

The Long-term Impacts of International Migration: Evidence from a Lottery

John Gibson, David McKenzie, Halahingano Rohorua, and Steven Stillman

Abstract

We examine the long-term impacts of international migration by comparing immigrants who had successful ballot entries in a migration lottery program, and first moved almost a decade ago, with people who had unsuccessful entries into those same ballots. The long-term gain in income is found to be similar in magnitude to the gain in the first year despite migrants upgrading their education and changing their locations and occupations. This results in large sustained benefits to their immediate family who have substantially higher consumption, durable asset ownership, savings, and dietary diversity. In contrast we find no measureable impact on extended family.

JEL classification: F22, O15

Key words: International Migration, Natural experiment, Assimilation, Household Wellbeing

Income differences between developed and developing countries are vast: in 2014, GDP per capita in high-income OECD countries was nine times that in middle-income countries and 68 times the per capita income in low-income countries.¹ These differences hold even within very narrowly defined occupations, with [Ashenfelter \(2012\)](#) showing wages of workers at McDonald's differ by as much as a factor of ten across countries. How much of this difference can someone migrating from a poorer to a richer country hope to gain?

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1 Source: *World Development Indicators* online query September 23, 2015: high-income OECD countries had a per-capita GDP of US\$43,619, middle-income countries US\$4,694, and low-income countries US\$639.

The answer depends on the sources of these income differentials. If better institutions, higher quality capital, and other factors raise the productivity of all workers, then the same worker will be immediately vastly more productive working in a rich country than in a poorer one and so should earn more as soon as they move. In contrast, if benefiting from these factors requires language skills, country-specific knowledge, and other attributes that are embedded in native workers, immigrants may be initially no more productive abroad than they were at home, and their short-term income gains may be limited. In the longer-term, any income gains will then only occur if immigrants can assimilate and gain these country-specific skills.

Determining either short-term or long-term gains from migration is complicated by the fact that migrants are self-selected, differing in skills, ambition, and other attributes from those who do not migrate. One approach is to try to control for as many observable differences as possible between migrants and non-migrants and then argue that the self-selection on unobserved factors needed to overturn the measured gains is too extreme to be likely. Using such an approach, [Clemens et al. \(2009\)](#) document that a typical individual from the average developing country should expect to earn 2.5 to 3 times their income upon moving to the United States.

Another way to deal with selection issues is to use evidence from migration lotteries. [In this paper, we use a ten-year follow-up survey of Tongans who had applied to the migration lottery conducted under New Zealand's Pacific Access Category \(PAC\), measuring long-term impacts of migration by comparing outcomes for successful and unsuccessful applicants. In previous work, McKenzie et al. \(2010\) and Stillman et al. \(2015\), we examine the short-run impacts of migration on income \(and in the second paper, also other forms of wellbeing\). However, this paper is the first we know of that examines both short-term and long-term impacts of migration in a modern setting where it is possible to get unbiased estimates of both.](#)²

Our previous work shows that migrants from Tonga to New Zealand are positively self-selected, have larger returns to unobservable attributes in New Zealand than in Tonga, and that migration leads to a 263 percent gain in income in the first year after migration. While this is a large impact, it is just half the gap in percapita incomes between the two countries. A key question, for which there is little evidence to date, is whether migrants are able to gain more of this gap over time. One hypothesis is that the income gains increase over time as migrants assimilate and gain new skills. But a competing hypothesis is that the gains may weaken over time if migrants have given up occupations at home with rising income trajectories to work in occupations abroad that offer higher immediate incomes but less prospect of career growth. For example, migrants working as teachers, public servants, or doctors in their home countries may face wage structures determined strongly by seniority, and earn more abroad as cleaners, shop assistants, and agricultural workers but with less scope for wage growth.

It is complex and costly to track immigrants and a comparison group of non-migrant lottery applicants over a decade and this is unlikely to be feasible elsewhere. It would be much easier if one could just take an unbiased estimate of the short-run impact of migration and add an assimilation profile from either the same migrant group or a more general group of migrants in the destination country. This approach has two problems. First, as noted by [Borjas \(1985\)](#) and [Abramitzky and Boustan \(2016\)](#), if the quality of each immigrant cohort differs, the experience of previous cohorts will give a biased picture of the true assimilation path for current migrants. Second, even if longitudinal data for an immigrant group are available, without a counterfactual group of non-migrants in the origin country, one must use destination country natives to estimate the extent to which any changes in income over time reflect actual

2 [Abramitzky et al. \(2014\)](#) examine short-run and long-run impacts of migration for individuals who moved to the United States at the beginning of the twentieth century. Other studies of migration lotteries include [Clemens \(2010\)](#), who examines Indian IT workers selected to migrate to the United States under the H1B visa lottery, and two studies of impacts on those left behind but not on the migrants themselves: [Mergo \(2011\)](#) for Ethiopian households with a family member migrating through the U.S. Diversity Visa lottery and [Gibson et al. \(2013\)](#) for Samoan households with a family member migrating through the Samoan Quota lottery.

assimilation versus returns to work experience, life-cycle effects, or general business cycle effects. Thus, one must assume that these effects are the same for migrants and natives. It is thus unsurprising that [Abramitzky et al. \(2014\)](#) find that changes over time for natives provide a poor counterfactual for estimating assimilation among immigrants.

Our first contribution is to show, in section 2, the assumptions needed if one uses evidence from assimilation studies that compare migrants with destination country natives as a proxy control group for estimating long-term migration impacts. This indirect approach requires origin and destination countries to experience similar income growth rates and to have similar lifecycle patterns in the labor market. While the first condition holds for the specific example of Tonga and New Zealand, the second condition does not. More generally, these conditions would often be violated for a developing country origin and a developed country destination, so indirect estimates of long-term migration impacts based on the assimilation literature are unlikely to equal direct estimates that use origin country comparison groups. Moreover, while native-to-migrant comparisons can inform about inequality in destination countries, they cannot inform about the sources of cross-country income differences in the way that migrant to origin country non-migrant comparisons can.

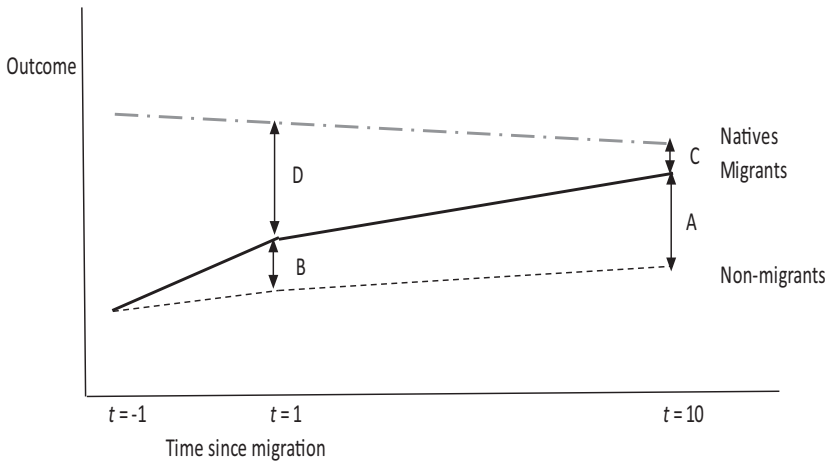
Given the above considerations, in order to study long-term migration impacts one needs a sample of migrants and a comparison group from the origin country. We discuss how we get such samples in section 3 and how we deal with issues that arise when migration occurred up to a decade earlier, including individuals who on-migrate to other countries, individuals who had not moved at the time of short-term follow-ups but have now moved (“slow compliers” rather than non-compliers) and ballot losers who migrate via substitute pathways. We also consider locational changes within the destination country that were less prevalent when the migrants were first studied, along with (associated) occupational changes. A further important contribution of our paper is that we have a long-run follow-up survey of the extended families of both the migrant households and the unsuccessful applicants, allowing us to examine the long-term impacts on extended family members.

We find that the long-term impact on income of Tongans migrating to New Zealand is similar to that found in the short-term. The economic payoff to this migration appears to come immediately and then hardly grow, notwithstanding various investments by the immigrants in qualifications, internal mobility, and occupational change.³ There are a number of possible explanations for this finding. Unlike many immigrants in other settings, Tongans speak the local language (English) prior to migration, many have previously been to New Zealand, and most have a family network in New Zealand. Hence, this group of individuals likely had a high level of New Zealand-specific knowledge prior to migrating. Furthermore, many of these individuals worked in public sector jobs in Tonga that have high returns to seniority and, hence, perhaps traded-off some future increased earnings growth for large immediate gains in income.

In previous work, Tongan migrants to New Zealand are seen to be quite similar in many dimensions to Mexican migrants to the US ([McKenzie et al. 2010](#)). This is also true among the dimensions discussed above. For example, Mexican migrants speak a language that is widely used in the destination country, many have previously traveled to the US, and most have an extended network already there. Furthermore, many worked in similar jobs as the Tongan migrants prior to migration (for example, teachers and nurses). Previous work using traditional methods has found limited assimilation for Mexican migrants to the US, which is typically viewed negatively ([Borjas 2015](#)). Seen in the light of our findings, perhaps Mexican migrants are also trading-off large short-run wage gains for lower future earnings growth and the reason that ‘assimilation’ does not occur is not lack of US specific human capital.

We also examine the impact of migration on a range of other outcomes. Since the initial gains in income are so large, migrants derive very large lasting benefits. They earn almost 300 percent more than

3 [Stillman and Maré \(2009\)](#), using traditional methods, also find no evidence of improvement over time in economic outcomes for a general group of Pacific Island migrants to New Zealand in a slightly earlier time period.

Figure 1. Direct and Indirect Estimates of the Long-term Gain from Migration

non-migrants, have better mental health, live in households with more than 250 percent higher expenditure, own more vehicles, and have more durable assets. We estimate a conservative lifetime gain to the adult principal migrants of NZ\$315,000 in net present value terms (approximately US\$237,000). These gains seem to accrue mostly to the migrant and their immediate family who accompany them with little significant measureable impact of this migration on extended family remaining in Tonga.

I. Direct and Indirect Migration Counterfactuals

The development literature and our own previous work (McKenzie et al. 2010) identifies the immediate gain from migration by comparing outcomes for migrants with those for similar non-migrants. In the case of a migration lottery, in which these two groups would have identical outcomes prior to migration and would exhibit the same trend in the absence of migration, this short-term gain is given by the difference in outcomes between migrants and non-migrants at time $t = 1$. This is indicated by the term B in figure 1. In contrast, studies on assimilation in the immigration literature compare the change over time in income or occupation for migrants and for natives in the destination country. In figure 1, this is given by the difference between the D term at $t = 1$ and the C term at $t = 10$. In the example shown, this difference is positive, with migrants catching up to the native workers over time.

An indirect estimate of the long-term gain from migration would add together these two effects, giving $B + (D - C)$, combining the short-term income gain with the assimilation effect. From figure 1 it is clear that this only gives an accurate estimate of the true, directly estimated, long-term gain (A) if the outcome trajectories for the native workers and for origin country non-migrants are parallel between time $t = 1$ and $t = 10$. For this to be the case, the home and destination economies must experience similar income growth rates over the decade and the labor market life-cycle dynamics must be similar in the two countries.

In many cases, we might expect this assumption to be violated so indirect estimates will be inaccurate. As a first example, consider migrants from fast-growing economies like China and India who move to the US. Then we might expect the income profile for Chinese or Indian non-migrants to exhibit much steeper wage growth over a decade than would be the case for US native workers, in which case the indirect estimate would overestimate the long-term income gain from migrating. In contrast, consider migrants fleeing impending civil war in Syria or Iraq and moving to Germany. In those cases, we might expect the origin country non-migrant income profile to exhibit negative growth over the subsequent

decade compared to positive growth for native workers in Germany. In this case, the indirect estimate would underestimate the long-term income gain from migrating.

Even when the origin and destination countries experience similar income growth rates over the period, the indirect estimate may not deliver an accurate estimate of the long-term gain if the two countries differ in lifecycle profiles. For example, if the origin country largely pays people based on job tenure rather than on productivity, the age-earnings profile may be steeper there than in the destination country. Similarly, if people retire or change occupations at different points in the lifecycle in the two countries, combining the short-term impact estimate (B) with the assimilation profile ($D-C$) will give a biased estimate of the impacts of migration on occupational mobility.

For the specific example of migration from Tonga to New Zealand, the income growth rates are similar but the labor market lifecycle dynamics are not. In recent years, both countries have experienced similarly faster rates of economic growth than their historic norms. Data from the *World Development Indicators* show an average growth of GNI per capita (in US dollars) of 6.8% for NZ and 6.7% for Tonga over 2005 to 2014. As a result, at the macroeconomic level, the assumption of parallel trends needed by the indirect estimator may be reasonable.⁴ However, the lifecycle effects are different, especially in terms of occupational mobility, with workers in Tonga leaving professional jobs, typically in the public sector, at a much earlier age than in New Zealand and shifting to occupations such as farmers and fishermen. Thus, part of the gains from migration in not having this enforced early exit from the primary labor market would be missed by using the indirect approach to form a counterfactual for the migrants.

II. Context to Our Surveys

The Kingdom of Tonga is an archipelago of islands in the Pacific approximately three hours north of New Zealand by airplane. The resident population is just over 100,000, and the gross national income (GNI) per capita is \$4,150 (in 2005 PPP \$), which is similar to Indonesia and ranks Tonga 121st out of 190 countries.⁵ Average incomes in New Zealand are about six times as high with a per capita GNI of \$24,400 in 2012. Tonga and New Zealand both rank 26 places higher in the Human Development Index than they do in terms of GNI, making New Zealand the largest positive outlier of very high human development countries and Tonga the second largest outlier of countries with medium levels of human development (UNDP 2013). Hence, while the countries differ a great deal in terms of average incomes, higher incomes in New Zealand are not compensating for lower levels of other aspects of human development.

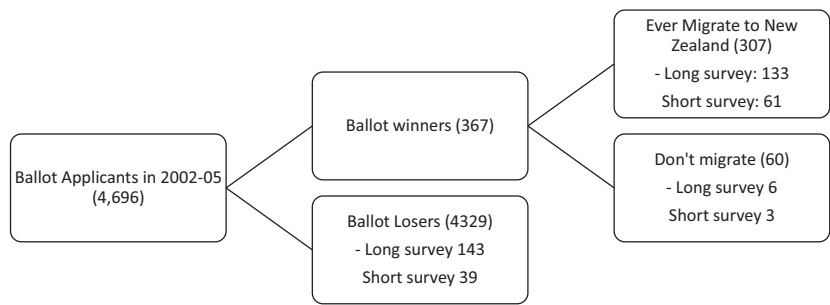
Emigration out of Tonga is high with 30,000 Tongan-born living abroad, mainly in New Zealand, Australia, and the United States. Despite an earlier history of employment migration to New Zealand, family reunification was the main channel of access in the 1990s following New Zealand's implementation of a points-based immigration system that favours highly skilled migrants.⁶ In 2002, New Zealand

4 However, New Zealand has also seen net long-term immigration rise to annual totals equivalent to 1.4 percent of the resident population with these inward flows comprised especially of returning New Zealanders, skill-selected workers, and international students. Strong growth in labor supply may create a wedge between macroeconomic trends and labor market trends; more generally, macroeconomic aggregates tend to over-estimate income gains from migration (McKenzie et al. 2010).

5 All statistics in this section are from the 2013 *Human Development Report* (UNDP 2013).

6 Family reunification migration to New Zealand is restricted to parents and spouses, with adult children and siblings having to seek entry through other residence channels such as the skilled migrant category. In 2007 and, again in 2012, rules to sponsor parents were tightened with income and wealth thresholds that exclude most Tongans and a rule that all children of the sponsored parent had to be in a different country than the parent with more than half the total children in New Zealand. Bedford and Liu (2013) show that these policy changes had a larger negative effect on Tongans than on any other immigrant group in New Zealand; the number of residence approvals for Tongans under the parents category fell by 70% after the 2007 policy changes.

Figure 2. Sampling Frame



introduced a new migration program, the Pacific Access Category (PAC), that lets a quota of 250 Tongans permanently migrate each year. Any Tongan citizens aged 18 to 45 who meet certain English, health, and character requirements may register. A random ballot selects amongst applicants (known as “principal applicants”) with odds of about 1-in-13 (7.8%) during the 2002–05 ballot years that our sample is drawn from.

If their ballot is selected, applicants have six months to obtain a full-time job offer in New Zealand that meets an income threshold similar to the minimum wage. This ensures self-reliance since Tongans are not eligible for most forms of welfare until they reside in New Zealand for two years. After a job offer is filed along with a residence application, it typically takes from three to nine months to receive residence approval, and immigration to New Zealand must then occur within 12 months. Spouses and any unmarried children up to age 24 are also eligible to immigrate with the ballot winner (and are part of the annual quota of 250 people).

The typical applicant is age 33 at the time of application with 11 to 12 years of schooling and two-thirds of them married (McKenzie et al. 2010). Just over half of the applicants were living in households in which all members would be eligible to migrate if they won while the rest were living in households that contained extended family such as their parents, siblings, nephews, and nieces (Gibson et al. 2011). The context is thus one in which migration results in a nuclear household moving permanently abroad and potentially leaving behind extended family members who they had been living with at the time, but would not necessarily continue living with, as they got older even without migration.

Sampling Design

Our population of interest consists of entrants to the 2002 to 2005 PAC migration lotteries. There were a total of 4,696 principal applicants of whom 367 were randomly selected as ballot winners (figure 2). Official records provided by the New Zealand immigration authorities in late 2012 show that 307 of these winners (84%) had residency applications approved and had ever migrated to New Zealand. The remaining 60 ballot winners did not migrate and are thus non-compliers to the treatment of migration.

Our prior studies measuring the short-term impacts of migration examined outcomes for a random one-third subset of the ballot winners who eventually moved to New Zealand. The main reason for using a subset was that the New Zealand government’s longitudinal survey of immigrants had randomly selected a large pool of the ballot winners as potential individuals to interview in their survey. To avoid respondent burden from being in two surveys, their details were not released to us at the time of our initial surveys. In late 2012, we received permission from the immigration authorities to view the names of all PAC ballot winners from 2002–05, including those earlier reserved for the official government survey.

In order to have a larger sample to examine long-term impacts, we decided that, instead of just trying to recontact the sample used previously, we would track all migrants from this full list. Given that the

only address details were from about ten years earlier, when the individuals had applied for residency and in many cases were for relatives, we undertook simultaneous fieldwork in both New Zealand and Tonga since people in their home village were often the best source of updated contact information.⁷ These tracking and surveying operations were costly and are unlikely to be possible in many other contexts when the countries and populations involved are larger.

Our main survey involved an extensive face-to-face interview, which also collected anthropometrics, blood pressure, peak lung flow, and included lab-in-the-field games. Of the 307 principal applicants ever migrating to New Zealand, 133 completed the full survey between late 2013 and the end of 2014. In order to bolster our sample size, in early 2015 we fielded a shortened survey that did not include health measurements or the lab-in-field games. This was mainly done as a telephone interview and was designed to reach those who had on-migrated beyond New Zealand or were located in parts of New Zealand that were impractical for face-to-face interviewing, although we also learned, through snowball effects, of more migrants in our face-to-face survey area and gave them the short survey as well. Overall, 61 additional ballot winners who had ever migrated to New Zealand were given the short survey, including 11 who had now on-migrated to Australia (ten) and the UK (one). In total, we were able to survey 194 households with principal applicants who ever migrated to New Zealand after winning the ballot (figure 2), which is 64 percent contact rate.

We had even less information available for the ballot losers and non-compliers since these individuals had not filled out residency applications. We therefore used the same surveying approach for these groups as we had in our previous survey, which was to sample from the same villages in Tonga from which our migrants originated. Out of 4329 ballot losers, 143 were administered the long form survey and 39 the short survey (of which nine had subsequently moved to New Zealand through alternative pathways, including by winning a later round of the PAC lottery). Finances limited us to this relatively small sample, but, based on our previous research, we judged that it would give us enough power to measure economically significant impacts. An advantage of surveying from the same origin villages is that we can implicitly control for any unobserved characteristics that vary spatially in Tonga. Finally, we have a small sample of nine non-compliers; six who received the long survey and three the short survey. This is out of a population of 60 non-compliers, which hence made it difficult to find many individuals in this group. Figure 2 shows these groups.

In our main results, we weight the ballot winner sample to reflect the population proportions of ever migrating to New Zealand versus non-compliers. This is necessary because we effectively have a choice-based sample, although our previous research suggests that there is no selection among the non-compliers, and, hence, they can effectively be excluded from the sample (McKenzie et al. 2010). We also examine robustness, using two other weighting schemes. The first uses a snapshot of data from late 2012 on cross-border movements coming from passport scans, which revealed that 265 of the 307 ever-migrating ballot winners were in New Zealand at that point in time.⁸ This set of weights allows for the possibility that we found it harder to track individuals who had left New Zealand and so puts more weight on the ballot winners in our survey who were found outside New Zealand. The second alternative weighting scheme allows for the possibility that on-movement among the ballot losers is higher than our sample suggests.

Our survey collects data on the principal applicants and the household members living with them, which is the focus of the majority of our analysis. As noted, principal applicants are typically adults in

7 Only one-third of the migrants surveyed used fixed line telephones with the remainder relying on mobiles, and very few were listed in any type of telephone directory. Moreover, a large fraction of the migrants had moved out of the main destination city of Auckland, which made it harder to track them through churches and other local social networks.

8 This overstates the importance of on-migration since the PAC migrants who reside in New Zealand but were abroad on the day we received the record of arrivals and departures are not counted in the 265.

Table 1. Pre-determined Characteristics

	Ballot Winners	Ballot Losers	P-value
Age	40.11	41.80	0.045
Male	0.66	0.67	0.814
Born in Outer Islands	0.23	0.24	0.973
Sample Size	203	182	
Joint orthogonality test p-value			0.306

Source: Author’s calculation from survey data described in text.

their early thirties who brought both a spouse and children to New Zealand. We also linked these applicant households to a “partner” household in Tonga containing either a parent or elder sibling of the principal applicant (with priority on parents over siblings and females over males).⁹ In about 15% of cases, the partner defined by these rules was in the same dwelling in Tonga as the unsuccessful PAC applicant, but most are not co-residents and, in this regard, differ from the left-behind family studied in [Gibson et al. \(2011\)](#). The advantage of this approach is that we are not conditioning on changes in household structure that occurred post-migration when drawing the sample; in other words, we always look for the same “partner” regardless of whether and how many times they have subsequently changed households. We collect data on 258 such households, which we use to measure the impact of migration on a specific type of extended family.

Assessing Balance

The use of a random lottery ensures that the ballot winners and losers have similar characteristics in the population of ballot applicants. We only have a sample of this population, so we check the extent to which this sample is balanced on observable, pre-determined characteristics. Since the ballots were at least a decade ago, the only such characteristics available in our survey are age, gender, and birthplace (a dummy for being born off the main island of Tongatapu). [Table 1](#) compares these characteristics for the samples of ballot winners and losers. Our samples are balanced on gender and island but not completely balanced on age. Nevertheless, a joint test of orthogonality cannot reject that these three characteristics are jointly balanced. This suggests that our treatment (ballot winner) and control (ballot loser) samples are comparable, and we can use the random assignment provided by the lottery to assess causal impacts. Nevertheless, we condition on age, gender, and island of birth to improve power and control for any effect of this slight difference in age, and we consider robustness to alternative weighting schemes that allow for different response rates among subgroups.

[Table 1](#) shows that the average applicant is 41 years old at the time of the follow-up survey. A 10–90 percentile range for age is 32 to 51. Two-thirds of the principal applicants are male.

Survey Variables for Measuring Impacts

In both the long and short survey, the PAC principal applicant reported pre-tax earnings in the previous week and total weekly earnings of all household members. These earnings, and other monetary values, are converted to New Zealand dollars (NZD) at the average market exchange rate for the month of the interview. The New Zealand CPI rose just 0.4 percent from December 2013 to March 2015, covering the span of the survey, and so nominal values can be treated as real values.¹⁰ For robustness, we will also

9 The sampling was based on these rules because we had the principal applicants and the partners play lab-in-field games, and we wanted to restrict the dyads for the games to specified relationships.

10 Over the same period, the Pa’anga exchange rate averaged 0.65 NZD and had a range of just three NZ cents. To allow comparison with results from other countries, the NZD-USD exchange rate averaged 1.33 over this period.

show impacts using PPP exchange rates to convert the income gains, using prices we collected in both countries.

We also calculate annual income for households that responded to the long survey. This includes: earnings; net returns from sales of food crops, fish, livestock, tapa cloth and mats (from household reports on an average month); income from investments, pensions, and rentals (from reports for the previous two weeks); imputed values for own-produced or own-captured food consumed by the household (from reports for the previous week); and, remittance receipts (from a household-level annual recall).

Household expenditure is measured with a 20-category recall module with reference periods ranging from one week to six months, depending on the source of expenditure. The imputed value of items consumed from own-production is added to the cash expenditure variable to measure total expenditures on consumption. Impacts on income and expenditure are estimated for total values and in per capita terms since household size can change with migration. One category of expenditure collected separately is remittance spending over the previous year, which is captured also for households in Tonga from questions about transfers given to other Tongan households. The other monetary measures for households in the long survey are the total value of savings and of net worth while ownership of various durable goods, financial access, and dietary diversity is also collected at the household level.

In addition to measuring monetary welfare, the subjective wellbeing of the principal applicant was elicited. Respondents were asked to imagine a ten-step ladder, where on the bottom step were the poorest people and the top step the richest people, and to state which step of the ladder they were on today. Ravallion and Lokshin (2001) refer to this as an economic ladder question and note that it leaves it up to the individual to define what constitutes “poor” or not and captures subjective economic welfare. Prior research shows PAC migrants perceive climbing the welfare ladder in a retrospective comparison with their premigration life but not when compared to unsuccessful PAC applicants (Stillman et al. 2015).¹¹

Other questions answered at an individual level are for education, mental health, and occupations. Mental health was measured using the MHI-5 index of Veit and Ware (1983). Respondents reported occupational changes in the past five years and what the prior and current occupations were. We code these responses using the New Zealand Socioeconomic Index of Occupational Status (NZSEI) of Davis et al. (1997), which gives a continuous measure ranging from ten (e.g., non-ordained religious associate professionals) to 90 (e.g., health professionals). Scores reflect characteristics of occupations that either translate into observable lifestyle factors such as incomes or reflect valued socioeconomic inputs such as educational levels. An occupational status index for New Zealand should give a better basis for cross-country comparisons with occupations in Tonga than would an explicit international index such as the ISEI developed by Ganzeboom and Treiman (1996), since an international index is unlikely to have a country as small as Tonga in mind. Moreover, given the role of New Zealand as the major migration outlet and one of the main suppliers of higher education to Tonga, it is reasonable to believe that the same occupations have similar status in both countries.

III. Methods

Our focus is on estimating the long-term impact of migration on both the immigrants themselves and on their extended families. To deal with self-selection bias, we rely on the PAC lottery randomly choosing a subset of individuals who become eligible to migrate from a larger pool of individuals who wanted to

11 In contrast, Tongan seasonal migrants show significant rises in their ladder position compared to respondents in households without seasonal workers (Gibson and McKenzie 2014b). These divergent results may be due to frame of reference effects; settlement migrants may use destination country standards to re-evaluate their former life in Tonga while seasonal migrants continue to evaluate using Tonga as a reference standard since they are not attempting to integrate into the destination country.

migrate. Given this mechanism, in the absence of non-compliance, a direct comparison of outcomes for lottery winners and losers would give us an unbiased estimate of the impact of migration. However, in practice, some lottery winners do not migrate and some lottery losers migrate to New Zealand via other pathways, including winning the PAC lottery in later rounds.

Therefore, we use two approaches to measure impacts. First, we estimate the *intention-to-treat* (ITT) effect of being offered the opportunity to migrate through the Pacific Access Category irrespective of whether the individual does migrate. This is done by simply comparing outcomes for ballot winner to those of losers:

$$Outcome_i = \alpha + \beta BallotWin_i + \gamma X_i + \varepsilon_i \quad (1)$$

where *BallotWin* takes value one if the principal applicant was chosen in the 2002 to 2005 Pacific Access Category ballots and zero otherwise, and *X* is a vector of controls for the pre-determined characteristics (age, gender, and island of birth), for survey type, and for each of the different PAC ballot years that the individual entered. The median and modal applicant in our sample entered only one ballot between 2002 and 2005. However, since those who entered multiple times had a higher probability of being chosen, we correct for this by conditioning on which lotteries were entered (Abdulkadiroglu et al. 2011).¹²

The second approach is to use assignment to the treatment (winning the PAC lottery) as an instrumental variable (IV) for the actual treatment of migrating, in regressions like:

$$Outcome_i = \mu + \lambda Migrate_i + \delta X_i + \omega_i \quad (2)$$

where *Migrate_i* is a dummy variable that equals one if person *i* ever migrated to New Zealand and is zero otherwise. This includes any individuals who thereafter on-migrated to other countries, since we want to estimate the impact of having ever migrated, and, in practice, on-migrants are a selected group and hence examining them separately would require another valid instrument. Estimating λ by instrumental variables gives us the *local average treatment effect* (IV-LATE) of ever migrating to New Zealand, which can be interpreted in this case as the impact on people who would migrate only if they won the PAC lottery and not otherwise.

IV. Results

We first examine impacts on income earned by the principal applicant and then their other individual level outcomes. We then turn to measuring household level outcomes, first for the household that lives with the principal applicant and then for the specific type of extended family in Tonga defined by our partner household rules.

Impacts of Migration on Income of the Migrant

Table 2 presents the estimated gain in weekly income (in current New Zealand dollars) under a variety of different specifications. Columns 1 to 3 show ITT impacts and columns 4 to 6 the LATE impacts. Columns 1 and 4 in panel A are our base specifications. Here, the market exchange rate is used to convert Tongan pa'anga into New Zealand dollars, and the sample is weighted to reflect the population proportions of ballot winners ever migrating to New Zealand versus those who are non-compliers. We estimate that winning the ballot leads to a NZ\$247 increase in weekly earnings while ever migrating leads to a NZ\$340 increase in weekly earnings. Average earnings for ballot losers in Tonga are NZ\$126 per week, so we estimate that migration leads to a 271 percent increase in weekly earnings.¹³

12 This is a specific case of the more general idea of conditioning on randomization strata; conditional on which ballots a person entered it is random whether or not they would be chosen.

Table 2. Impact on Income Gain

	ITT	ITT	ITT	LATE	LATE	LATE
Panel A: Impact on Income Gain in New Zealand Dollars						
Ballot winner	246.9*** (32.8)	240.0*** (35.8)	220.9*** (36.7)			
Migrate				340.0*** (24.5)	334.1*** (31.0)	328.1*** (33.9)
Weighting used:	w1	w2	w3	w1	w2	w3
Sample Size	385	385	385	385	385	385
Control Mean	125.6	125.6	125.6	125.6	125.6	125.6
Panel B: Impact on Income Gain in PPP-adjusted New Zealand Dollars						
Ballot winner	260.5*** (34.1)	252.8*** (37.0)	233.3*** (37.8)			
Migrate				358.7*** (24.8)	351.9*** (31.2)	346.5*** (34.0)
Weighting used:	w1	w2	w3	w1	w2	w3
Sample Size	385	385	385	385	385	385
Control Mean	126.1	126.1	126.1	126.1	126.1	126.1

Notes: Robust standard errors in parentheses, *, **, *** indicate significance at the ten, five, and one percent levels, respectively. All regressions include controls for age, gender, island of birth, ballot years entered, and survey form (long or short).

Weights: w1 weights for population proportions of compliers versus non-compliers, w2 also weights migrants for observed movements out of New Zealand, w3 additionally allows ballot losers who migrate to have twice the weight of those who remain in Tonga.

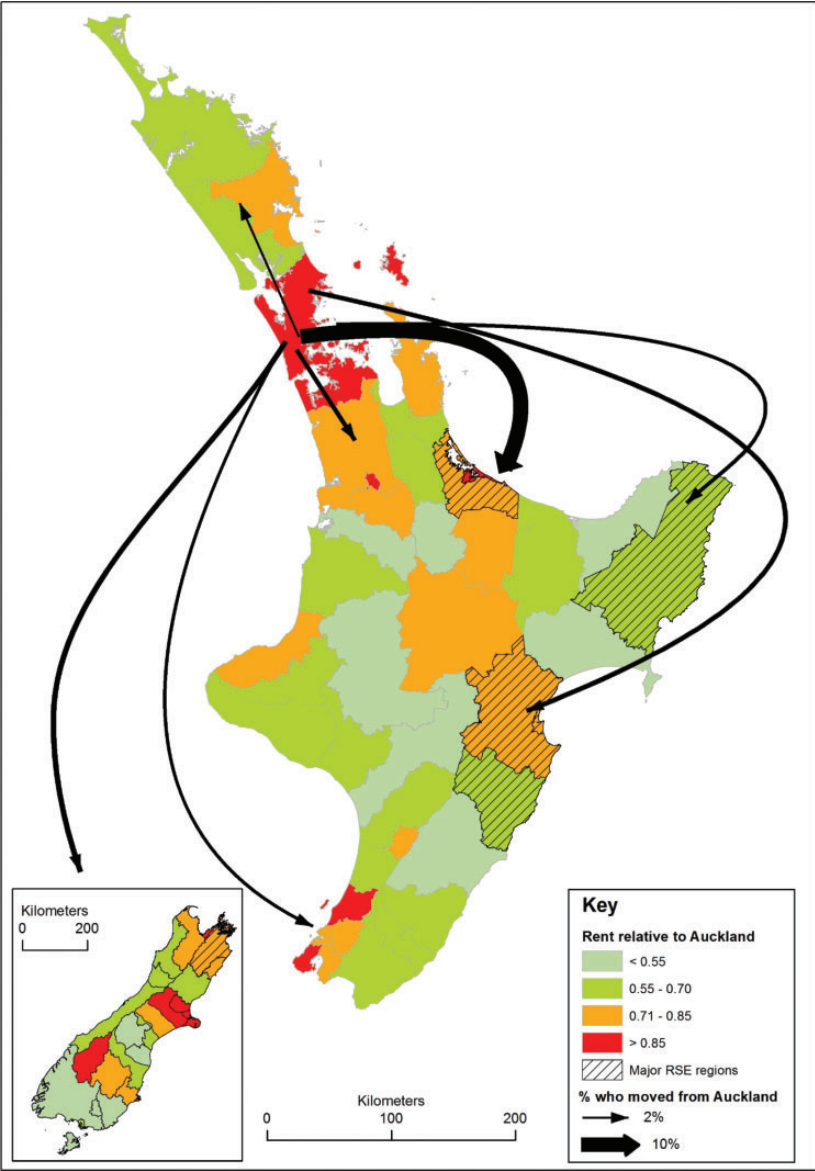
Source: Author's calculation from survey data described in text.

In columns 2 and 5, we use an alternative weighting scheme based on the information provided to us from cross-border passport scans, giving a higher weight to on-migrants than to ballot winners in New Zealand.¹⁴ In columns 3 and 6, we further allow for migration to New Zealand through other channels by giving twice the weight to the migrants among the ballot loser sample as to the non-migrants. Our estimated impact of migration on earnings is qualitatively unaffected by these different weighting choices.¹⁵

In panel B, we examine whether the results are robust to adjustments for cost of living differences between Tonga and New Zealand and between different regions in New Zealand. To examine differences in the cost of living between New Zealand and Tonga, we collected prices for a common basket of goods in both countries and calculated a PPP exchange rate. When we did this in 2005 (McKenzie et al. 2010), we found the exchange rate and PPP exchange rate coincided. Redoing these calculations in 2015, the PPP exchange rate was 1.62 pa'anga per New Zealand dollar while, over the course of the survey, the market exchange rate had varied from 1.43 to 1.54 pa'anga per New Zealand dollar. As a result, using the market exchange rate would slightly undervalue the gains to migration.

- 13 Migrant ballot winner earnings are distributed more like a normal than a log-normal since people with very high earning potential would likely have migrated through skill immigration categories. If log earnings are used, the sample size falls to $n = 310$, and the LATE coefficient is 1.121 (implying a 208 percent increase due to migration).
- 14 This assumes that, while our sample of on-migrants is proportionally too small compared with those still in New Zealand, this sample is representative of all on-migrants.
- 15 In other tests of robustness we estimated without weights, we adjusted non-complier incomes to have the same mean as ballot losers in Tonga, and we dropped all non-compliers and estimated equation (2) with OLS. The estimates of migration impacts were \$342, \$345, and \$347, compared to the \$340 in table 2. The lack of fragility in the estimates to different ways of dealing with non-compliers supports the finding of McKenzie et al. (2010) that there is little selection among non-compliers.

Figure 3. Movement of Migrants Out of Auckland over the Decade



The second adjustment was to put everything into Auckland prices based on a spatial cost of living index reflecting rental cost differences and a housing budget share of 44 percent. The majority of PAC migrants start by living in the Auckland metro area, but, over the ten years of our sample period, many moved to locations with cheaper living costs (see figure 3). The proportion of our sample located outside Auckland rose from 14 percent in the first survey in 2005–06 to 39 percent in 2013–14. Interestingly, the majority of migrants who left Auckland moved to regions that are the main locations for Tongan seasonal workers. It was not the case that PAC migrants had adopted an itinerant lifestyle and moved to the regions in search of seasonal work. Instead, in qualitative interviews, most said that they only became

aware of opportunities in these regions through the experience of the seasonal workers often mediated by extended family in Tonga. Some of the PAC migrants who moved to the regions also noted that they had worked in elementary occupations in Auckland, such as factory workers and cleaners, and did not expect to get back to the same occupations they had in Tonga (most commonly this was teaching, and also police and clerical work). Therefore, if they were going to do work of similar elementary status, they may as well get the benefits of a lower cost of living and less crowded accommodation by moving out to the regions.

These price adjustments increase the gain to migrating by five to six percent, which is only a small fraction of the overall gain of between 275 and 285 percent. By comparison, the estimated income gain approximately one year after migration was NZ\$308 (Stillman et al. 2015) in June 2006 currency (equivalent to NZ\$366 in current dollars), which was a 263 percent income gain.¹⁶ These results suggest that, almost a decade after migration, the impacts on earnings are much the same as they were in the first year. In other words, the economic payoff to migrating from a poor country to a richer one seems to come immediately and not grow substantially thereafter.

Given that the gain in migration appears to be a level effect of approximately NZ\$340 per week and that we do not observe return migration over the decade, we can estimate the lifetime income gain from winning the PAC ballot. The typical PAC migrant moves to New Zealand aged around 32. If we assume they work for 33 years thereafter, then the lifetime earnings gain is NZ\$583,440. This assumes all income is consumed, so that there is no investment return on this gain. In addition, they would then be eligible for New Zealand Superannuation (retirement benefit), which currently has an after-tax value of NZ\$288 per week for each spouse in a married couple. In contrast, Tonga's retirement benefits fund is a defined contribution fund that will offer much less.¹⁷ If we assume a retirement of 15 years, this is an additional benefit of NZ\$188,760, for a total lifetime benefit of NZ\$772,200. Using a five percent discount rate, this has a net present value of NZ\$315,000. This is conservative as it allows for no asset earnings and yet still represents a 48-fold increase.

Impacts of Migration on Other Individual Outcomes for the Migrant

Table 3 examines the impact of migration on other individual level outcomes. We examine changes in employment, occupation, subjective welfare, mental health, and education. Since these are outcomes in several different domains, it does not make sense to aggregate them into a single index. Instead, we allow for multiple hypothesis testing by noting whether the result is still statistically significant when using the Holm (1979) sequentially rejective Bonferroni method.

Column 1 shows a large impact on the likelihood of being employed, which survives corrections for multiple hypothesis testing. The LATE impact is a 30 percentage point increase in the likelihood of paid employment.¹⁸ Column 2 shows that migrants are more likely to have changed occupation in the past five years, but this impact is only significant at the ten percent level and does not survive correcting for multiple testing. Migrants who change occupation appear to maintain a relatively constant occupational status with an average value of 33 for the old jobs and 34 for the new jobs on the 10–90 point NZSEI scale. One-third of the occupational changes recorded amongst the migrants were to working on farms, in orchards, or in pack houses.

16 The four year impact reported in Stillman et al. (2015) is NZ\$367 with a 95 percent confidence interval of \$305 to \$428.

17 It is a defined contribution fund, with the worker and government each paying 5–7.5% of the worker's pay into an account with the worker then getting this benefit plus any earnings upon retirement. Taking 7.5% of the average income of ballot losers of NZ\$125/week, and assuming 2% annual income growth and 5% annual return, this benefit is equivalent to NZ\$35,807 at retirement, or NZ\$46/week for 15 years.

18 If we break the overall income gain into changes in intensive and extensive margins, 84% of the gain is due to higher earnings conditional on working, although this calculation assumes there is no selection as to who works.

Table 3. Impact on Outcomes for the Migrant

	Currently Employed	Changed Occupation in last 5 years	Change in occupation score	New occupation score	Old occupation score	Has Tertiary Education	Currently Studying	Subjective Welfare Ladder	Mental Health MHI-5
Panel A: ITT Impact									
Ballot Winner	0.217*** (0.070)	0.093* (0.052)	17.100*** (5.486)	11.935** (4.604)	-5.164 (3.541)	0.146** (0.066)	0.078** (0.031)	-0.011 (0.084)	0.687*** (0.259)
Panel B: LATE Impact									
Migrate	0.299*** (0.085)	0.128* (0.066)	21.908*** (6.655)	15.292*** (5.293)	-6.616 (4.687)	0.201** (0.091)	0.103*** (0.038)	-0.016 (0.115)	0.871** (0.371)
Sample Size	385	385	70	70	70	385	282	385	279
Control Mean	0.659	0.121	-16.952	23.143	40.095	0.181	0.000	5.852	17.475

Notes: Robust standard errors in parentheses, *, **, *** indicate significance at the ten, five, and one percent levels respectively.

All regressions include controls for age, gender, island of birth, ballot years entered, and survey form (long or short).

Regressions weighted to account for the population proportions of compliers and non-compliers.

New and old occupation scores are only available for those who changed occupation in the last five years.

Source: Author's calculation from survey data described in text.

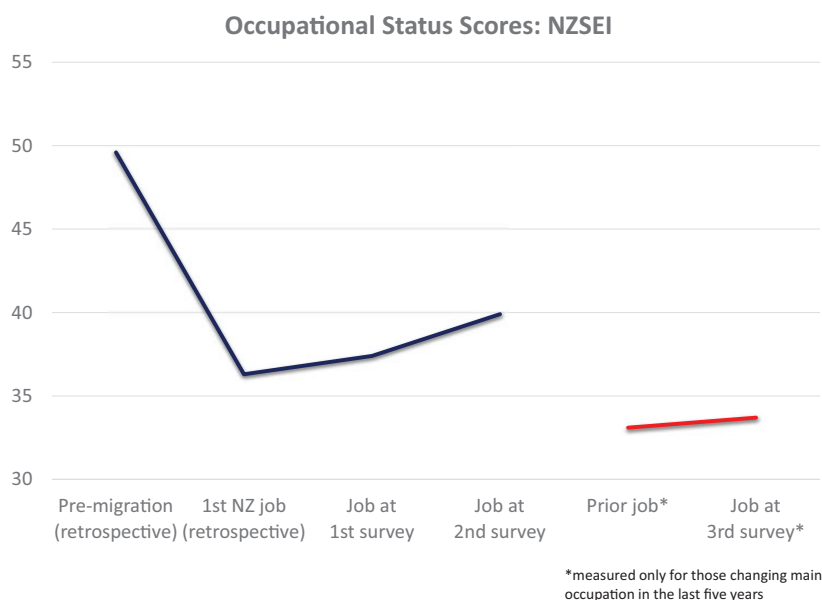
Figure 4. The “Reverse J-Shaped” Pattern of Occupational Decline for Immigrants

Figure 4 provides a summary of the occupational history of the PAC migrants from their premigration occupations in Tonga, their first job in New Zealand, and their occupations at the time of the first two surveys, which were roughly one and four years after they moved. The figure also includes the previous and current occupation at the time of the current (third) survey, but this is available only for those who had changed occupations in the last five years. Since the samples are changing over time, and there is a larger than five-year gap between the second and third surveys, this figure does not give a longitudinal account but should still approximate the pattern experienced by the average migrant.

There is a reverse-J pattern in figure 4, where occupational status falls with migration and does not recover. This contrasts with the U-shaped pattern suggested in the literature (e.g., Chiswick et al. 2005,

Akresh 2008), where the first job in the destination is of lower status than the last job in the home country but the migrant then regains some occupational status over time. However, this idea of a U-shaped pattern is also under challenge from other studies that note that the literature may be overly optimistic about immigrant convergence with natives. For example, Abramitzky and Boustan (2016) report that, both in the past and the present, long-term immigrants to the United States experienced occupational growth at about the same pace as natives, so those who held lower-paid occupations upon arrival did not catch up to natives over a single generation. More generally, the prior studies of occupational assimilation have used non-experimental methods and, hence, may be subject to the same critique as the wage assimilation literature, specifically that assimilation is overstated because of changes in cohort quality over time. If this is the case as well for occupational assimilation, this may explain why we find a reverse-J as opposed to a U-shaped pattern.

To provide some qualitative evidence on the dynamics of occupational change, we contacted PAC migrants included in the first survey of McKenzie et al. (2010) who had been teachers in Tonga, which was the most common prior occupation in that sample. All of these migrants had applied for jobs as teachers in New Zealand, but just half were successful; at the time of the third survey, a decade after migrating, the others were working as a social worker, a recruitment officer, farm supervisors ($n = 2$), a bus driver, and a farm worker. The ones who failed to regain their occupation from Tonga had lower premigration qualifications, holding a teaching diploma rather than a bachelor degree, fewer years of teaching experience in Tonga, and were less likely to have undertaken postmigration training in New Zealand. Specifically, all six who became teachers and the two whose occupational status did not fall so far (the social worker and the recruitment officer) invested in postmigration training. Since a teaching diploma is a higher level of education than is needed for a bus driver or farm worker, there appears to be some degree of ‘brain waste’ in this occupational downgrading, which may reflect poorly transferrable skills (Mattoo et al. 2008).

Despite largely static occupational status for the PAC migrants, the estimated impacts in columns 3 and 4 show a strong and significant impact of migration on occupational status for those who do change occupations. Migration leads to a 22-point increase in occupational status on the 80-point scale, and this impact remains significant after correcting for multiple hypothesis testing. The effect is driven by declining occupational status among ballot losers, where those changing occupations dropped from an average occupational score of 40 down to just 23. Some of this reflects people leaving professional careers to farm their ancestral lands and may be due to a relatively early retirement age of 55 in Tonga. Since many of the PAC migrants were public sector workers in Tonga and some are now in their early 50s, the correct counterfactual for what their occupations in Tonga would have been is unlikely to be a continuation of the premigration occupation. Therefore, a potential benefit of migration in this case is the movement to a labor market with fewer restrictions on older workers, where those restrictions in turn reflect a lack of employment opportunities for qualified young people.

The apparent lack of growth in postmigration wage impacts and in occupational status is despite a significant migration impact on human capital. Migrants are more likely to be currently studying (only observed in the long survey), and this is significant even after correcting for multiple hypothesis testing. They are 20 percentage points more likely to have attained tertiary education (which is typically from subdegree tertiary providers rather than from universities).¹⁹

19 This strong response of tertiary study for working-age adults may reflect some features of tertiary funding in New Zealand. Student loans are widely available, are nominal interest free as long as the borrower stays in New Zealand, and repayments are income-contingent. Specialized tertiary providers (called *Wananga*) have responded to these market conditions by providing tertiary study options for Maori, Pacific, and new settler groups who may not have the usual preparation for university study and now rival the largest university in terms of enrollments.

Columns 8 and 9 show two other welfare measures—the economic ladder and mental health (only in the long survey). Consistent with the short-term results in [Stillman et al. \(2009\)](#), there is an improvement in mental health with the ITT impact remaining significant after multiple testing corrections, although the magnitude of the long-term impact (0.87) is just under half of the one-year impact (1.94). In contrast, the economic ladder question shows no difference in subjective welfare between the migrants and those staying in Tonga, although these comparisons use unanchored scales and so may reflect changes in the frame of reference as discussed in [Stillman et al. \(2015\)](#).²⁰

Household-Level Impacts

The typical migrant lives in a nuclear household in New Zealand with 84 percent of the migrants living with a spouse and 80 percent having a child in the house. Conditional on having at least one child, the mean (median) oldest child in the household is 11.1 (12) years old. Household outcomes for the migrant therefore reflect the gain from migration on this family unit that is eligible to move with the migrant under the Pacific Access Category.

[Table 4](#) presents the estimated impacts of migration on household outcomes. Incomes and expenditures are transformed into logarithmic terms, since they are always non-zero whereas weekly earnings may be zero; thus, proportionate changes for these measures may be estimated as: $[\exp(\beta_i) - 1]$. We consider both total and per-capita impacts, although we do not find evidence of an impact of migration on household size. Migration leads to a 185 to 215 percent increase in household income and a 258 to 295 percent increase in household expenditure, with both effects statistically significant.

In terms of annual household labor earnings, migration leads to a gain of NZ\$27,000, which is almost four times as large as labor earnings in Tonga. This is greater than the gain in total income, reflecting that households in New Zealand have little own production while this is an important source of income for households in Tonga. Migrant households also have substantially more durable goods: they have 0.8 more vehicles, are 58 percentage points more likely to own a computer, 24 percentage points more likely to own a DVD player, almost all have microwaves and are 58 percentage points more likely to own a washing machine. The only durable asset where ownership does not increase with migration is cellphones. We see no significant impact on financial access as measured by having a bank account or ATM card with there being little room for improvement here due to 99 percent of ballot-loser households in Tonga having these. We also estimate migrant households to have 18 times the level of savings as ballot-loser households and to be consuming a more diverse diet. All these impacts are significant at the one percent level and survive corrections for multiple hypothesis testing. Overall, households of migrants are much better off.

A key reason given for migration is often to improve the welfare of one's children by providing them with access to better schooling opportunities. Schooling in Tonga is near universal through secondary education, and 100 percent of the 14 to 17 year olds in ballot-loser households in Tonga are currently attending school. The effect is therefore likely to occur in terms of tertiary education, where opportunities are much more limited in Tonga. However, most children are too young for this age range, so we cannot yet detect an effect.²¹

20 Anchoring of these scales using vignettes would be a useful topic for future research. [Stillman et al. \(2015\)](#) find that migrants report significant improvements in their subjective welfare when making retrospective comparisons with their pre-migration life but place themselves at the same point on an unanchored scale as do the non-migrants, and this discrepancy may reflect a changing frame of reference as they adapt to the destination country.

21 Only 28 of the 282 households in the long survey have a child in the 18 to 22 age range.

Table 4. Impact on Household Level Outcomes

Outcome	Sample Size	Control Mean	ITT Impact	LATE Impact
Log Household Income	282	9.488	0.791*** (0.134)	1.047*** (0.134)
Log Household Income per capita	282	7.987	0.866*** (0.152)	1.146*** (0.129)
Log Household Expenditure	282	9.185	0.962*** (0.122)	1.274*** (0.115)
Log Expenditure per capita	282	7.684	1.037*** (0.142)	1.373*** (0.104)
Household Size	385	5.264	−0.229 (0.241)	−0.385 (0.390)
Household Earned Income	385	6829	16182*** (2178)	27131*** (2216)
Remittances sent to Tonga	385	118	911*** (137)	1528*** (180)
Number of Vehicles	385	0.797	0.462*** (0.074)	0.775*** (0.122)
Household has a Cellphone	385	0.511	−0.114* (0.065)	−0.192* (0.107)
Household has a Computer	385	0.181	0.348*** (0.068)	0.584*** (0.118)
Household has a DVD player	385	0.104	0.140*** (0.037)	0.235*** (0.064)
Household has a Microwave	385	0.082	0.563*** (0.067)	0.944*** (0.075)
Household has a Washing Machine	385	0.385	0.344*** (0.073)	0.576*** (0.120)
Household has a Bank Account	282	0.986	0.015 (0.011)	0.020 (0.015)
Household has an ATM card	282	0.986	0.015 (0.011)	0.020 (0.015)
Total Savings	282	428	5944*** (1100)	7869*** (1102)
Number of Foods in Diet	282	7.385	1.571*** (0.239)	2.080*** (0.284)

Notes: Robust standard errors in parentheses, *, **, *** indicate significance at the ten, five, and one percent levels respectively.

All regressions include controls for age, gender, island of birth, ballot years entered, and survey form (long or short). Regressions weighted to account for population proportions of compliers and non-compliers.

Source: Author's calculation from survey data described in text.

Impacts on Extended Family

Finally, we consider the impacts of migration on one particular type of extended family, consisting of a household containing a parent or elder sibling of the PAC applicant. By defining the partner household in terms of a fixed relationship rule, it prevents incorrect inferences from conditioning on changes in household structure that occurred postmigration.

The impacts of having a child or younger sibling migrate through the PAC on these extended households are reported in [table 5](#). The only significant differences found are that families of PAC migrants receive more remittances and are slightly more likely to have bank accounts and an ATM card. However, the magnitude of additional remittances received is relatively low: NZ\$215 annually in additional remittances, which is less than one week of the income gain to migrants and only 1.4 percent of

Table 5. Impact on Partner Household Level Outcomes

Outcome	Sample Size	Control Mean	ITT Impact	LATE Impact
Log Household Income	282	9.447	−0.077 (0.097)	−0.109 (0.140)
Log Household Income per capita	282	8.213	0.030 (0.101)	0.043 (0.144)
Log Household Expenditure	282	9.126	−0.018 (0.097)	−0.025 (0.137)
Log Expenditure per capita	282	7.892	0.090 (0.115)	0.126 (0.165)
Household Size	282	3.720	−0.295 (0.234)	−0.415 (0.354)
Household Earned Income	282	7809	155 (1098)	218 (1542)
Remittances received	282	466	153* (78)	215** (97)
Number of Vehicles	282	0.678	0.052 (0.082)	0.073 (0.119)
Household has a Cellphone	282	0.364	−0.053 (0.085)	−0.075 (0.119)
Household has a Computer	282	0.119	0.032 (0.062)	0.045 (0.088)
Household has a DVD player	282	0.028	−0.005 (0.015)	−0.007 (0.021)
Household has a Microwave	282	0.063	−0.020 (0.024)	−0.029 (0.035)
Household has a Washing Machine	282	0.287	0.076 (0.081)	0.107 (0.118)
Household has a Bank Account	282	0.951	0.060** (0.026)	0.084** (0.036)
Household has an ATM card	282	0.951	0.062** (0.027)	0.087** (0.038)
Total Savings	282	545	194 (201)	273 (286)
Number of Foods in Diet	282	7.308	−0.269 (0.194)	−0.378 (0.282)

Notes: Robust standard errors in parentheses, *, **, *** indicate significance at the ten, five, and one percent levels respectively.

All regressions include controls for age, gender, island of birth, ballot years entered, type of relative of the PAC applicant, and for whether the partner co-resided with the applicant. Regressions weighted to account for the population proportions of compliers and non-compliers.

Source: Author's calculation from survey data described in text.

annual total household income for ballot- loser households. Given this small difference in remittances, it is unsurprising that we do not see impacts on other household outcomes for extended family.

This limited impact is in a context where the households that applicants lived in at the time of applying for the migration lottery would naturally be dissolving as adults leave their parental homes and set up households of their own. Even if these individuals had stayed in Tonga, most would be living in a separate dwelling from this extended family. As such, these extended households will have had to adjust to the absence of the applicant regardless of their migration outcome. One caveat that we should note is that it is possible that a key benefit of having a migrant abroad is as an insurance policy, someone that can provide help in the case of major shocks, something we are unable to measure with our data.

V. Conclusions and Discussion

A long-term follow-up survey of Tongans who applied to migrate to New Zealand through a visa lottery enables us to measure the impact of international migration after a decade. We find that the long-term income gain for migrants is similar to the short-term estimates. That the income gain occurs immediately upon migrating to a richer country suggests that the same labor and skills can be vastly more productive when used with the institutions and complementary physical and human capital present in a developed country rather than in a developing country. However, the gain in income is less than the percapita income gap between countries and does not appear to grow further despite various postmigration investments made by the immigrants in qualifications, occupational change, and internal mobility. Even if the income gain is reasonably static, it is so large that the lifetime benefit from migrating for the migrant and his or her accompanying spouse and children is enormous.

The effort and cost of tracking applicants down a decade after applying for migration, coupled with the migration lottery, make this study unique in its ability to provide estimates of the long-term causal impacts of migration. A question that then arises in any evaluation is the extent to which its findings may apply beyond the specific setting of the study. The findings here are most likely to generalize to the likely longer-term impacts of migration on people from small, island countries. Özden et al. (2011) note the salience of such places: “the origin countries most affected by international migration are small, typically island states, mostly in the Pacific or the Caribbean. The destination countries most affected by migration are the countries of the New World (the United States, Canada, Australia, and New Zealand). . . .”

In addition, two other factors make Tonga-New Zealand migration of potential interest beyond the methodological value provided by the lottery-determined migration flow. First, it may provide some insight into on-migration because the immigrants have unrestricted entry to Australia under the Trans-Tasman Travel agreement once they obtain New Zealand permanent residence. The easy access to an economy that is over five times larger sees New Zealand normally lose about one percent of its residents per year to Australia (although this flow has recently reversed), which it then replaces with immigrants (in both countries, one-quarter of the resident population are foreign born). There are many other examples of on-migration, such as the *de facto* free mobility for third-country nationals once within the borders of the Schengen area (Koikkalainen 2011). This on-migration provides one reason why short-term and long-term gains from migration may differ. Moreover, on-migration reduces the value of using destination country natives as a comparison group for studying labor market trajectories of migrants, since the migrant experience may span several destination countries.

The second factor of potential interest is that New Zealand opened a much larger seasonal work option in 2007 for up to 9000 migrants per year in horticulture, with preference for Pacific Islanders. About 1500 Tongans come to New Zealand each year under this scheme (Gibson and McKenzie 2014a); more than six times the number coming via PAC settlement migration. There is no formal interaction between these two migration pathways (seasonal workers are told they have no option to settle), and they select from different parts of the skills distribution in Tonga with the PAC applicant pool positively selected (McKenzie et al. 2010) and the seasonal workers negatively selected by a focus on people not in formal employment (Gibson and McKenzie 2014b). Moreover, the seasonal workers go to regional New Zealand while Auckland is the first destination for most PAC migrants. Yet our surveys and follow-up qualitative interviews uncovered at least two linkages; since many Tongans come to regional areas on a rolling basis throughout the year for seasonal work, it creates a derived local demand for settlement migrant Tongans. Second, information about opportunities in regional centers appears to flow from seasonal workers to settlement migrants indirectly through family and other contacts in Tonga; this stickiness of information corroborates findings of McKenzie et al. (2013) that migrants may

not be well informed about opportunities in destination countries. Other countries are also expanding seasonal and temporary migration, so these interactions with settlement migration may be of interest, especially because they provide another lens for comparing short-term and long-term gains from migration.

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