

THE IMPACTS OF INTERNATIONAL MIGRATION ON REMAINING HOUSEHOLD MEMBERS: OMNIBUS RESULTS FROM A MIGRATION LOTTERY PROGRAM

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Abstract—We use a migration lottery program to overcome the double-selectivity problems posed by migration. We compare a wide range of outcomes for the remaining household members of Tongan emigrants with those of members of similar households who were unsuccessful in the lottery, with the policy rules determining which household members can move. Multiple hypothesis testing procedures are used to examine robustness. The overall impact on households left behind is largely negative in terms of resource availability, and both sources of selectivity matter, leading studies that fail to address them adequately to misrepresent the impact of migration on households.

I. Introduction

THE impacts of international migration on development in the sending countries, and especially the effects on remaining household members, are increasingly studied. Empirical analysis is needed because the effect of migration on development in source communities is a priori unclear. Migrant-sending households and their communities can benefit from remittance inflows, which now make up 30% of total financial flows to the developing world, but earnings and other household inputs that migrants would have generated locally are lost. Even more studies are likely in the future as new survey data become available and labor mobility increases in response to growing international wage gaps, rising demand for services, divergent trends in youth and elderly populations in developed and developing countries, and catch up from the previously “everything but labor” nature of globalization in the post–World War II era (Pritchett, 2006).

The biggest difficulty in measuring impacts of migration on development is posed by selectivity issues. A common research strategy in this literature is to use household survey data to compare households that have had at least one member emigrate to those that have not. Such comparisons are complicated by a double-selectivity problem: first, households self-select into migration, and second, among households involved in migration, some send a subset of members with the rest remaining while other households migrate en masse.

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In this paper, we address these selectivity issues using the randomization provided by an immigration ballot under New Zealand’s immigration policy. We survey applicants to this random ballot and compare outcomes for the remaining household members of emigrants with those for members of similar households who were unsuccessful in the ballot. The policy rules determine which household members can accompany the principal migrant, providing an instrument to address the second selectivity issue. Since this migration channel has only recently opened, we measure only the short-term impact of migration. This may be when household challenges are greatest, as they adapt to the absence of household members and have yet to receive large remittances.

The particular policy we focus on is the Pacific Access Category (PAC), which was established in 2001 and allows an annual quota of 250 Tongans to immigrate as permanent residents to New Zealand without going through the usual channels used for groups such as skilled migrants and business investors. Many more people apply than the quota allows, so a ballot is used by the New Zealand Department of Labor (DoL) to randomly select from among the applicants. The probability of success in the ballot is approximately 10%. We evaluate the impact of individuals migrating to New Zealand via the PAC on household members remaining in Tonga (mainly parents, siblings, and nephews and nieces of the migrant applicant). Both our data and the previous literature indicate that Tongan households as a rule pool resources. However, in addition to examining household-level impacts (income, durable assets, financial service use, and diet) for which an assumption about intrahousehold allocation is required to judge changes in well-being, we also examine individual impacts (employment, schooling, mental and physical health), which do not require knowledge of the household-sharing rule to judge changes in well-being.¹

Under the assumption that resources are pooled within households prior to migration, our results suggest that at least in the short run, there may be some adverse consequences for those left behind when a subset of their household migrates to New Zealand. Income falls by approximately 22% to 25%, whether measured per capita or per

¹ In an earlier paper published in a conference volume (McKenzie, Gibson, & Stillman, 2007), we used the same data set to estimate the experimental impact of migration on poverty, household size, and total income. The current paper also considers household size and total income as 2 of the 62 outcomes considered in this paper. Despite this small overlap, the current paper differs significantly from our earlier work. In addition to looking at many more outcomes, the current paper is the first of our work (and the first in the migration literature) to explicitly note the double-selectivity issue caused by migration and show the bias that this causes in non-experimental results and the first to examine the importance of using multiple hypothesis testing for interpreting the results.

adult equivalent, with a rise in net remittances failing to offset a large fall in labor earnings. Ownership of livestock, durables, and access to financial services is also lower for the remaining household members than for the control group. Diets change, with less fruit, vegetables, and fats consumed and more rice and root crops. However, given the high incidence of obesity among Tongan households, this reduction in resources actually leads to some beneficial health changes, with the body mass index and waist-to-hip ratio declining for working-age adults.

We also use data from a sample of nonapplicants and from ballot losers in households that would entirely move if they had been successful in the PAC ballot to examine the degree of selection of households into migration and selection among households with a migrant as to which would partially move and which would move en masse. We find that selection is important in both dimensions. Thus, nonexperimental estimates of migration impacts are biased. These results are confirmed using two large-scale general-purpose surveys of the sort mostly commonly used in the migration literature. In particular, nonexperimental estimation leads one to conclude that emigration has made remaining household members wealthier, whereas the natural experiment shows the opposite result. In contrast, we find evidence that the individuals who remain behind when some household members positively self-select into migration are not that different from the general population, and so the nonexperimental estimates are closer to the experimental ones when examining individual-level impacts.

These results should have broader applicability since Tongan migrants to New Zealand under the PAC are quite typical of developing country migrants to the United States in terms of both their levels of education and the degree of educational self-selection relative to nonmigrants (McKenzie, Gibson, & Stillman, 2010). Moreover, although a stereotype is of a husband migrating alone and leaving a family behind in a developing country, a majority of married developing country immigrants in the United States actually have their spouse present, similar to our setting.² Immigration policies in many countries worldwide (for example, Australia, Canada, Ireland, the United Kingdom, France, and Italy) allow individuals moving on an employment visa to bring their spouse and dependent children, but not to immediately bring their parents or adult siblings. The United States also allows parents to accompany the migrant, but not adult children or siblings. Consequently, the impacts on household structure and other outcomes for the families of those who move may be quite similar in many other migrant-sending countries to what we observe among the Tongans left behind when family members emigrate to New Zealand.

² Specifically, using the 5% public use sample of the 2000 U.S. Census, we find that 59% of married immigrants from developing countries who arrived in the United States in the previous year also had their spouse present. Even for Mexico, we find that 46% of newly arrived married immigrants have migrated with their spouse.

In section II, we review relevant literature on the impact of emigration on source areas and discuss channels through which emigration may affect household members left behind. section III provides background on the immigration program we examine, and section IV describes the data from the Pacific Island–New Zealand Migration Study (PINZMS) and our estimation methods. The impacts on household-level outcomes are presented in section V and on individual outcomes in section VI. Section VII discusses multiple hypothesis testing, while section VIII concludes and draws out general lessons for other studies.

II. Previous Literature

A. Channels and Impacts

The most studied impact of migration on household members left behind has been the impact on remittances received. There are a variety of reasons that migrants send remittances, including altruism toward those left behind, exchange for a variety of services provided by the remaining family members (such as caring for property or other relatives), repayment of loans made to finance migration or education, and insurance and strategic motives (Rapoport & Docquier, 2006). These remittances directly contribute to household income, allowing households to purchase more assets and buy more normal goods, including education and health inputs. They can also relax liquidity constraints, enable greater household investment in businesses and children's education, and allow households to better mitigate the impact of domestic shocks.

If migration resulted purely in an exogenous increase in income for the remaining household members, the sign of the expected impact on many outcomes of interest would be easily determined. However, migration can also have a number of other impacts on the sending household. Most obvious, an absent migrant earns no domestic wage and provides no time inputs into household production. These effects may counteract the effect of remittances received, so, for example, households may have less time to spend educating children but perhaps more money to spend on them. Migrants may also transfer knowledge and attitudes to their remaining family members. For example, Hildebrandt and McKenzie (2005) find contraceptive knowledge to increase with emigration of household members from Mexico to the United States. Absence of decision makers may also lead to changes in the bargaining power of remaining members in the household, leading to a reallocation of household spending priorities (Chen, 2006). Separation from family members may have an impact on mental health. Finally, migration of some family members may make it more likely that others will migrate in the future, changing the incentives to acquire education. The result of all of these different potential channels is that the overall impact of migration on various measures of the welfare of remaining family members is theoretically uncertain.

B. Selection and Identification

The main challenge facing empirical analysis of the impacts of migration and remittances on sending households is a double-selectivity problem. First, households choose whether to engage in migration.³ Households that send migrants are likely to differ along a number of observable and unobservable dimensions from households that do not send migrants, with some of these characteristics likely correlated with outcomes of interest. For example, an unobserved asset shock may make the sending household poorer and encourage emigration. Households with aptitude and knowledge of foreign languages may be more inclined to engage in migration and also have children who do better in school. Second, among households that decide to engage in migration, some decide to move with their entire families, while in others, only some members emigrate.⁴ A third form of selection that also occurs in many contexts is selection into which migrants return. Since we are examining the short-run impacts of migration, this source of selectivity is not an issue here.⁵

We are not aware of any study of the impact of migration on sending households that explicitly deals with the second form of selection, since almost all developing country migrant data sets lack information on entire households that move. The literature has used a variety of approaches to address the first form of selection. Examples include assuming selection on observables (Adams, 1998; Cox-Edwards & Ureta, 2003), parametric selection correction models (Barham & Boucher, 1998; Acosta, Fajnzylber, & Lopez, 2007), propensity-score matching (Esquivel & Huerta-Pineda, 2007), instrumental variables methods, predominantly using current migration networks (Mansuri, 2006; Brown & Leevy, 2007) or historic networks as instruments (Woodruff & Zenteno, 2007; McKenzie & Rapoport, 2007),⁶ and work by Yang (2008), which uses a natural experiment provided by exchange rate shocks in destination

countries to look at impacts within the group of households with migrants abroad.

However, one may question the identification assumptions underlying these nonexperimental approaches to constructing no-migration counterfactuals. There is evidence that migrants self-select in terms of both observables and unobservables (McKenzie et al., 2010; Akee, 2010), so methods that assume selection on observables (which include OLS and matching) are likely to be biased. This is particularly the case since most studies rely on cross-sectional surveys in the sending country, which typically have very limited data on the situation of the households prior to migration. This limits the ability of researchers to match on premigration characteristics. For example, Esquivel and Huerta-Pineda (2007), the only published example of matching in the literature that we are aware of, match on demographic characteristics of the household head, geography, number of rooms in the house and whether it has a bathroom, and household size and number of women in the household (which, as we shall see below, changes with migration and so should be used to match on).

Selection correction methods rely on parametric structure and dubious excludability assumptions. For example, Acosta et al. (2007) and Barham and Boucher (1998) assume that household asset holdings predict selection into migration but do not directly affect earnings or labor force participation. However, these assets could be used to help finance businesses or could be the result of labor earnings. The use of current migration networks as an instrument is subject to concerns about other variables at the community level that also affect migration and outcomes of interest. For example, a recent community weather shock such as a drought may have led to both increased migration and a reduction in agricultural income in the community. Historic networks are less subject to concerns about recent shocks but still need to rely on a plausible story of why networks exogenously formed in one location and not in another, such as the pattern of development of the railroad system in Mexico, as used by Woodruff and Zenteno (2007). The natural experiment that Yang (2008) used provides the cleanest identification of the impact of changes in remittance receipts among households receiving remittances but is unable to examine the other channels through which migration can affect households.

C. On Which Household Outcomes Does the Literature Focus?

The growing literature on the impact of migration and remittances has examined a variety of outcomes, all intended to measure the extent to which migration can aid “development” in the sending countries. However, each study typically focuses on the impact of migration on only a few (often one) outcomes in the sending country, preventing analysis of the full range of impacts of migration on households in any one sending country. Common outcomes

³ To be precise, they choose whether to engage in migration given the existing policy environment. In most cases, this is a policy environment that also involves substantial selection from the receiving country side, with employers and government officials screening interested potential migrants to determine which ones can actually move. Since there is less screening at destination in our case than in the case of much other legal migration, the degree of self-selection is likely to be less in our example than in cases where employers and governments are also involved in selecting the migrants. As a result, our results will be, if anything, conservative in terms of showing the potential bias from self-selection.

⁴ A further issue for some of the literature is the attempt to distinguish the impact of remittances from the overall impact of migration. See McKenzie (2005) for a critique of this approach.

⁵ None of the PAC migrants had returned to live in Tonga during the period of our study.

⁶ Other instrumental variables have also been used, but the exclusion restriction underlying these are perhaps less convincing than the historic network variables. For example, Amuedo-Dorantes and Pozo (2006a) assume that the number of Western Union branches in a state in Mexico affects labor supply only through current migration, when these branches are likely to have been established as the result of factors that have driven migration historically, including the level of development in a state, which likely also has an impact on labor supply.

of interest include income and poverty levels, employment and business ownership, child health and education, and asset ownership. These outcomes are of inherent interest and also the most commonly available measures in household surveys.

Existing evidence paints a generally rosy picture of the impact of migration on the incomes, asset holdings, and poverty levels of household members left behind (Adams, 2007, provides a recent review). These studies generally attempt to construct a no-migration or no-remittance counterfactual by estimating what the income of the household would be without remittances but with the migrant working in the home country (Barham & Boucher, 1998; Adams, 1998).

Among the few studies of the impact on child health outcomes, all show positive effects, although more mixed results when examining inputs. For example, Hildebrandt and McKenzie (2005) find lower infant mortality rates and higher birth weights among Mexican migrant-sending families, but also that children in migrant households are less likely to be breast-fed or be vaccinated. Acosta et al. (2007) find higher weight for age and height for age among children in migrant families in Nicaragua and Guatemala.

The existing literature finds ambiguous effects of migration on several other key outcomes of interest. In terms of the effect on child education, Cox-Edwards and Ureta (2003) find that migration increases school attendance rates in El Salvador, and Yang (2008) finds that increased remittances lead to more schooling in the Philippines, both consistent with higher income alleviating liquidity constraints, whereas McKenzie and Rapoport (forthcoming) find that migration lowers schooling attainment in Mexico, with boys in migrant households more likely to drop out of school to migrate and girls undertaking more housework.

Evidence is also mixed in terms of the impact on adult employment. Funkhouser (1992) finds remittances to be associated with lower overall labor supply but higher self-employment in Nicaragua. Acosta (2006) finds a negative impact on female labor supply in El Salvador but no effect on male labor supply. Yang (2008) finds that higher remittances lead to households being more likely to engage in entrepreneurial activities and to spend more hours in self-employment but have no significant effect on overall labor supply. Amuedo-Dorantes and Pozo (2006a) find remittance receipt lowers female labor supply in Mexico and shifts male labor supply from formal to informal sector work. Woodruff and Zenteno (2007) find remittance receipt to significantly increase the amount of capital invested in micro-enterprises in Mexico, whereas Amuedo-Dorantes and Pozo (2006b) find a significant negative impact of remittances on business ownership in the Dominican Republic.

In this paper, we consider these outcomes, along with other welfare outcomes such as diet, anthropometric health measures, and mental health, which are less often measured in household surveys and for which we have not been able to identify existing literature. For example, a recent submis-

sion to the Global Commission on International Migration states (Carballo & Mboup, 2005, p. 5) that “for close family and relatives left behind, the departure of migrants to seek a living elsewhere is also fraught with psychosocial difficulties,” but provides no evidence for this assertion.

In addition, Aggarwal, Demirgüç-Kunt, and Peria (2006) have recently used cross-country panel data to show an association between remittances and financial development, with the argument being that the receipt of remittances paves the way for recipients to demand and gain access to other financial services, even if the funds themselves are not received through banks. However, they note that remittances may instead substitute for use of credit and other demands for bank accounts, so that the direction of causation is unclear. Furthermore, it is possible that household members who use the banking system are more likely to migrate, reducing household use of bank accounts when they leave. We therefore also consider measures of access to bank accounts as another outcome measure.

III. Context and the Pacific Access Category

A. Background

The Kingdom of Tonga is an archipelago of islands in the South Pacific, with the capital a three-hour plane flight from New Zealand. The population is just over 100,000, with a GDP per capita of approximately US\$2,200 in PPP terms. One-third of the labor force is in agriculture and fishing, with the majority of paid workers in the manufacturing and services sectors, which are dominated by the public sector and tourism.

Emigration levels are high, with 30,000 Tongans living abroad, 94% of them in New Zealand, Australia, and the United States. Migration to New Zealand began in sizable numbers during the 1960s and 1970s, with Tongans arriving on temporary permits to take up work opportunities. After their permits expired, some returned to Tonga, and others stayed on in New Zealand illegally. An amnesty in 1976 granted many of these individuals permanent residence. Migration for work continued in the late 1970s and 1980s. In 1991, New Zealand introduced a selection system for immigration, in which potential migrants are awarded points for education, skills, and business capital. Few Tongans qualified to migrate under this system, and so most Tongan migration since this time has been under family-sponsored categories. For example, in 2004–2005, only 20 Tongans were admitted as principal applicants under the points system, compared to 349 under family categories, the majority through marriage or as dependent children. Migration to Australia and the United States has also become much more restrictive and reliant on family reunification categories. Australia admitted 284 Tongans during the 2004–2005 financial year. The United States admitted 324 Tongans in the 2004 calendar year, comprising only 5 under employment-based preferences, 290 under immediate

relative or family-sponsored categories, 23 diversity lottery winners, and 6 in other categories.

B. The Pacific Access Category

In 2002, another channel was opened up for immigration to New Zealand through the creation of the Pacific Access Category (PAC). This allows a quota of 250 Tongans to emigrate to New Zealand each year without going through the usual migration categories used for groups such as skilled migrants and business investors.⁷ Specifically, any Tongan citizens aged between 18 and 45, who meet certain English, health, and character requirements,⁸ can register to migrate to New Zealand. Many more applications are received than the quota allows, so a random ballot is used by the New Zealand DoL to draw from among the registrations. The odds of having one's name drawn were approximately one in ten during the period we study.

Once their ballot is selected, applicants must provide a valid job offer in New Zealand (unskilled jobs suffice) within six months in order to have their application to migrate approved. After a job offer is filed along with their residence application, it typically takes three to nine months for an applicant to receive a decision. Once receiving approval, they are then given up to one year to move. The median migrant in our sample moved within one month of receiving their residence approval.

The person who registers for the PAC is the principal applicant. If this person is successful, his or her immediate family (spouse and dependent children up to age 24) can also apply to migrate as secondary applicants. The quota of 250 applies to the total of primary and secondary applicants and represents about 80 migrant households. Successful applicants cannot take other members of their household to New Zealand, so anyone living with parents, siblings, or other relatives leaves household members behind when they migrate.

These two features of the PAC, random selection among applicants and a rule specifying which family members can and cannot accompany the successful migrant, allow us to address the double-selectivity issues involved in assessing the impact of migration on the remaining household. In particular, we can compare the group of households in Tonga with a PAC emigrant to the group of unsuccessful ballots who would not be eligible to move their entire household to New Zealand had their principal applicant been chosen in the ballot.

⁷ The PAC also provides quotas for 75 citizens from Kiribati and Tuvalu. A similar scheme, the Samoan Quota, allows 1,100 citizens of Samoa to move each year. There have been some small changes in the conditions for migration under the PAC since the period we examine in this paper. Here we describe the conditions that applied for the potential migrants studied in this paper.

⁸ Data supplied by the New Zealand DoL for residence decisions made between November 2002 and October 2004 reveal that out of 98 applications, only 1 was rejected for failure to meet the English requirement and only 3 others were rejected for failing other requirements of the policy.

IV. Data, Methods, and Selection

A. Data

The main data used in this paper are from the Tongan component of the PINZMS, a comprehensive survey designed to measure multiple effects of migration, taking advantage of the natural experiment provided by the PAC.⁹ The survey design and enumeration, which we oversaw in 2005–2006, covered random samples of four groups of households that we describe here. The survey included questions on household demographics, education, labor supply, income, asset ownership and diet, self-reported health status, smoking and alcohol use, and anthropometric measurements of height and weight for all individuals and waist and hip circumference and blood pressure for adults. It also measured mental health for individuals aged 15 and older using the Mental Health Inventory 5 (MHI-5) of Veit and Ware (1983).

In a perfect randomized experiment, the impact of the treatment (here, having some household members emigrate) can be obtained by comparing means for each group. But mean comparisons may be biased if control group members substitute for the treatment with a similar program or if treatment group members drop out (Heckman et al., 2000). For example, substitution bias will occur if PAC applicants who are not drawn in the ballot migrate through alternative means, and dropout bias will occur if PAC applicants whose names are drawn in the ballot fail to migrate to New Zealand. Substitution bias is of little concern here; the low odds of winning the ballot and the limits on eligibility for other migration channels available to Tongans mean that those with the ability to migrate using other arrangements should have done so previously. But dropout bias is a more relevant concern because approximately 15% of ballot winners do not ultimately move to New Zealand.

To adjust experimental estimates for possible dropout bias, we use three subsets of the PINZMS sample (see table A1): (a) 61 households, with 283 individuals, in Tonga with some previous members who are now PAC migrants in New Zealand, referred to as the treatment group; (b) 26 households, with 115 individuals, containing successful participants from the same PAC ballots who were still in Tonga—referred to as noncompliers—who had not moved when surveyed because their application for New Zealand residence was not approved (typically because of lack of a job offer) or was still being processed; and (c) 124 households, with 654 individuals, containing unsuccessful participants from the same ballots who were still in Tonga. Those in group c are the “control” group and were typically selected from the same villages that the sampled PAC migrants had lived in prior to moving. The two samples of successful ballots have a much higher sampling rate than the sample of unsuccessful ballots (expansion factors of approximately 3.4, 2.5, and 37.9 are needed to weight each

⁹ See <http://www.pacificmigration.ac.nz> for more details of the survey.

sample up to the relevant population), and all of the analyses take this into account.

Finally, we use a fourth subsample from PINZMS to examine selection into migration and carry out nonexperimental estimation of the impacts of migration. This sample consists of 124 households, with 727 individuals, where no member of the household applied for the PAC but where there was an adult in the 18 to 45 age range, which is the basis of PAC eligibility. These households were randomly chosen from the same villages as the PAC households and administered the same questionnaire. This gives us a sample that is implicitly matched on geography and age range and is working in the same local labor markets as the migrants.

For comparison purposes, we also calculate nonexperimental estimates by comparing our migrant households to two large nationally representative household surveys that are more typical of the standard, general-purpose surveys that much of the existing literature has had to rely on to look at the impact of migration. These are the Tongan Labour Force Survey (TLFS) of 2003–2004, which contains data on 11,152 individuals in 2,121 households, and the Tongan Household Income and Expenditure Survey (HIES) of 2000–2001, with data on 8,967 individuals from 1,627 households. In both cases, we use the Tongan CPI to convert values to Tongan currency, the pa'anga, at the time of our survey. In common with other labor force and income and expenditure surveys, these surveys have collected data on a narrower range of outcomes than PINZMS. From the point of view of migration to New Zealand through the PAC, it seems appropriate to think of both surveys as providing a pool of nonmigrant households.¹⁰

At the time of our survey, the sampled Tongan households with PAC emigrants in New Zealand had a mean (median) time abroad for their former household members of ten months (eight months). Just over three-quarters (77%) of migrant-sending households were interviewed less than one year after eligible household members had emigrated to New Zealand. Thus, our analysis examines the initial impact of sending emigrants. The use of a homogeneous period of time abroad allows us to avoid averaging short- and long-run effects, which may differ in sign (as found in Lucas, 1987).

B. Movers and Stayers

We use the age and relationship rules governing which secondary applicants can move with the principal applicant to identify household members that who would have moved to New Zealand if the principal applicant had been successful and compliant with the treatment. These rules appear to be the binding constraint since the remaining family members of PAC emigrants are almost all outside the age and

relationship eligibility for moving to New Zealand (see table A1).¹¹ Since the treatment group with migrants does not have cases where the whole household moved, neither should the control group or noncomplier group. We therefore drop 75 unsuccessful households and 18 noncomplier households in which their age and relationship structure would have allowed all members to move to New Zealand. Note that 60% of the unsuccessful ballots fall into this category.

Individuals in these all-move households, plus those who would have moved from the partial-move control group and noncomplier households and the few eligible individuals in treated households who did not immediately move to New Zealand, are all dropped for the individual-level analyses. Thus, only similar individuals in the treatment, noncomplier, and control groups are compared to each other for our experimental estimates. We define these stayers to be the individuals whom the legal rules would require to stay behind if their principal applicant had been successful in the PAC ballot.

The remaining household members of PAC emigrants typically contain working-age adults who are either the parent or the siblings of the principal applicant, along with children who are often their nephews and nieces. Specifically, 46% of migrant households contain a parent of the principal applicant, and 52% have a sibling. Just over half (57%) of other relatives are under age 18, and are mostly nephews and nieces of the principal applicant. Very few of these extended family members appear to have joined the household since the emigrants left,¹² and so as original household members, their welfare is likely to have been affected by the departure of the PAC emigrants.

As we noted in section I, these remaining household members are likely to be similar to the household members remaining in many countries when migrants move to developed countries through employment categories. With the exception of the United States, which also allows parents, all traditional immigrant-receiving countries restrict the relatives who can accompany a migrant to the spouse and dependent children. While in some cases emigrants can later sponsor their parents or siblings, they cannot do this until they have spent several years in the country, and even then there can be restrictions or long waiting periods.¹³ Thus, in the

¹¹ Specifically, just 11 (of the 283 residents of treatment group households) eligible family members stayed in Tonga rather than immediately move to New Zealand with their principal applicant. Those who did were mainly very young children and their mothers, who eventually moved after our survey, when the children were at a more suitable age for travel.

¹² We ask about how many of the previous twelve months each person was attached to the household. The number of recent members who had been attached for less than twelve months was slightly lower (0.48 versus 0.63) for migrant families than for those with unsuccessful ballots. We do not know for all households who was attached to the household at the time the ballot result was announced since this is outside the twelve-month window for many. However, given the low turnover in household composition, this does not appear to be a concern.

¹³ For example, several countries employ a "gravity" principle, which allows parents to be sponsored only if they have no remaining children in the home country, and then impose income requirements on the sponsoring migrant. In general, parents are still easier to sponsor than siblings.

¹⁰ The HIES survey was collected before the PAC was introduced, while the TLFS was run before most of those with successful ballots had moved to New Zealand, and the number of successful ballots is small relative to the overall population.

TABLE 1.—TESTS OF RANDOMIZATION

	Successful Ballot	Unsuccessful Ballot	T-Test, p-Value
Stayer household characteristics ($n = 118$)			
Size of the stayer household	4.2	3.3	0.068
Number of adults 18–45 among stayers	1.5	1.5	0.928
Number of children <18 among stayers	1.6	0.8	0.005
Number of adults >45 among stayers	1.1	1.0	0.726
Proportion of adults 18–45 who are female	0.53	0.52	0.949
Annual labor earnings of stayers in 2004	4,118	5,337	0.419
Characteristics of stayer children ($n = 146$)			
Proportion female	0.45	0.58	0.150
Mean age in months	91	114	0.189
Characteristics of stayer working-age adults ($n = 176$)			
Proportion female	0.53	0.49	0.578
Mean age	29.4	27.5	0.172
Mean height	167	168	0.693
Born on Tongatapu	0.79	0.68	0.394
Mean years of education	11.0	10.7	0.456
Visited New Zealand prior to 2000	0.14	0.10	0.388
Weekly labor earnings in 2004	46	48	0.903
Characteristics of stayer older adults ($n = 121$)			
Proportion female	0.57	0.55	0.777
Mean age	61.2	58.5	0.177
Mean height	167	165	0.750
Born on Tongatapu	0.76	0.72	0.659
Mean years of education	9.7	9.3	0.382
Visited New Zealand prior to 2000	0.40	0.27	0.141

T-tests account for household level-clustering. Successful ballots include those in migrant households and in noncomplier households.

short run, the remaining family of migrants is likely to be anyone apart from their spouse and dependent children.

C. Verifying Randomization

We first test whether the PAC ballot correctly randomizes stayer households into a treatment and a control group by examining whether the stayer group within the households containing ballot losers statistically differs from the stayer group in households containing ballot winners (both the migrant families and the noncompliers). The results in table 1 show that most ex ante premigration characteristics are the same for ballot winners and losers (at 95% confidence level). The only exception is that there are more children among the stayer group in successful ballot households. We present all regression results with and without controls for the characteristics of these stayer members to examine the robustness of our findings to small sample differences in the treatment and control group.

D. Calculating Experimental Estimates

Throughout the remainder of the paper, when we present experimental estimates of the impact on households and individuals of having household members move to New Zealand under the PAC, we do not directly compare means of the treatment and control groups due to concerns about dropout bias from noncompliers. Instead, instrumental variables regression (IV) models, where ballot success is used as an instrument for having coresidents emigrate, are used

to estimate the treatment effect on the treated.¹⁴ The PAC ballot outcome can be used as an excluded instrument because randomization ensures that success in the ballot is uncorrelated with unobserved individual attributes that might also affect outcomes among the stayer household members, and success in the ballot is strongly correlated with migration.

E. Looking for Evidence of Selection

In addition to obtaining consistent estimates of the impacts of migration, one of the other goals of this paper is to examine how these estimates might change if we were unable to correctly control for the double selectivity. Tables 2 and 3 examine the evidence for selection in terms of the household and individual outcomes of interest, respectively. As we have noted, the appropriate comparison group for the remaining individuals in migrant households is the group of individuals residing in ballot-loser households who would

¹⁴ While an IV regression usually estimates the local average treatment effect (LATE), Angrist (2004) demonstrates that in situations where no individuals assigned to the control group receive the treatment (i.e., there is no substitution), the IV-LATE is the same as the average treatment effect on the treated. We focus on the average treatment effect since this is the parameter we can cleanly identify and gives the overall impact of migration. With a larger sample, we could examine the average treatment effect for subgroups of households, such as those from poorer backgrounds or single versus married applicants, or those with different household compositions. We see such analysis of the heterogeneity of migration impacts as a fertile area for future studies should other migration lottery data be able to be collected.

TABLE 2.—SELECTIVITY IN HOUSEHOLD CHARACTERISTICS AMONG WHO APPLIES AND WHO MOVES WITH THEIR WHOLE HOUSEHOLD

	Sample Means				<i>p</i> -Value for <i>T</i> -test of Equality with		
					Stayer Ballot Losers		
	Stayer Ballot Losers	All-Move Ballot Losers	Nonapplicant Stayer HHs	All Nonapplicant Households	All-Move Ballot Losers	Nonapplicant Stayers	All Nonapplicants
Total household size	6.65	4.37	6.04	5.90	0.000	0.227	0.128
Adults aged 18 to 45	3.08	1.84	2.62	2.55	0.000	0.085	0.035
Children aged under 18	2.57	2.53	2.88	2.89	0.930	0.406	0.387
Adults aged over 45	1.00	0.00	0.50	0.43	0.000	0.001	0.000
Log total income per capita	8.36	8.41	7.93	8.00	0.784	0.002	0.007
Total income per capita	5,400	6,508	3,626	3,896	0.238	0.001	0.005
Household labor earnings per capita	2,683	3,359	1,712	1,851	0.274	0.004	0.013
Agricultural income per capita	282	141	124	113	0.283	0.126	0.079
Subsistence income per capita	2,192	2,621	1,659	1,789	0.360	0.103	0.206
Remittances per capita	243	373	130	130	0.133	0.179	0.156
Home ownership	0.53	0.35	0.29	0.32	0.043	0.004	0.011
Improve home	0.06	0.01	0.06	0.05	0.142	0.899	0.734
Value of durables	7,672	7,456	6,042	6,250	0.611	0.001	0.003
Number of cars	1.24	1.01	0.83	0.82	0.047	0.001	0.000
Number of pigs	5.96	4.12	5.36	5.22	0.010	0.607	0.495
Number of chickens	8.49	3.84	6.28	6.07	0.001	0.148	0.097
Number of cattle	1.71	0.87	0.71	0.73	0.042	0.004	0.003
Has bank account	0.89	0.88	0.64	0.64	0.792	0.002	0.002
Has ATM card	0.76	0.57	0.44	0.49	0.034	0.000	0.001
Number of meals rice	0.08	0.29	0.23	0.25	0.004	0.030	0.016
Number of meals roots	1.57	1.85	1.81	1.83	0.022	0.048	0.029
Number of meals fruits/vegetables	3.27	1.79	2.77	2.66	0.000	0.151	0.068
Number of meals fish	0.55	0.63	0.62	0.62	0.435	0.493	0.450
Number of meals fats	0.84	0.65	0.81	0.78	0.149	0.868	0.695
Number of meals meats	1.02	0.96	1.00	1.02	0.608	0.875	0.977
Number of meals milk	0.35	0.43	0.20	0.26	0.447	0.073	0.305
Sample size	49	75	107	124			

Stayer households include at least one member who, if the household had won the PAC ballot, would not be eligible to move to New Zealand. All-move households contain only eligible members.

remain in Tonga even if the principal applicant from their household had been successful in the PAC ballot.¹⁵ That is, those in the right comparison group for the parents, siblings nephews, and nieces of a migrant are the individuals who are the parents, siblings, nephews, and nieces of the would-be migrant in ballot-loser households. Means of the outcomes of interest for this group are presented in the first column of tables 2 and 3.

The second column of table 2 presents means for ballot-loser households where everyone in the household would be eligible to move if the household had been successful in the ballot. We call these households “all-move” ballot losers. A comparison of columns 1 and 2 then enables us to examine the evidence for selection among migrant households in terms of which migrants take their whole household and which do not. Likewise, column 2 of table 3 presents means for adults and children who would be eligible to move if the principal applicant in their household was successful in the PAC ballot. We see definite signs of selectivity. Not surprisingly, whole households that move are smaller than households in which some individuals would

stay. Failure to remove these all-move households from the ballot losers will therefore bias estimates of the impact of migration on household size, since these smaller all-move households are gone from the ballot-winner sample but still present in the ballot-loser sample. We also see other areas where this form of selection is important: all-move households have fewer farm animals, are less likely to own an ATM card, and have a diet with less fruit and vegetables than stayer households.

At the individual level, table 3 shows strong evidence of positive selectivity into migration among the working-age adults within a household. The individuals who would migrate if their household won the lottery have one more year of education and twice the weekly income of the same-age adults who would stay behind. These differences remain significant in a regression controlling for household fixed effects and gender. However, these individuals who are eligible to move have worse mental health than those who are not eligible to migrate if someone in their household is successful in the PAC ballot.

The third column in tables 2 and 3 presents the means for stayer households and individuals in the sample of nonapplicants. Comparing this column to the first allows us to examine the other channel of selection—selection into migration. We see that stayers in households where someone has applied to migrate are from larger, richer house-

¹⁵ This is after adjusting for imperfect compliance by instrumenting migration with ballot success. The same discussion as is applied here to ballot loser households applies equally to noncomplier households among ballot winners.

TABLE 3.—SELECTIVITY IN INDIVIDUAL CHARACTERISTICS AMONG WHO APPLIES AND WHO MOVES WITH THEIR WHOLE HOUSEHOLD

	Sample Means				<i>P</i> -Value for <i>T</i> -test of Equality with		
					Stayer Ballot Losers		
	Stayer Ballot Losers	Mover Ballot Losers	Nonapplicant Stayers	All Nonapplicants	Mover Ballot Losers	Nonapplicant Stayers	All Nonapplicants
Adults aged 18 to 45							
Currently employed (males)	0.46	0.59	0.57	0.56	0.161	0.243	0.284
Currently employed (females)	0.33	0.47	0.25	0.28	0.152	0.308	0.537
Business owner	0.10	0.12	0.13	0.11	0.666	0.463	0.679
Main activity is agriculture	0.30	0.25	0.26	0.25	0.436	0.506	0.343
Currently studying	0.20	0.13	0.10	0.10	0.173	0.025	0.027
Years of education	10.41	11.49	10.45	10.66	0.001	0.892	0.398
Weekly work income (pa'anga)	53.94	111.75	54.17	60.27	0.001	0.983	0.581
Very good health	0.34	0.36	0.34	0.34	0.760	0.974	0.967
Currently smokes	0.14	0.21	0.19	0.18	0.261	0.374	0.477
Alcoholic drinks per month	3.3	9.1	6.6	6.0	0.203	0.210	0.269
Body mass index	32.4	34.2	33.0	33.0	0.136	0.558	0.577
Waist-to-hip ratio	0.92	0.92	0.93	0.93	0.317	0.668	0.733
Diastolic blood pressure	86.3	83.7	83.7	83.7	0.126	0.187	0.169
Mental health	20.4	19.3	20.0	19.8	0.000	0.118	0.035
Children							
English literacy	0.45	0.57	0.44	0.49	0.148	0.952	0.629
Tongan literacy	0.61	0.64	0.54	0.59	0.675	0.454	0.881
Currently studying	0.63	0.72	0.61	0.64	0.263	0.871	0.889
Years of education	5.64	5.71	5.26	5.33	0.797	0.019	0.071
Very good health	0.68	0.52	0.55	0.54	0.070	0.143	0.112
BMI for age	1.18	1.49	1.32	1.31	0.332	0.680	0.706
Adults aged 46 and over							
Currently employed (males)	0.36	NA	0.38	0.38	NA	0.938	0.938
Currently employed (females)	0.30	NA	0.28	0.28	NA	0.869	0.869
business owner	0.16	NA	0.19	0.19	NA	0.740	0.740
Main activity is agriculture	0.43	NA	0.42	0.42	NA	0.845	0.845
Very good health	0.37	NA	0.23	0.23	NA	0.121	0.121
Currently smokes	0.26	NA	0.15	0.15	NA	0.179	0.179
Alcoholic drinks per month	3.61	NA	4.72	4.72	NA	0.784	0.784
Body mass index	35.6	NA	34.8	34.8	NA	0.770	0.770
Waist-to-hip ratio	0.92	NA	0.94	0.94	NA	0.153	0.153
Diastolic blood pressure	87.1	NA	90.3	90.3	NA	0.216	0.216
Mental health	19.3	NA	20.2	20.2	NA	0.036	0.036

T-tests account for household-level clustering. NA denotes not applicable, since individuals aged over 45 cannot be migrants under the PAC. Given this, the group of nonapplicant stayers is identical to the group of all nonapplicants for this age group. Mover ballot losers include both individuals who would move with their entire households, as well as individuals who would move when some of their members remain in Tonga.

holds than stayers in nonapplicant households. However, although the stayers in nonapplicant households earn less and have lower education than the individuals in applicant households who would migrate, they are much more similar in characteristics to the stayers in ballot-loser households.

Finally, the fourth column of tables 2 and 3 includes all nonapplicants, thereby combining the two forms of selectivity bias: selection into a migrant household and selection among migrant households as to who moves. However, since only 14% of nonapplicant households are classified as all movers, the overall effect of the two sources of selectivity is similar to that found for the impact of selection into migration on its own. As a result, we see that the full sample of nonapplicants differs significantly from migrant households in terms of household characteristics, but is not so different in terms of individual-level characteristics when compared with the stayers in migrant households. As a result of this positive self-selection into migration within migrant households, we would expect to see more selectivity when comparing household-level outcomes (which

incorporate the resources brought into the household by these high-income, high-education individuals) than when we compare individual-level outcomes for the stayers to the general population. This is, in fact, what we will see when comparing the experimental and nonexperimental results.

F. Calculating Nonexperimental Estimates of the Impact of Migration

Our experimental estimates of the impacts of migration come from IV regressions for the group of individuals whom the PAC policy rules identify as stayers, with the migration ballot outcome used as an instrument for migration. We also carry out other regression specifications that we use to illustrate the bias caused when the two channels of selection are ignored. First, we again estimate IV regressions but do not use the PAC rules to eliminate ballot-loser households and individuals who would have moved if the principal applicant had had a winning ballot in the PAC lottery. Comparing these estimates to the experimental esti-

mates illustrates the impact of selection among migrant households as to which members in the household moves. In our second comparison specification, we use OLS estimation to estimate the impact of migration by comparing migrant-stayer households to only the stayer nonapplicant households. This directly isolates the bias caused by household selection into migration. Then, in our third comparison specification, we examine the combined bias from both sources of selection by comparing migrant-stayer households to all nonapplicant households.

In all nonexperimental regressions that examine household-level impacts with our survey data, we condition on location (in Tongatapu or not), the maximum education level in the household, and the household's labor income earnings in 2004 (the year before the survey). For individual impacts, we condition on the same covariates as used in the experimental estimates with controls: location, gender, age, height, and income in 2004 for adults and birth order, maximum education level in the household, and log household income in 2004 for children. As noted previously, standard surveys do not typically contain much, if any, data on premigration characteristics of the household, so the use of premigration income as a control is already an improvement on what most existing studies that assume selection on observables are able to do. Moreover, these nonapplicant households were drawn from the same villages as the applicants and restricted to households with an adult in the eligible age range.

Finally, we also calculate nonexperimental estimates using the much larger samples available in the TLFS and HIES, combined with our migrant sample. For household-level comparisons, we condition on the maximum education level in the household and location (Tongatapu or not). One partial solution to dealing with the selectivity of whole households that would move is to drop nuclear households (consisting of at most married adults aged 18 to 45 and their children only) from the nonmigrant sample, on the assumption that such households would be most likely to depart en masse if they engaged in migration. We therefore present nonexperimental results for the full sample and for the sample dropping these nuclear households. For individual impacts, we condition on age, age squared, gender, years of education, and location. For comparison purposes, we also provide propensity-score matching estimates for adult labor market outcomes with the TLFS, where we match on quartiles in age and education, gender, marital status, island of birth, and the interaction between age and education, using kernel matching within the common support.

V. Impacts on Household-Level Outcomes

We now turn to estimating the impact of emigration on the remaining household members. A limitation of this analysis is that our surveys do not provide detail on how resources are allocated within households. While this means some caution must be had in interpreting the results, it is

entirely in keeping with the existing literature, which also has not been able to look at within-household allocation.¹⁶ Moreover, the comparison of experimental and nonexperimental methods will not depend on this limitation. Nevertheless, we will also return to the issue of what our data and other evidence say about income pooling within Tongan households after presenting the household-level results.

A. Household Size and Composition

We begin by examining the impact of emigration on household size and composition, since one immediate effect is that there are fewer mouths to feed. The impact of having some household members migrate to New Zealand on household size and composition is shown in table 4. Emigration leads to a significant reduction in household size. The mean household has 6.7 people, and emigration is estimated to reduce this by 2.2 people. Emigration leads to households having, on average, 1.5 fewer prime-age adults and 0.7 to 0.8 fewer children. There is no change in the number of older adults (those over 45 years), which is reassuring since they are not eligible to move as secondary applicants.

Table 2 showed that whole households that move are smaller than stayer households. Failure to remove all-move ballot losers would therefore cause us to understate the fall in household size from migration.¹⁷ Similarly, households that apply to migrate are larger than those that do not. Thus, panel C shows that all the nonexperimental estimates, which ignore this selectivity, understate the fall in household size arising from migration. Instead of correctly estimating a fall of 2.2 people, we now find that household size is estimated to decrease by only 0.4 to 1.1 people (an insignificant impact for four of the seven nonexperimental estimates). The nonexperimental estimates also incorrectly indicate that migration leads to a statistically significant change in the number of adults aged over 45. Dropping nuclear households helps a little in terms of moving the nonexperimental estimates in the right direction but still leads to an underestimate of the decline in total household size from migration.

B. Household Income

We next examine the impact on total household income, which can be disaggregated into four sources: (a) household earnings (annualized from individual reports for the previous

¹⁶ The only paper we are aware of that collects data on within-household allocation and migration is De Vreder, Lambert, and Safir (2008), who consider large polygamous households in Senegal and look at allocation within subunits of the household.

¹⁷ For household-level impacts, the first set of nonexperimental estimates does not include controls. They are thus directly comparable to the estimates in panel A in each table examining household impacts. The reason for not including controls in these regressions is that the household-level controls are defined in terms of stayer characteristics and are thus not defined for all-move households.

TABLE 4.—IMPACT OF MIGRATION ON HOUSEHOLD COMPOSITION

	Total Household Size	Adults Aged 18 to 45	Children Aged under 18	Adults Aged over 45
A: Experimental Estimates without Controls				
Impact of migration	-2.23*** (0.62)	-1.54*** (0.33)	-0.80* (0.44)	0.06 (0.18)
B: Experimental Estimates with Controls				
Impact of migration	-2.25*** (0.69)	-1.49*** (0.34)	-0.72 (0.52)	-0.09 (0.20)
Mean for unsuccessful stayer households	6.65	3.08	2.57	1.00
Sample size	118	118	118	118
C: Estimates Using Nonexperimental Control Groups				
Including all-move ballot losers ($n = 211$)	-0.85 (0.53)	-0.76*** (0.23)	-0.78** (0.39)	0.64*** (0.15)
Compared to nonapplicant stayers ($n = 175$)	-0.72* (0.41)	-0.60*** (0.18)	-0.74** (0.32)	0.61*** (0.15)
Compared to all nonapplicants ($n = 192$)	-0.64 (0.40)	-0.54*** (0.18)	-0.78** (0.31)	0.68*** (0.15)
TLFS full sample ($n = 2,158$)	-0.37 (0.44)	-0.17 (0.19)	-0.48 (0.34)	0.16 (0.17)
TLFS dropping nuclear households ($n = 1,524$)	-0.72 (0.44)	-0.22 (0.19)	-0.41 (0.34)	-0.24 (0.17)
HIES full sample ($n = 1,688$)	-0.83** (0.35)	-0.44*** (0.16)	-0.60** (0.25)	0.03 (0.12)
HIES dropping nuclear households ($n = 1,328$)	-1.10*** (0.36)	-0.47*** (0.16)	-0.57** (0.26)	-0.26** (0.12)

Experimental estimates are IV estimates where migration is instrumented with the PAC ballot outcome and the estimation sample of $n = 118$ includes noncompliers. Controls are labor earnings of stayers in 2004, the proportion of adult stayers who are female, highest education level of stayer adults, and whether the household lives on Tongatapu. See text for more detail on the estimates using nonexperimental control groups. *, **, and *** indicate significance at the 10%, 5%, and 1% levels.

week); (b) net returns from sales of fish, crops, livestock, tapa cloth, and mats (annualized from household reports on an average month); (c) the imputed value of own-produced or own-captured food consumed by the household (annualized from household reports for the previous week); and (d) net remittances of money and goods.

Since households in Tonga that have had some members move to New Zealand under the PAC have fewer members, we examine the impact on per capita incomes and alternatively on adult equivalent incomes.¹⁸ The results in table 5 for log total income indicate that the families of migrants have 25% to 26% lower incomes than the families of non-migrants, when no control variables are included, regardless of whether income is per capita or per adult equivalent. The estimated impact is a 22% to 24% decline in income when control variables are added, but the per capita estimate is no longer statistically significant. If we instead estimate a linear model, which is more sensitive to outliers, we find that income declines by \$1,000 per capita or \$1,250 per adult equivalent (19% to 20% of the mean for treatment group households) for families of migrants when there are no controls and by \$641 per capita or \$927 per adult equivalent (12% to 14%) when controls are included. In neither case are the estimates significantly different from 0.

¹⁸ Nutrition-based equivalence scales are not available for Tonga. We therefore follow Deaton and Paxson (1994) and define the number of adult equivalents as the number of adults 18 and over, plus 0.5 times the number of children 17 and under. As households in Tonga that have had some members move to New Zealand under the PAC have fewer children, equivalence scales based on children needing less food and other resources than adults will raise per person resources more for the control group than for the migrant group.

Examining the four components of household income, we find that having household members migrate to New Zealand under the PAC leads to significant reductions in household labor income per capita (\$1,281–\$1,556) or per adult equivalent (\$1,064–\$1,305). This is the main opportunity cost to the household of sending a migrant: the income the individual would have earned had he or she not migrated. These falls are partially offset by significant increases in remittances received of \$466 to \$503 per capita or \$560 to \$590 per adult equivalent. There is no significant change in either agricultural or subsistence income per person.¹⁹ Thus, while households with PAC migrants receive more remittances and have fewer mouths to feed, this does not compensate for the large reduction in labor earnings these households face.

Table 2 showed little selection between stayer and all-move ballot-loser households in terms of income variables. As a result, the first nonexperimental specification (which uses all ballot losers, including all-move households) produces results similar to the experimental results in panel A. In contrast, table 2 indicated significant positive selection on income in terms of being an applicant household. Thus, the nonexperimental estimates in table 5 that use the nonapplicant sample understate the fall in income from migration. In fact, the point estimates here indicate that migration leads to a 15% to 18% increase in total income per capita

¹⁹ It is worth noting that both overall agriculture and subsistence income decline significantly for households with PAC migrants, but the decline in the number of individuals in these households offsets these declines at least using per capita and per adult equivalent measures of household income.

TABLE 5.—IMPACT OF MIGRATION ON THE HOUSEHOLD INCOME OF REMAINING HOUSEHOLD MEMBERS

	Log Total Income	Total Income	Household Labor Earnings	Agricultural Income	Subsistence Income	Net Remittances
A: Experimental Estimates, per Capita without Controls						
Impact of migration	−0.259* (0.149)	−1,007 (720)	−1,281*** (421)	−197 (165)	5 (461)	466*** (156)
B: Experimental Estimates, per Capita with Controls						
Impact of migration	−0.218 (0.153)	−641 (743)	−1,064** (428)	64 (137)	−144 (552)	503*** (163)
C: Experimental Estimates, per Adult Equivalent without Controls						
Impact of migration	−0.253* (0.143)	−1,246 (782)	−1,556*** (497)	−233 (175)	−18 (481)	560*** (199)
D: Experimental Estimates, per Adult Equivalent with Controls						
Impact of migration	−0.242* (0.143)	−927 (778)	−1,305*** (490)	46 (144)	−259 (578)	590*** (207)
Mean PC for unsuccessful stayer households	8.37	5,400	2,683	282	2,192	243
Mean PAE for unsuccessful stayer households	8.55	6,377	3,224	322	2,546	285
Sample size	118	118	118	118	118	118
E: Estimates Using Nonexperimental Control Groups, per Capita						
Including all-move ballot losers ($n = 211$)	−0.243* (0.145)	−1,397* (844)	−1,478*** (553)	−90 (118)	−195 (418)	366** (150)
Compared to nonapplicant stayers ($n = 175$)	0.178 (0.134)	585 (537)	−206 (231)	−45 (71)	259 (319)	576*** (159)
Compared to all nonapplicants ($n = 192$)	0.149 (0.129)	490 (529)	−220 (225)	−32 (69)	167 (313)	581*** (156)
TLFS full sample ($n = 2158$)			35 (241)			
TLFS dropping nuclear households ($n = 1524$)			217 (248)			
HIES full sample ($n = 1688$)	0.177* (0.096)	14 (437)	−384* (206)	−203** (80)	1,632*** (253)	−408** (164)
HIES dropping nuclear households ($n = 1292$)	0.181* (0.097)	20 (447)	−328 (211)	−201** (82)	1,682*** (251)	−516*** (178)

Experimental estimates are IV estimates where migration is instrumented with the PAC ballot outcome and the estimation sample of $n = 118$ includes noncompliers. Controls are labor earnings of stayers in 2004, the proportion of adult stayers who are female, highest education level of stayer adults, the number of stayers who are children and adults 18 to 45, and whether the household lives on Tongatapu. See text for nonexperimental control group details. *, **, and *** indicate significance at the 10%, 5% and 1% levels.

for the remaining household members rather than the correct finding of a 25% to 26% fall that the experimental estimate yields. Essentially nonapplicant households are substantially poorer than migrant households would be (even conditional on prior labor earnings), and thus using them as a control group causes one to think that migrant households are relatively well off. Similarly, the use of these nonexperimental control groups causes one to miss the fall in household labor earnings per capita that comes with household members migrating.

Using the HIES would also lead one to conclude that migration has increased log total household income per capita, with little difference from dropping nuclear households. The HIES also would lead one to erroneously conclude that remittances have fallen and subsistence income risen as a result of migration. We believe this occurs because the HIES does not provide detailed enough geographic information to match on the same villages as the migrants are coming from and other areas of Tonga (even within island) likely grow less of their own produce and receive more remittances from migrants who left through other migration channels than do households in the PAC villages. The TLFS contains information on only labor earnings, and the nonexperimental estimates using these data would not detect the large fall in household labor earnings arising from having some household members migrate.

C. Durable Assets and Financial Access

We next examine changes in other measures of household resources, including three types of durable assets; the dwelling, durable goods, and livestock. We also examine the impact on the financial access of each household, in particular, whether any household members have bank accounts or ATM cards.²⁰

Among our survey questions, we ask whether the dwelling is owned by anyone in the household and whether, in the past twelve months, any renovations have been done. Our survey also asks whether household members own any of 24 durable assets, including household appliances, entertainment equipment, and motor vehicles. We aggregate these responses into a single index using the prices of durable goods we collected from stores in Tonga.²¹ A separate question is asked on the number of automobiles that household members have available for their regular use. The final asset questions concern holdings of domestic livestock

²⁰ An ATM card allows access to a remittance channel with transactions costs ten percentage points lower than the usual money transfer fees in the New Zealand-to-Tonga corridor (Gibson, McKenzie, & Rohorua, 2006).

²¹ We also used principal component analysis to create a single dimensional index of wealth based on the first principal component and found similar results.

(pigs, chicken, cattle, goats, and horses). We examine the impact of having household members emigrate to New Zealand and on the value of durables, the number of cars, and the holdings of the main livestock: pigs, chickens, and cattle.

Table 6 reports the estimated impact of having household members migrate to New Zealand on each of these outcomes. Again, impacts are estimated both without any control variables and with controls to deal with small sample differences between treatment and control groups. Although some outcomes are discrete, we continue to present estimates from linear instrumental variable regression models. We also estimated treatment effects for the discrete outcomes using the equivalent simultaneous equations probit models and found nearly identical marginal effects as those presented for the models with no covariates, but had difficulty getting the models with control variables to converge (a small number of covariates perfectly predict whether households are in the noncomplier group).

We find that having household members migrate to New Zealand leads to the remaining members' having fewer cars and livestock and being less likely to have a bank account or ATM card. When control variables are added, these effects persist for chickens and the financial access variables. The impacts are large, with the remaining family of emigrants having half as many chickens as nonmigrant households and being 17% less likely to have a bank account and 31% to 34% less likely to have an ATM card. We also find negative, albeit insignificant, impacts on home ownership, the likelihood of having renovations, and the value of durable goods.

It is worth emphasizing that all of these results merely reflect changes in household-level assets or financial access. These changes may be occurring for a number of reasons: households may have sold off assets so the proceeds could be used by the individuals moving to New Zealand, the lower incomes caused by having these family members move to New Zealand may have caused a reduction in assets and financial access relative to unsuccessful stayer households, the individual in the household who used a bank account may have been the person who migrated, or the change in household composition (for example, the moving away of working-age household members) reduces needs for particular assets (such as cars and computers) or financial access. Only 10% of migrant households in New Zealand with family members remaining in Tonga report selling livestock, vehicles, or other assets before moving to New Zealand, suggesting that the first explanation is not the main channel.

Table 2 showed that stayer ballot losers tend to be wealthier than nonapplicant households, and in some dimensions, they also hold more assets than entire households that move. The nonexperimental estimates at the bottom of table 6 reflect the consequences of this selection. Using the nonapplicant sample, one would conclude that migration has made remaining household members wealthier, with a higher value of durable assets, more cars, greater home

TABLE 6.—IMPACT OF MIGRATION ON DURABLE ASSETS AND FINANCIAL ACCESS

	Home Ownership	Improve Home	Value of Durables	Number of Cars	Number of Pigs	Number of Chickens	Number of Cattle	Has Bank Account	Has ATM Card
A: Experimental Estimates without Controls									
Impact of migration	-0.022 (0.103)	-0.043 (0.041)	-615 (508)	-0.288* (0.153)	-1.339* (0.807)	-4.639*** (1.711)	-0.860* (0.493)	-0.172** (0.078)	-0.340*** (0.095)
B: Experimental Estimates with Controls									
Impact of migration	-0.068 (0.122)	-0.038 (0.049)	-348 (624)	-0.247 (0.159)	-1.560 (1.000)	-3.746** (1.864)	-0.822 (0.512)	-0.171* (0.090)	-0.308*** (0.106)
Mean for unsuccessful stayer households	0.531	0.061	7,672	1.24	5.96	8.49	1.71	0.891	0.761
Sample size	118	118	117	118	118	118	118	115	115
C: Estimates Using Nonexperimental Control Groups									
Including all-move	0.117 (0.093)	0.018 (0.034)	-328 (403)	-0.181 (0.125)	-0.207 (0.814)	-2.015 (1.230)	-0.438 (0.376)	-0.147** (0.072)	-0.219** (0.090)
ballot losers ($n = 211$)									
Compared to nonapplicant	0.170** (0.080)	-0.068*** (0.024)	1,150*** (388)	0.295** (0.115)	-0.081 (1.155)	-1.231 (1.170)	0.260 (0.277)	0.137* (0.074)	0.039 (0.080)
stayers ($n = 175$)									
Compared to all	0.149* (0.079)	-0.057*** (0.021)	1,066*** (380)	0.318*** (0.112)	0.054 (1.088)	-1.063 (1.109)	0.263 (0.270)	0.146** (0.072)	0.002 (0.078)
nonapplicants ($n = 192$)									

Experimental estimates are IV estimates where migration is instrumented with the PAC ballot outcome and the estimation sample of $n = 118$ includes nonmigrants. Controls are labor earnings of stayers in 2004, the proportion of adult stayers who are female, highest education level of stayer adults, the number of stayers who are children and adults 18 to 45, and whether the household lives on Tongatapu. See text for description of nonexperimental control groups. *, **, and *** indicate significance at the 10%, 5%, and 1% levels.

TABLE 7.—IMPACT OF MIGRATION ON THE DIET OF REMAINING HOUSEHOLD MEMBERS

	Number of Meals	Number of Meals	Number of Meals	Number of Meals	Number of Meals	Number of Meals	Number of Meals
	Rice	Roots	Fruits and Vegetables	Fish	Fats	Meats	Milk
A: Experimental Estimates without Controls							
Impact of migration	0.189** (0.072)	0.392*** (0.142)	−1.291*** (0.434)	0.177 (0.111)	−0.213 (0.149)	−0.133 (0.137)	−0.054 (0.116)
B: Experimental Estimates with Controls							
Impact of migration	0.128 (0.087)	0.337** (0.168)	−1.349*** (0.498)	0.209 (0.144)	−0.355** (0.179)	−0.158 (0.167)	0.001 (0.121)
Mean for unsuccessful stayer households	0.082	1.571	3.265	0.551	0.837	1.020	0.347
Sample size	118	118	118	118	118	118	118
C: Estimates Using Nonexperimental Control Groups							
Including all-move ballot losers ($n = 211$)	0.052 (0.078)	0.356** (0.144)	−0.511 (0.344)	0.142 (0.092)	−0.069 (0.128)	−0.005 (0.122)	−0.024 (0.104)
Compared to nonapplicant stayers ($n = 175$)	0.026 (0.077)	0.135 (0.142)	−0.221 (0.290)	0.145* (0.085)	−0.100 (0.117)	−0.129 (0.114)	0.023 (0.077)
Compared to all nonapplicants ($n = 192$)	0.010 (0.076)	0.120 (0.138)	−0.136 (0.282)	0.150* (0.083)	−0.080 (0.114)	−0.147 (0.111)	−0.019 (0.077)

Experimental estimates are IV estimates where migration is instrumented with the PAC ballot outcome and the estimation sample of $n = 118$ includes noncompliers. Controls are labor earnings of stayers in 2004, the proportion of adult stayers who are female, highest education level of stayer adults, the number of stayers who are children and adults 18 to 45, whether the household lives on Tongatapu, and day of the week fixed effects. Roots include taro (swamp taro), taro taruas (chinese taro), kumara (sweet potato), taamu/kape, yams, cassava/manioc, and potato. Fruits and vegetables include other vegetables, coconut (fresh and dry), banana, mango, pawpaw, and other fruits. Fish includes tinned fish and fresh fish. Fats include corned beef, mutton, and coconut (fresh and dry). Meats include corned beef, mutton, fresh beef, chicken, pork, and other meat (such as sausage). *, **, and *** indicate significance at the 10%, 5%, and 1% levels.

ownership, and increased likelihood of having a bank account. All of these results just reflect selection, and if anything, our experimental results indicate that migration of some household members has reduced the wealth of those left behind.

D. Diet

The final set of household-level outcomes we examine is the impact of having some household members emigrate to New Zealand on the diet of those who stay. Specifically, we ask about the number of meals at which each of thirty different foods were eaten by any household resident the previous day. To focus our analysis, we examine the cumulative number of meals in which seven foods are consumed, six of which are composites. These foods are rice, roots, fruits and nonroot vegetables, fish, fats, meats, and milk.

The results presented in table 7 indicate that having family members emigrate leads to a significant increase in the consumption of rice and roots and a significant decrease in the consumption of fruits and vegetables. These changes in diet are large: consumption of rice doubles, consumption of roots goes up by 20% to 25%, and consumption of fruits and vegetables declines by 38% to 40%. It is unlikely that these patterns reflect some prior division within households whereby those who would be movers eat fruit and vegetables and those who would be stayers eat rice and roots. When we use the ballot-loser households that would not be all-movers to test whether there is an equal effect on the number of meals of each particular food group from how many individuals within detailed age and gender group would be stayers versus how many would be movers, we

cannot reject the null hypothesis of equal effects across the movers and stayers.²²

The question on diet asks which foods anyone in the household ate yesterday. We would thus expect larger households to be more likely to have someone eating any given food group. Since this question is asked of the entire household, and not just of the members who would stay if the household had a PAC ballot winner, the smaller size of migrant households should lead to a tendency to find lower likelihoods of consuming any particular food group. The significant negative result for fruits and vegetables may therefore just reflect that migrant households do not have as many prime-age adults who are likely to eat these foods. However, this mechanical effect of household size cannot explain the increase in rice and root consumption among migrant households.

The main selectivity in terms of diet is on rice, roots, and fruits and vegetables. Consistent with stayer ballot-loser households being wealthier, they have a diet richer in more expensive fruits and vegetables and eat less rice and roots, which are cheap sources of calories. As a consequence of this selectivity, failure to exclude whole households that would have moved or using the nonapplicant sample leads one to understate the fall in fruits and vegetables and the rise in root and rice consumption. This parallels the understatement of the fall in wealth and income seen in the previous sections.

E. Pooling of Resources and Interpretation of Results

We have seen that migration reduced the per capita resources available in migration households. However,

²² This adapts a procedure for deriving individual diets from household-level data suggested by Chesher (1997). Results are available from the authors on request.

since we do not know how resources were distributed within the household prior to migration, it is difficult to know whether these changes reflect a genuine change in resources available to the family members left behind, particularly as we have shown that the adult movers contributed significantly more to the household in terms of labor earnings than do the adult stayers.

Nevertheless, we believe that the available evidence does suggest that resources are pooled within Tongan households in the absence of migration. First, prior anthropological investigations argue that food and other resources are shared equally among extended family members within a household (Pollock, 1992). Second, we use the stayer ballot-loser households to test whether there is an equal relationship between the age and gender composition and earnings of stayers and movers in the household and household diet and cannot reject the null hypothesis of equal effects across the two groups for any of the food groups at the 5% significance level.²³ The HIES shows that food consumption is approximately two-thirds of the household budget, so income-pooling effects on diet seem likely to extend to total consumption. Third, if resources were unequally shared within the household, we should expect to see differences in food intake manifest themselves in differences in anthropometric measures between stayers and movers. However, when we regress anthropometrics on household fixed effects and a dummy for being a stayer, we find no difference in child height for age or BMI for age, or in adult BMI between stayers and movers.²⁴ This evidence leads us to believe that the fall in household resources per capita from migration does indeed represent a genuine fall in resources available to the stayers.

We now turn to comparing individual-level outcomes, which do not require assumptions about the distribution of resources within the household to make judgments about the impact of migration on individual well-being.

VI. Impacts on Individuals

A. Working-Age Adults

Table 8 examines the impact of migration on the labor supply, employment activity, and health of 18- to 45-year-old stayer adults—the age range eligible to apply for the PAC. Since the literature has found that the impact of migration on labor supply varies by gender, we split the employment results by gender. The point estimates suggest a negative effect for females and positive impact on males, but neither is significant. There is also no significant impact on business ownership or self-employment or on whether

²³ Only one of the seven food groups (meats, weakly at the $p = 0.08$ level) shows any evidence consistent with the hypothesis that a pa'anga dollar of earnings from a stayer is treated differently within the household from a pa'anga of earnings from a mover when it comes to household diet.

²⁴ Results available on request.

TABLE 8.—IMPACT OF MIGRATION ON OUTCOMES FOR 18–45 YEAR-OLD ADULTS REMAINING IN TONGA

	Currently Employed (Males)	Currently Employed (Females)	Business Owner	Engage in Agriculture	Currently Studying	Very Good Health	Currently Smokes	Alcoholic Drinks Per Month	Body Mass Index	Waist-to-Hip Ratio
A: Experimental Estimates without Controls										
Impact of migration	0.084 (0.123)	-0.103 (0.100)	-0.001 (0.059)	-0.178* (0.092)	-0.085 (0.072)	-0.005 (0.072)	0.009 (0.068)	7.476** (3.426)	-0.565 (1.776)	-0.028** (0.011)
B: Experimental Estimates with Controls										
Impact of migration	0.090 (0.167)	-0.053 (0.095)	0.081* (0.048)	-0.168* (0.098)	-0.010 (0.061)	0.010 (0.095)	0.033 (0.076)	6.352 (4.229)	-2.149* (1.100)	-0.030** (0.012)
Mean for unsuccessful stayer households	0.459	0.333	0.097	0.300	0.197	0.338	0.143	3.31	32.4	0.925
Sample size	85	91	175	170	174	171	135	134	157	159
C: Estimates Using Nonexperimental Control Groups										
Including all-move ballot losers ($n = 417$)	-0.021 (0.093)	-0.146* (0.074)	-0.044 (0.043)	-0.168** (0.065)	-0.069 (0.053)	-0.033 (0.068)	-0.057 (0.064)	-0.873 (5.205)	-1.718 (1.097)	-0.022** (0.011)
Compared to nonapplicant stayers ($n = 328$)	-0.080 (0.064)	-0.073 (0.066)	-0.020 (0.033)	-0.093* (0.056)	-0.013 (0.044)	-0.013 (0.062)	-0.030 (0.060)	2.414 (3.632)	-1.639* (0.932)	-0.026*** (0.009)
Compared to all nonapplicants ($n = 383$)	-0.038 (0.063)	-0.076 (0.063)	-0.006 (0.031)	-0.098* (0.050)	-0.023 (0.042)	0.009 (0.057)	-0.010 (0.055)	3.883 (3.405)	-1.489* (0.857)	-0.026*** (0.009)
TFLS all 18–45 year olds ($n = 4,050$)	0.112 (0.083)	-0.067 (0.059)	-0.006 (0.039)	-0.184*** (0.046)	-0.002 (0.029)					
TFLS propensity score matched in common support ($n = 3,478$)	0.131 (0.093)	-0.044 (0.068)	-0.036 (0.039)	-0.234*** (0.046)	0.021 (0.028)					

Experimental estimates are IV estimates where migration is instrumented with the PAC ballot outcome. Controls are gender, age, years of education, height, labor earnings in 2004, and whether the household lives on Tongatapu. Standard errors account for household-level clustering. Propensity score matching uses kernel regression to match on quartile in age, quartile in education years, gender, marital status, birth island, and Age \times Education years, restricts analysis to common support and bootstraps standard errors. See text for further description of the non-experimental estimates. *, **, and *** indicate significance at the 10%, 5%, and 1% levels.

TABLE 9.—IMPACT OF MIGRATION ON OUTCOMES FOR CHILDREN REMAINING IN TONGA

	English Literacy	Tongan Literacy	Currently Studying	Years of Education	Very Good Health	BMI for Age
A: Experimental Estimates without Controls						
Impact of migration	0.062 (0.123)	−0.059 (0.122)	−0.027 (0.111)	−0.118 (0.358)	−0.190 (0.141)	0.123 (0.374)
B: Experimental Estimates with Controls						
Impact of migration	0.243* (0.129)	−0.006 (0.036)	0.003 (0.076)	−0.783 (0.997)	−0.186 (0.171)	0.173 (0.352)
Mean for unsuccessful stayer households	0.447	0.605	0.629	1.50	0.676	1.18
Sample size	146	146	143	146	142	123
C: Estimates Using Nonexperimental Control Groups						
Including all-move ballot losers ($n = 487$)	0.044 (0.080)	0.051 (0.059)	0.024 (0.047)	−0.089 (0.208)	−0.157 (0.100)	−0.161 (0.313)
Compared to nonapplicant stayers ($n = 402$)	−0.005 (0.071)	−0.041 (0.051)	−0.022 (0.036)	0.463 (0.329)	−0.020 (0.092)	−0.049 (0.238)
Compared to all nonapplicants ($n = 466$)	−0.023 (0.074)	−0.055 (0.052)	−0.028 (0.036)	0.138 (0.139)	−0.020 (0.090)	−0.020 (0.232)
HIES full sample of children ($n = 4,052$)			0.017 (0.026)			

Experimental estimates are IV estimates where migration is instrumented with the PAC ballot outcome and the estimation sample is up to $n = 146$, depending on outcome, and includes noncompliers. Controls are gender, age in months, age in months squared, birth order, log(household income in 2004 + 1), maximum education level in household, and whether the household lives on Tongatapu. Standard errors account for household-level clustering. See text for nonexperimental estimates. *, **, and *** indicate significance at the 10%, 5%, and 1% levels.

the adult is currently studying. However, we do find that individuals are less likely to be engaging in agriculture, significant at the 10% level.

Self-assessed general health status does not change with migration of other household members. We do see significant impacts on some health behaviors and anthropometric measures. Individuals in migrant households do not change their smoking behavior, but they consume significantly more alcoholic drinks per month, although the significance disappears when we add controls. The Tongan population has one of the highest levels of obesity in the world, with 60% of the population classified as obese (Colagiuri et al., 2002). Migration is found to reduce BMI, significant at the 10% level, and to reduce the waist-to-hip ratio, significant at the 5% level. The waist-to-hip ratio is a marker of central obesity, which is more strongly associated than BMI with the risk of myocardial infarction among many ethnic groups is (Yusuf et al., 2005). Reductions in BMI and waist-to-hip ratios thus represent health improvements for the remaining adults in emigrant households.²⁵ The point estimates also suggest lowered blood pressure, but this effect is insignificant.

In related work (Stillman, McKenzie, & Gibson, 2009), we have found migration to improve the mental health of the Tongans who move to New Zealand under the PAC. The last column of table 8 shows that their remaining family members do not receive the same improvement. The MHI-5 ranges from 5 to 25, with higher scores indicating better mental health. The point estimates thus suggest that, if anything, migration lowered the mental health of remaining family members, although this effect is insignificant.

²⁵ These health improvements have occurred along with a decline in household economic resources, suggesting that one pathway is that less food leads to less overnutrition, but behavioral change could also be a factor (for example, more walking if migrants sold the family car). Evidence from developed countries suggests that people adopt healthier lifestyles when income declines, such as during recessions (Ruhm, 2005).

Table 3 showed that although the migrants themselves differ from the nonapplicants, the stayer adults in migrant households are fairly similar. As a result, selectivity is not such an issue in our case for calculating the impact of migration on the individuals left behind. Thus, the nonexperimental estimates also lead us to conclude that migration causes less engagement in agriculture and lowers BMI and waist-to-hip ratios. The results using the TLFS are similar to those using the nonapplicant sample, and we find that while propensity score matching on the common support eliminates about 15% of the sample, the results are qualitatively similar.

However, this is not always the case. For example, table 3 shows that there is some evidence for selectivity in mental health, with stayer household members in ballot-loser households having better mental health than individuals who would move if the household had been successful in the ballot, and also better mental health than nonapplicants. The point estimates in panel C of table 8 reflect this selectivity, with the results that do not control for migrant selectivity indicating that migration improves the mental health of those left behind, whereas the experimental estimate suggests it lowers mental health. However, these differences are not statistically significant.

B. Children

Table 9 examines the impacts of migration of other household members on the education and health of children aged 17 and under. Recall that the migrants here are typically aunts and uncles of these children,²⁶ and their parents are not the ones migrating. For this reason we might expect

²⁶ Seventy-two percent are classified as “other relative” in terms of their relation to the principal applicant, and 23% are a sibling of the principal applicant.

fewer of the potential negative effects of parental absence on education and health and that the main channel through which migration would affect these children is through income effects. Tonga has good basic education and health services and is ranked by the UN as high in terms of human development, with an adult literacy rate of 98.2%. As such, liquidity constraints are unlikely to be very important in determining access to schooling and health, and thus changes in income may have relatively small impacts on health or education outcomes.

The results in table 9 are consistent with this hypothesis. Migration is not found to significantly affect the likelihood of whether children are currently studying, their years of education attained, Tongan literacy, or either parental-assessed or anthropometric health measures. The only marginally significant effect is greater English literacy among children in migrant households, which is significant only when controls are added. This impact is missed when the nonexperimental control groups are used. Similarly, the point estimates using the nonapplicant sample lead to the conclusion that migration increases years of education among left-behind children. This contrasts with the zero or negative effect found with the experimental estimates and likely reflects the positive selection of households into migration. The only information on children available in the HIES is whether they are currently studying, which gives a small and insignificant effect in line with the other samples. The TLFS has no data on children under 10. Again, the lower degree of selectivity at the individual level than at the household level means that we would not draw very different conclusions here about impacts on children left behind from the nonexperimental estimators.

C. Older Adults

Finally, in table 10, we report the estimated impacts of migration of household members on adults aged 46 and older. The majority of these older household members are parents of the migrant, with a mean age of 60. The point estimates suggest that both older males and females are less likely to be employed when their children migrate. The magnitudes are sizable relative to the mean, corresponding to a halving of the employment rate. However, the results are not significant when we examine men and women separately, and are significant only at the 10% level when we combine males and females and do not include additional controls in the regression. The point estimates also show large negative effects on the likelihood of being a business owner, but again these are statistically insignificant. As with younger adults, there is a tendency for older adults to be less involved in agriculture when they are in migrant households, although this difference is insignificant. Older adults are marginally less likely to view themselves as being in very good health when other household members have migrated, but we find no significant impacts on health beha-

TABLE 10.—IMPACT OF MIGRATION ON OUTCOMES FOR ADULTS AGED 46 AND ABOVE IN TONGA

	Currently Employed (Males)	Currently Employed (Females)	Business Owner	Main Activity Is Agriculture	Very Good Health	Currently Smokes	Alcoholic Drinks per Month	Body Mass Index	Waist-to-Hip Ratio	Diastolic Blood Pressure
A: Experimental Estimates without Controls										
Impact of migration	-0.159 (0.129)	-0.163 (0.109)	-0.083 (0.069)	-0.130 (0.104)	-0.104 (0.093)	-0.102 (0.097)	0.463 (3.838)	-0.469 (3.576)	0.007 (0.012)	1.258 (3.360)
B: Experimental Estimates with Controls										
Impact of migration	-0.193 (0.133)	-0.092 (0.140)	-0.121 (0.105)	-0.077 (0.130)	-0.181 (0.116)	-0.062 (0.143)	4.292 (3.739)	2.057 (2.079)	0.002 (0.014)	-0.752 (3.416)
Mean for unsuccessful stayer households	0.364	0.296	0.163	0.435	0.367	0.263	3.61	35.6	0.918	87.1
Sample size	53	68	121	117	121	85	85	105	104	104
C: Estimates Using Nonexperimental Control Groups										
Compared to nonapplicant stayers ($n = 113$)	0.030 (0.121)	-0.159 (0.110)	-0.065 (0.070)	-0.187 (0.127)	0.104 (0.104)	0.065 (0.089)	1.054 (5.332)	0.436 (1.490)	0.000 (0.014)	-2.598 (2.636)
TLFS adults 46+ ($n = 2,158$)	-0.170* (0.097)	0.029 (0.067)	-0.058 (0.054)	-0.107 (0.082)						

Experimental estimates are 2SLS estimates where migration is instrumented with the PAC ballot outcome and the estimation sample is up to $n = 121$ depending on outcome, and includes non-compliers. Controls are gender, age, years of education, height, labor earnings in 2004, and whether the household lives on Tongatapu. Standard errors account for household-level clustering. See text for nonexperimental estimates. Individuals of this age are not eligible to migrate under the PAC, so there is only single selection (into migrant families), and not the selection into whose entire family migrates and who migrates for this age. *, **, and *** indicate significance at the 10%, 5%, and 1% levels.

vior, BMI, waist-to-hip ratios, blood pressure, or mental health.

Since, under the PAC rules, none of these older adults would be eligible to move, the only channel for selection is in terms of whether adults aged over 45 in households where someone else applied to the PAC are different from adults aged over 45 in households where no one applies to migrate. Again, table 3 suggested that this selectivity is relatively mild, so most of the nonexperimental estimates are similar in sign and size to the experimental results. The one variable where there appeared to be some selection was mental health: older adults in nonapplicant households have better mental health. This results in a negative point estimate when using the nonexperimental control group, as opposed to the positive experimental point estimate, although neither estimate is statistically significant.

VII. Omnibus Effects and Multiple Hypothesis Testing

Our analysis so far has followed the existing literature and tested for the impact of migration on particular outcomes on a one-by-one basis. An advantage of this approach is that we can directly compare our results for any particular outcome, for example, the impact of migration on business ownership, to those from other studies examining the same outcome. However, in total over tables 4 through 10, we are examining the impacts of migration on 62 different outcomes, and for each outcome, we consider the result both with and without controls. This raises questions about multiple hypothesis testing. In this section, we examine which of our results are robust to different corrections for multiple testing.

One approach sometimes used with multiple outcomes is to aggregate them into particular groupings to examine whether the overall impact of the treatment on a family of outcomes is different from zero.²⁷ This is useful if the intention is to see whether the global impact of a particular treatment is generally positive or negative. This is not our focus here, since we are interested in the individual channels through which migration affects family members left behind. For example, we are interested in whether household labor earnings and subsistence earnings go down with migration and remittances go up, more than whether the average effect over all types of income is positive or not.

We instead consider the significance of individual coefficients when viewed as part of a family of n hypotheses. For example, we consider all outcomes related to diet as a family. The family-wise error rate is then defined as the probability of at least one type 1 error in the family (Shaffer, 1995). Then we can maintain the family-wise error rate at some designated level α , such as 0.05 or 0.10, by adjusting the p -values used to test each individual null hypothesis in the family. The simplest such method is the Bonferroni method, which uses as critical values α/n . Thus, with 10 out-

comes in a family, we would need to use a cutoff of a p -value less than 0.01 when testing each individual outcome to maintain the family-wise error rate at 10%.

Several refinements to the Bonferroni method offer greater power.²⁸ Ranking the n outcomes in increasing order of their p -values for testing a null effect, so that $p_1 \leq p_2 \leq \dots \leq p_n$. Then, Holm's (1979) sequentially rejective Bonferroni method is applied as follows. In the first step, a null effect for outcome 1 is rejected if $p_1 \leq \alpha/n$. If we cannot reject this outcome, we cannot reject null effects for all other outcomes. Otherwise, reject a null effect for outcome 2 if $p_2 \leq \alpha/(n-1)$, and at step j , reject a null effect for outcome j if and only if null effects have been rejected for all outcomes $i < j$, and $p_j \leq \alpha/(n-j+1)$. Hochberg (1988) provides a step-up modification of this procedure, which rejects null effects for all outcomes $i \leq j$ if $p_j \leq \alpha/(n-j+1)$ for any $j=1, 2, \dots, n$.

The disadvantages of these approaches are that the larger the number of outcomes in the family, the smaller the average power for testing each individual outcome. Furthermore, these tests are conservative, as they are based on the assumption of independence between outcomes. This is certainly not the case in our application, where most outcomes within families are very closely related to one another. We therefore follow Katz, Kling, and Liebman (2007) in calculating bootstrapped estimates of adjusted p -values using a modification of the free step-down algorithm of Westfall and Young (1993).²⁹ This approach uses the correlation across test statistics to increase the power for testing each individual outcome.

Table 11 shows the eighteen outcomes for which the experimental estimates are significant (with controls added) at the 10% level when examined individually. If a researcher were examining migration papers for evidence of a significant impact of migration on, say, working-age adult BMI, then the p -value of 0.054 is strongly suggestive that migration lowered BMI. However, a researcher reading our paper to see what the significant effects of migration are should have a lot more caution in interpreting these BMI results; they may just be the outcome observed by chance to be significant among a whole host of health outcomes that are being examined at the same time. In contrast, the adjusted p -value of 0.211 for adult BMI means that if one were to search for an effect among the twelve different working-age adult outcomes in table 8, at least one effect this large would be observed 21.1% of the time.

Given the loss of power involved in multiple testing and our small sample sizes, we fix the family-wise error rate at 10%. If we were to consider all 62 outcomes as a family, the Bonferroni p -value is thus 0.0016. The only outcomes that are significant at this level are total household size and the number of adults aged 18 to 45. The Holm and Hoch-

²⁸ The description of methods here is based on Shaffer (1995).

²⁹ See appendix A of Katz et al. (2007) for a detailed description of how this is implemented.

²⁷ See, for example, O'Brien (1984) and Kling and Liebman (2004).

TABLE 11.—*P*-VALUES AND FAMILY-WISE ADJUSTED *P*-VALUES FOR MODELS WITH CONTROLS

	Single Variable <i>p</i> -Value	Adjusted <i>p</i> -Value for Family-Wise Comparison		
		Bonferroni	Holm	Westfall-Young
				Step-Down MinP
Adults aged 18 to 45	0.000	0.000	0.000	0.000
Household size	0.002	0.006	0.005	0.000
Net remittances per capita	0.003	0.032	0.032	0.084
Household has ATM card	0.005	0.041	0.041	0.127
Net remittances per adult	0.005	0.063	0.058	0.120
Number meals of fruits and vegetables	0.008	0.055	0.055	0.114
Household labor earnings per adult equivalent	0.009	0.107	0.089	0.137
Household labor earnings per capita	0.014	0.174	0.130	0.177
Adult waist-to-hip ratio	0.015	0.181	0.181	0.052
Number of chickens	0.047	0.423	0.376	0.369
Number meals of roots	0.047	0.329	0.282	0.307
Number meals of fats	0.050	0.353	0.252	0.307
Adult BMI	0.054	0.653	0.598	0.211
Household has bank account	0.058	0.522	0.406	0.484
English literacy for children	0.065	0.455	0.455	0.770
Adult main activity is agriculture	0.093	1.000	0.926	0.339
Log total income per adult equivalent	0.094	1.000	0.752	0.421
Adult business owner	0.096	1.000	0.867	0.339

This table shows the eighteen outcomes for which the experimental estimates are significant (with controls added) at the 10% level when examined individually. See the text for more information about how the adjusted *p*-values are calculated.

berg and Westfall-Young adjustments do not reveal any other outcomes to be significant. That is, we can be very confident that migration lowers the size of the sending household, a none-too-startling result.

A slightly less conservative approach is to consider the outcomes in each table as a family of outcomes. The second, third, and fourth columns of table 11 provide adjusted *p*-values for this family-wise comparison. Doing this again reveals that in addition to lowering household size, we can be confident that migration raises the amount of remittances the sending household receives and that migration has lowered adult waist-to-hip ratios among remaining adults. After the Westfall-Young adjustments, four other outcomes are marginally significant (in the 0.10 to 0.14 adjusted *p*-value range):³⁰ the fall in household labor earnings per adult, the increase in net remittances per adult, the decline in household ATM card ownership, and a drop in the number of meals of fruits and vegetables eaten by the household. None of the individual-level outcomes for older adults or children (tables 9 and 10) are significant when we adjust for multiple hypothesis testing.

We also carried out the family-wise comparisons for the nonexperimental estimates using the full nonapplicant sample and for those using the HIES sample. These results confirm that we would arrive at different conclusions to those found using the experimental approach for some household-level outcomes.³¹ For example, using the nonapplicant

sample, we would falsely conclude that migration increased the value of household durables and the number of cars in the household, which in fact reflects the positive selection of households into migration. Using the HIES sample, we would falsely conclude that subsistence income has risen significantly with migration and that migration through the PAC was actually lowering remittances. This negative result is likely generated by spatial differences since the HIES survey does not allow us to match on the geographical distribution of migrants like we do with the nonapplicant sample. In contrast, in keeping with our previous discussion that there is less selectivity when comparing the individual stayers in ballot-winner households to other individuals, the experimental and nonexperimental estimates give similar results for individual-level outcomes.

VIII. Conclusion

In this paper, we have made two innovations to advance the literature on estimating the impact of migration on development. First, we have used an unusual randomized migration policy along with data collected specifically to exploit the experimental variation provided by this policy to estimate the true short-run impact of migration on household members remaining in a developing country after some family members have migrated to a developed country. We have also demonstrated that both the selection of households into migration and the decision among migrant households whether to send a subset of members or to migrate en masse biases nonexperimental estimates of the impact of migration on development. Second, in contrast to most studies, which examine the impact of migration on at most a few outcomes, we examined the impact on a comprehensive set of household and individual-level develop-

³⁰ Note that when the treatment tends to operate in the same direction on the different outcomes in a family, the Westfall-Young *p*-values are smaller than the more conservative Bonferroni and Holm *p*-values. However, in some of the families of outcomes examined here, there can be a negative correlation. For example, the more positive and significant is the increase in remittances from migration, the less negative (and hence less significantly negative) is the fall in total income.

³¹ Results not shown for reasons of space, but available on request.

ment indicators, including income, asset ownership, labor supply, business ownership, physical and mental health, and child education.

Our results suggest that family members remaining in Tonga may initially be made worse off in terms of household resources per capita available after some of their household members immigrate to New Zealand and that failure to account for the double selectivity of migration would miss most of this impact. Households sending migrants are smaller in size and receive more remittances per capita. However, the amount received in remittances and the reduction in household size is not enough to compensate for the lost labor earnings of the migrants, leading to sizable reductions in household income per capita. Migrant-sending households also appear to have fewer durable assets and livestock and are less likely to have access to banking services, such as ATM cards. Without direct evidence on how resources are shared within a household, we cannot definitively conclude that this results in fewer resources actually available to the household members left behind. However, our indirect tests suggest evidence of income pooling, and we also find evidence of reductions in adult waist-to-hip ratios and BMI, which is consistent with less food intake among left-behind members.

While the results in this paper concern the impact of migration on a relatively small number of Tongan households migrating to New Zealand, we believe they offer broader lessons for other studies. First, from a substantive viewpoint, positive self-selection of households into international migration is a widespread pattern. For example, Grogger and Hanson (2008) show that for almost all countries, there is positive selection on education in terms of migration to the OECD. The existing literature has focused much less on within-household selectivity into international migration, but it seems reasonable that the same policy environment that leads to positive selection at the household level will also lead to positive selection within households. As a result, our general results that nonexperimental estimates will tend to overstate the household-level benefits of having members migrate if they cannot adequately control for this selection, and that selectivity should matter less for comparing the individuals left behind to the general population are likely to apply more broadly.

Second, our findings draw attention to the importance of selectivity as to which migrant households migrate en masse and which have members remaining behind, which the literature has generally ignored. This has implications for both future data-gathering exercises and future analysis in other studies. Researchers need to pay more attention to the policies that determine which individuals within a household are able to join a migrant and use this information to form better comparison groups among nonmigrant households for the stayers in migrant households. Household surveys rarely ask about the policies under which migrants are emigrating, so collecting this information in future survey efforts will be important. In addition, collecting a full matrix of relation-

ships within a household, rather than just relationship to the household head, as is commonly done, would enable researchers to better identify which other household members would be legally eligible to move if a particular individual in the household were to migrate.

Third, most studies of migration rely on cross-sectional surveys with little information available on the characteristics of the household premigration and on individual-level behaviors and outcomes. Further data collection efforts should endeavor to collect much more information on asset ownership and labor earnings in the past, enabling researchers to more closely match migrant households to households that were similar to them before the migration event. In our survey, households were able to retrospectively provide information on labor earnings in the past two years, which yielded premigration data for the recent migration experience on which we focus. Additional research is needed to ascertain what information can be accurately recalled over longer periods. There should also be more effort made to better measure the distribution of resources within households and to collect more individual-level outcomes (for example, on individual diets, mental health, personal spending, and height and weight) that can be used to measure changes in individual well-being without relying on assumptions about intrahousehold allocation of resources.

Finally, we would be remiss not to note that although our findings give a less rosy picture of the impact of migration on the incomes and wealth of household members left behind than is provided by much of the existing literature, we are examining only short-run impacts. While this has the advantage that we are not mixing together households whose emigrants have different lengths of time in the receiving country, as do many papers in the literature, it is possible that the impacts will differ in the long run. More important, the migration treatment studied in this paper is the combination of emigration and restriction on which family members can accompany the principal migrant. Other research with PINZMS data shows that individuals who move to New Zealand experience large gains in income (McKenzie et al., 2010) and improvements in mental health (Stillman et al., 2009). These positive impacts would likely extend to the remaining family if they were also allowed to move with the migrant. Since almost all migrant-destination countries impose age and relationship rules blocking certain family members from accompanying migrants, there may be millions of migrant-sending households in the developing world whose remaining members become worse off. The methodological comparisons in this paper suggest that these negative impacts would be unlikely to be detected by conventional methods and data used in previous studies of the development impact of migration.

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TABLE APPENDIX

TABLE A1.—SAMPLE SIZE

	Full Sample	Dropping All Movers	Percentage All Movers	Dropping Individual Movers	Percentage Individual Movers
Individuals					
Unsuccessful ballots	654	326	50%	160	51%
Successful ballots—noncompliers	115	31	73%	11	65%
Successful ballots—migrants ^a	283	283		272	4%
Nonapplicants	727	641	12%	604	6%
Households					
Unsuccessful ballots	124	49	60%		
Successful ballots—noncompliers	26	8	69%		
Successful ballots—migrants	61	61			
Nonapplicants	124	107	14%		
Relationship to Principal Applicant in Migrant Households		All Individuals	Percentage	Dropping Individual Movers	Percentage
Spouse		4	1.4%	2	0.7%
Own/adopted child		18	6.4%	9	3.3%
Son/daughter-in-law		6	2.1%	6	2.2%
Parent		45	15.9%	45	16.5%
Parent-in-law		4	1.4%	4	1.5%
Brother/sister		73	25.8%	73	26.8%
Other relative		132	46.6%	132	48.5%
Nonrelative		1	0.4%	1	0.4%
Individuals		283		272	

The nondropped spouses and own/adopted children are all mothers with young children. ^aIncludes successful ballots with members remaining in Tonga.