the house prices along this margin, consistent with our balance tests.

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

E Uncertain Gains from Education (and Comparative Advantage)

Consider an extension of the model presented in section 6 where the gains from education are uncertain and households have Epstein and Zin (1989) preferences. Specifically, assume that the gains from education in the non-fishing sector are stochastic and distributed

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

and the utility function of the parents is

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

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

In this case, we have that

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

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

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

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

The right-hand sides of these expressions differ from those in equations (3a)-(3d) due to the variance terms $\frac{\gamma}{2}\sigma_a^2$, $\frac{\gamma}{2}\sigma_s^2$, and $\frac{\gamma}{2}\sigma_\phi^2$. Here σ_a^2 and σ_s^2 are the variances of the intergenerational shocks to absolute and comparative advantage, respectively, i.e., the variances of $\epsilon^a(i)$ and $\epsilon^s(i)$. In our earlier model, the three variance terms did not appear because of two simplifying assumptions:

log-utility and non-stochastic education. Analogous algebra to that in section 6.2 yields an equation for the cutoff points for moving $q^{*\prime}$ and q^* that can be written

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

where

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

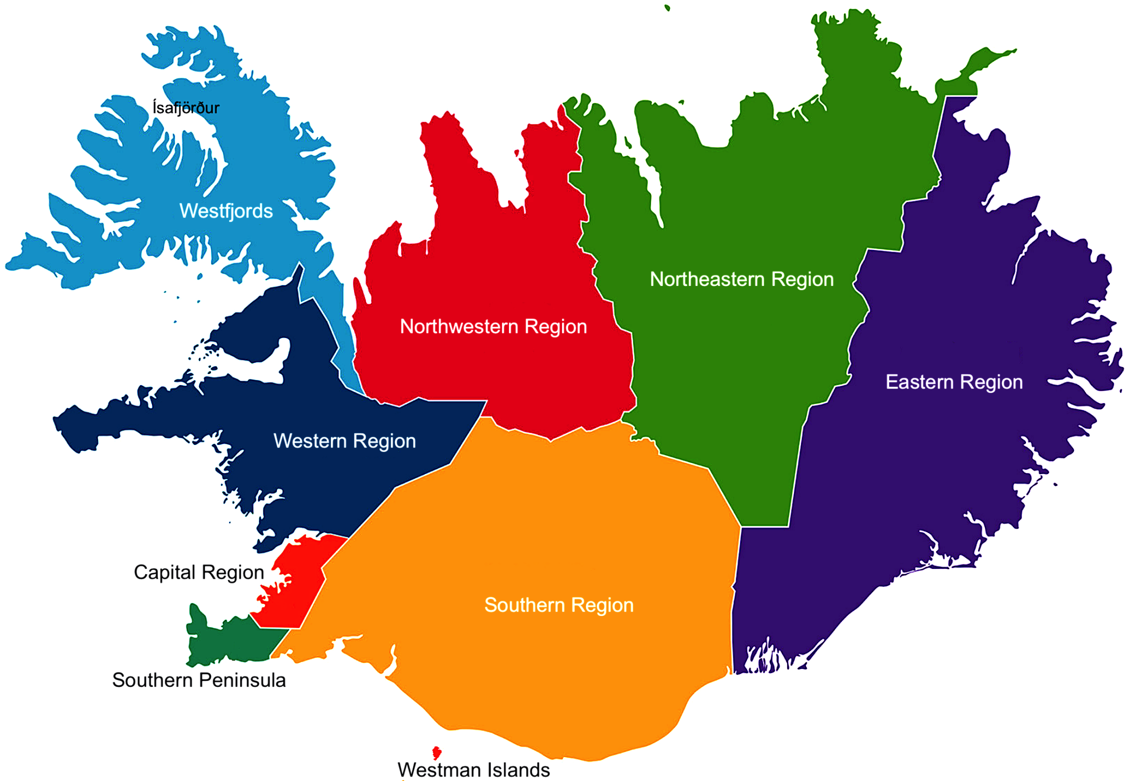
Relative to the expression for $\Psi(m(i), q)$ in our baseline model, there are two additional terms $-\frac{\gamma}{2}\sigma_s^2$ and $-\frac{\gamma}{2}\sigma_\phi^2$. In this model, risk is a source of "moving costs" in the sense that it makes people more reluctant to move for a given expected return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

F Price Level in the Westman Islands

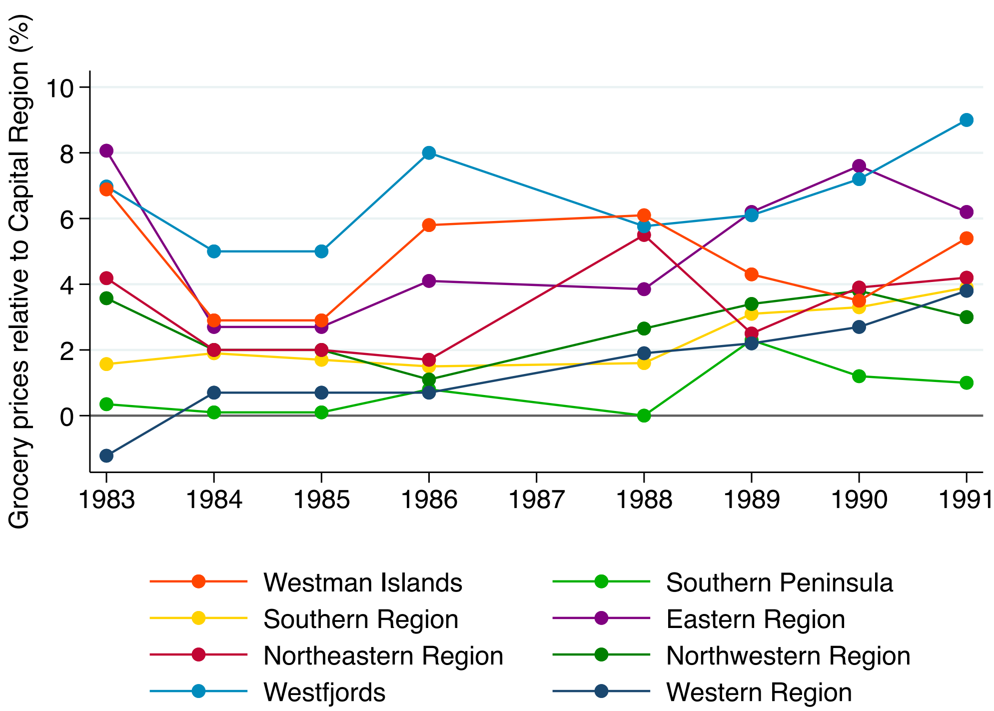
To evaluate whether our causal effects on earnings may be explained by compensating differentials, such as differences in cost of living in the Westman Islands relative to Reykjavik, we study regional differences in the prices of goods and services. We draw on data from detailed regional price surveys conducted by the Public Price Control Authority (*Verðlagsstofnun Ríkisins*) – the predecessor of the Competition Authority—dating back to the 1980s.

Figure F.2, panel (a), shows a regional map of Iceland and panel (b) documents the price level for the corresponding regions. To be informative for our main results, we present the price level in each region in relative terms to that in the Capital Region and treat the Westman Islands, which generally is part of the Southern region, as a separate region. The survey conducted by the Public Price Control Authority records prices of 370 common products in grocery stores all over Iceland. Regional price level is measured as an average of prices of a basket of food and beverages, consumption-weighted based on a standard family, across all stores surveyed in a give region. Figure F.2 documents that prices in the Westman Islands were among the highest in Iceland over this sample period, about 4-6 percent higher than in the Capital Region. The prices in the Westman Islands are similar to what is found in the regions that are furthest away from Reykjavik, the Westfjords and the Eastern Region. They are considerably higher than in the rest of the Southern region that is geographically closest to the Westman Islands, but where, e.g., land transport is possible.[26] This price difference is likely to reflect a general pattern. Indeed when looking at prices

[26] Simple linear regression shows that for every 100km travelled from Reykjavik the price level increases by between 0.67 and 0.83 percent.



(a) Regions of Iceland



(b) Regional price level relative to price level in the Capital Region

Figure F.2: Regional Price Level

Notes: Panel (a) is a map of the regions of Iceland. Panel (b) plots the regional price level relative to the price level in the Capital Region, based on price surveys carried out by the Public Price Control Authority. Data was not available for all regions in 1987. See text for more details.

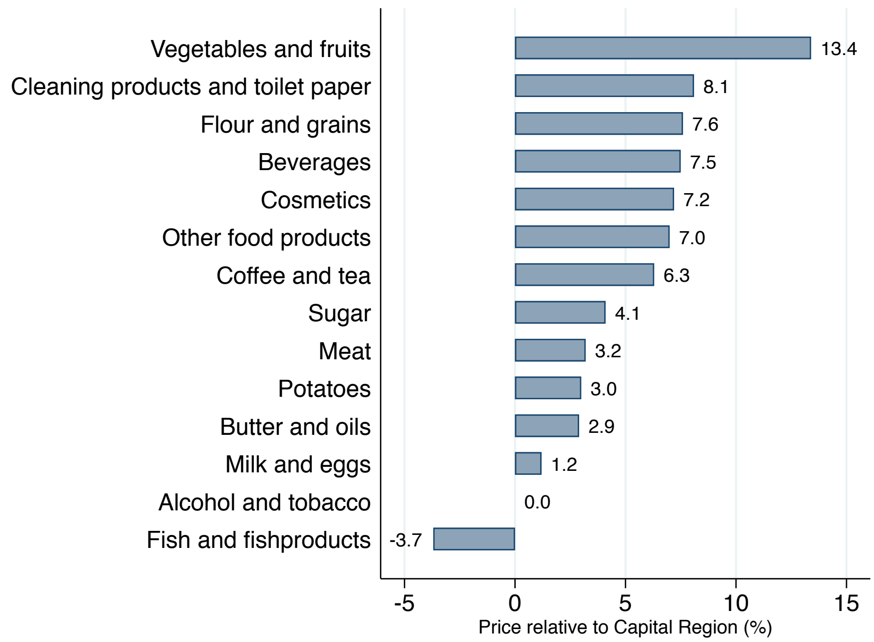


Figure F.3: Price level by product group relative to prices in the Capital Region

Notes: The figure plots the price level difference by product groups in all regions outside the Capital Region relative to the price level in the Capital Region. Data is from price surveys carried out by the Public Price Control Authority. See text for more details.

of other goods and services surveyed by the Public Price Control Authority we find similar price difference. Prices of a standard basket of breads and cakes in bakeries was 9.4 percent higher in the Westman Islands than in the Capital Region, and prices of standard haircuts was 6 percent higher on average.

Figure F.2 paints a clear picture of a higher cost of living in the Westman Islands than in the Capital Region. Does this difference in the average price level reflect a large price difference of a narrow set of goods? Or are differences in prices widespread? Figure F.3 plots price differences by product group in regions outside of the Capital Region relative to that in the Capital Region. The figure documents that almost all products are more expensive outside of the Capital Region, except fish and fish products which are products that are most likely to the produced locally is almost all regions.[27]

While the data shows a clear and general tendency for almost all goods to be more expensive in the Westman Islands than in the Capital Region, this will not reflect true differences in the cost of living if those living in the Westman Islands do not buy goods locally. The Public Price

[27]Prices of alcohol and tobacco are influenced by the fact that these goods are sold by the *State Alcohol and Tobacco Company of Iceland*, which is a state owned company with a monopoly on the sale of alcoholic beverages and tobacco.

Control Authority carried out a survey in 1990 to analyze local markets in the regions of Iceland, which is helpful in shedding light on this issue (Public Price Control Authority, 1990). The survey documents that people living in the Westman Islands bought 85% of their goods locally and the remainder mostly in the Capital Region. These numbers are similar to other regions that are far from Reykjavik, such as the Westfjords (85%) and the Eastern Region (82%), whereas in regions closer to Reykjavik this share is much lower (e.g. 51% in the Southern Peninsula).

Why are prices in the Westman Islands so much higher than in the Capital Region? One possible reason is transportation and inventory costs. In 1987 the Public Price Control Authority conducted a specific investigation into the roots of price differences in the two locations where in earlier surveys they had found prices to be highest: Ísafjörður in the Westfjords (see map in Figure F.2, panel (a)) and the Westman Islands (Public Price Control Authority, 1987). The report finds similar explanations for higher prices in the two locations: higher transportation costs and markups. Transportation costs are high and somewhat higher in the Westman Islands than in the Westfjords. At the time there were seven grocery stores and several smaller neighborhood stores in the Westman Islands. In 98% of cases prices were higher in supermarkets and larger grocery stores in the Westman Islands than in the Capital Region, and in 88% of cases when comparing prices in larger neighborhood stores across locations. It is only in smaller shops, where markups are likely to be high in general, where prices are most comparable. Moreover, there is very little price dispersion across stores in the Westman Islands—much less than in other municipalities—indicating limited competition. With limited changes in transportation costs and the competition environment these regional price differences have been very persistent.

G Supplementary Tables

Table A.1: First Stage Estimates for Dependents, Household Heads and Descendants

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

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

Table A.2: Effects on the Logarithm Earnings – Cohorts Younger than 25 at Time of Eruption

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

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

Table A.3: Effects of Moving on Earnings – Descendants

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

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

Table A.4: Other Outcomes – Cohorts 25 and Older at Time of Eruption

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

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

Table A.5: Effect of Pension on Earnings Estimates – Cohorts Younger than 25 at Time of Eruption

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

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

Table A.6: 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.

H Supplementary Figures

Figure A.1: Fish Catch by Year

Note: Total fish catch in thousands of tones per year in Southern Iceland (left axis) and all of Iceland (right axis). 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*.

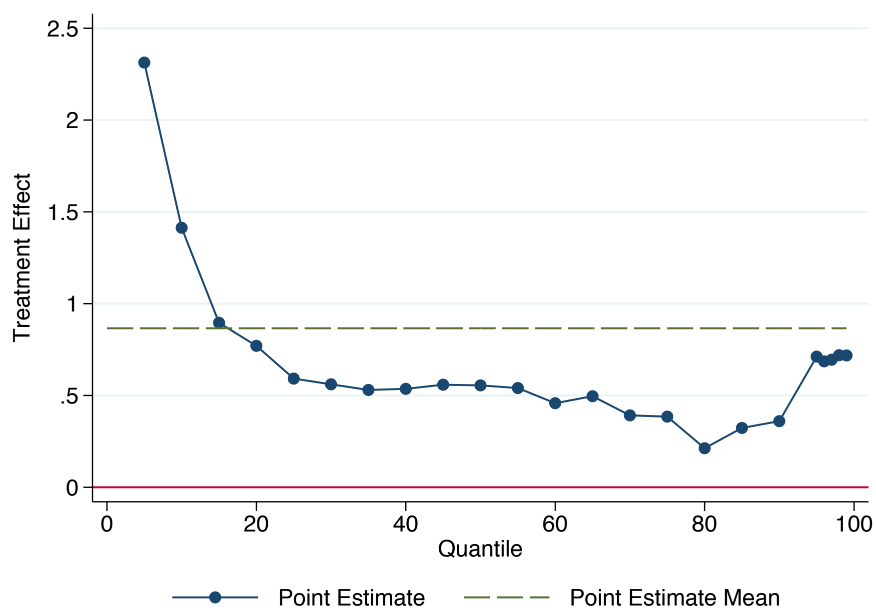
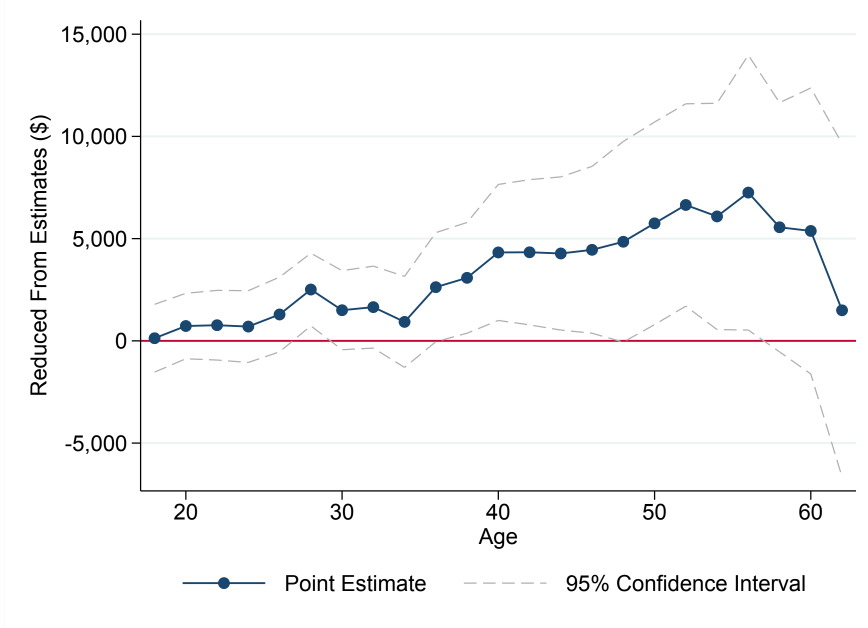
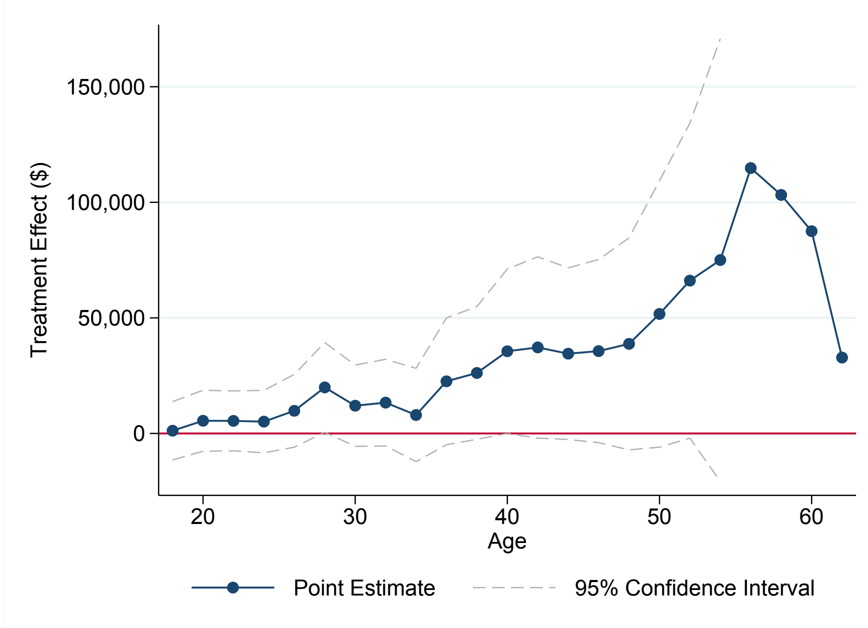


Figure A.2: IV Quantile Effects for Log(Earnings) – Cohorts 25 and Older at time of Eruption

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

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

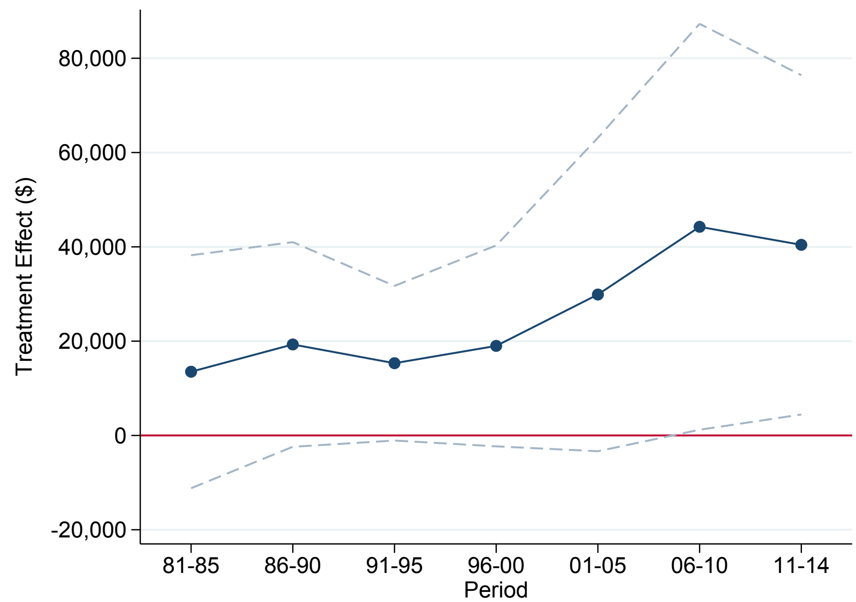


Figure A.4: IV Earnings Effect by Year – Cohorts Younger than 25 at time of Eruption.

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

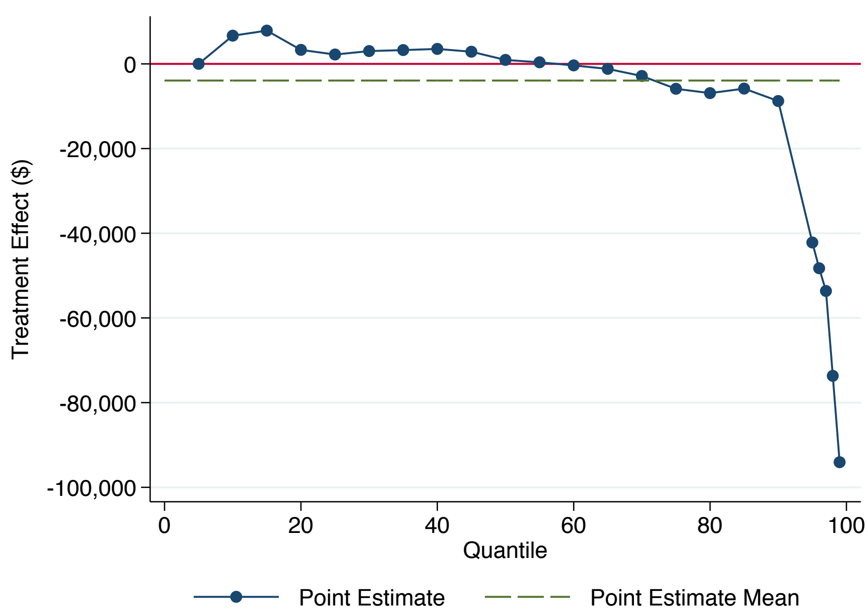
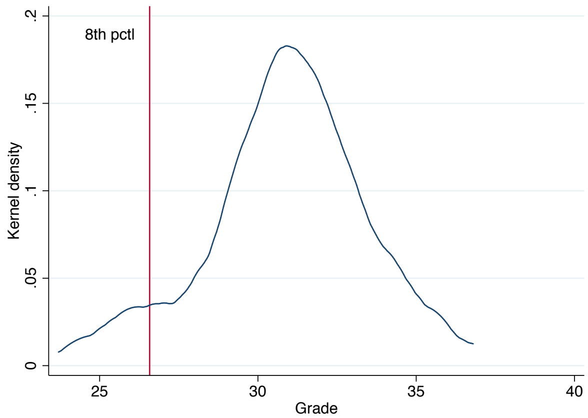
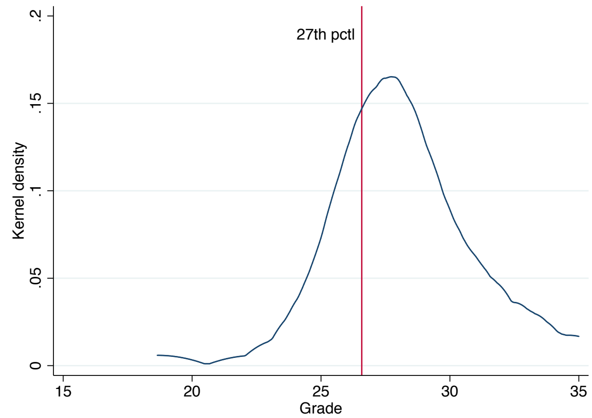


Figure A.5: IV Earnings Quantile Effects – Cohorts 25 and Older at time of Eruption

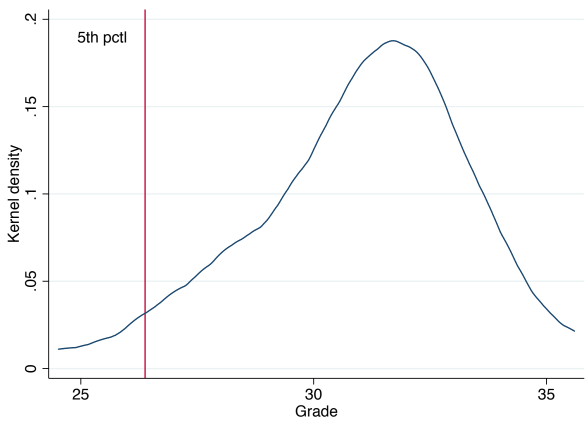
Note: The figure plots quantile treatment effects using the estimator proposed by Abadie, Angrist, and Imbens (2002) for the 5th to the 99th percentile. The effects are estimated in 5 percentile increments up to the 95th percentile, and in 1 percentile increments for 96th to 99th percentile. The green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



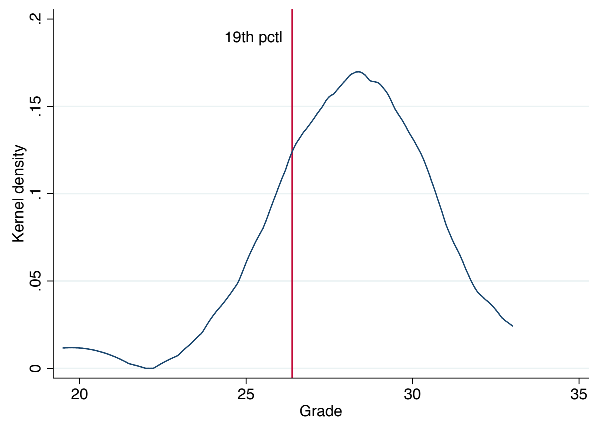
(a) Mathematics – Capital Region



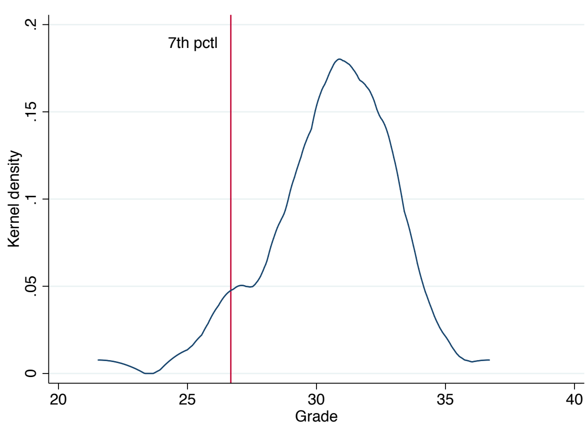
(b) Mathematics – Other Regions



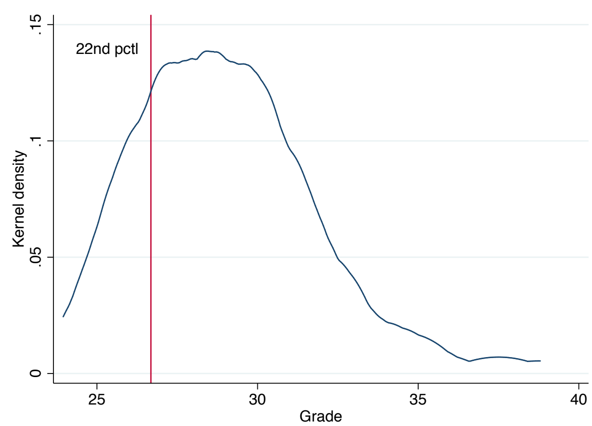
(c) English – Capital Region



(d) English – Other Regions



(e) Icelandic – Capital Region



(f) Icelandic – Other Regions

Figure A.6: 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.