Migration, Income and Health: Evidence from a Natural Experiment

John Gibson
Department of Economics
University of Waikato

Abstract
Over four million people emigrate every year in search of better economic and social opportunities. World Bank calculations suggest that restrictions on these people flows have larger welfare costs than the more widely studied restrictions on international trade. However, measuring the effects of migration is complicated by non-random selection of migrants from the general population, which makes it hard to obtain an appropriate comparison group of non-migrants. In this paper, estimates of the income and health effects of immigration are obtained by comparing immigrants who enter New Zealand through a random ballot with unsuccessful participants in the same immigration ballots who remain in their home countries in the Pacific Islands. Surveys of non-applicants are also used to see how well the non-experimental data and methods typically used by economists can replicate the experimental results. Immigrants appear to be positively selected in terms of both observed and unobserved skills. As a result, non-experimental methods overstate the income gains from migration, so calculations of global benefits from increasing people flows are likely to be sensitive to the modelling of this selectivity bias. However, the income gains for immigrants appear larger than expected by potential emigrants and are not associated with all of the adverse health consequences of immigration that are suggested in the literature.

Keywords: Health, Migration, Natural Experiment, Selection
JEL codes: C93, F22, I12, J61, 015

Acknowledgements:
This lecture builds on surveys and joint work conducted with Halahingano Rohorua, Azmat Gani, and especially David McKenzie and Steven Stillman. I am particularly grateful to these colleagues for allowing our joint work to be presented here. Thanks are due to the Government of the Kingdom of Tonga for permission to conduct the survey there, the New Zealand Department of Labour Immigration Service for providing the sampling frame, and most especially the survey assistants and respondents. Helpful comments and assistance have been received from Mary Adams, Steven Stillman, David McKenzie and Geua Boe-Gibson. The study was approved by the multi-region ethics committee of the New Zealand Ministry of Health. Financial support from the World Bank, Stanford University, the Waikato Management School and Marsden Fund grant UOC0504 is gratefully acknowledged. The views expressed here are those of the author alone.
I. Introduction
The effects of immigration on income and health are topics of obvious interest in Australia and New Zealand. Almost one-quarter of the population in these two countries are first generation immigrants, which is one of the highest rates in the developed world (United Nations, 2002). Moreover, many of these immigrants come from countries that depend on economic development assistance from Australia and New Zealand. This is especially the case for emigration from the Pacific Islands to New Zealand. Therefore the income and health of immigrants is important not only for their own-wellbeing and for their impact on public revenues and productivity in host countries, but also because it affects the source countries through their ability to send remittances. Consequently there has been considerable recent debate about the role of labour mobility as a development policy in the Pacific (World Bank, 2006).

There is also broader interest in the labour market performance of immigrants because of the potential global welfare gains from reallocating labour across countries. In an influential study, Walmsley and Winters (2005) use cross-country wage differentials as a measure of the income gain from migration and estimate that a modest increase in migration from developing to developed countries would lead to a gain in world income exceeding the gains to be had from removing all remaining barriers to goods trade. Similar calculations are reported by the World Bank (2006a), who model a migration shock that raises the share of developing country immigrants in the labour force of developed countries from 5.8 percent in 2001 to 8.8 percent in 2025 (compared with a baseline of 6.0 percent in 2025). After adjusting for the higher cost of living in rich countries and the effect that this movement of 14 million workers would have on prices, the global gains are estimated to be $356 billion in 2025; equivalent to a 0.6 percent increase in global income. These gains are skewed more toward the developing countries (including the new immigrants) whose income rises by 1.8 percent compared with 0.4 percent for the developed countries. In contrast, complete liberalization of goods trade brings smaller gains, and the gains to poor countries are only slightly larger than those for the rich countries.

The scope for freer international movement of labour is also highlighted by Freeman (2006) who calculates that trade and international capital flows are a larger proportion of activity in goods and capital markets than immigration is in labour markets. Consequently, there is much greater international dispersion in wages than in goods and capital prices. This dispersion can lead to considerable migration pressure, as evident both from the large number of illegal immigrants and the dire predictions sometimes made about hordes of immigrants moving to countries or regions that contemplate lowering immigration restrictions.

However, estimates of the income gains from migration, and consequent people flows motivated by these gains, that are based on either simple cross-country comparisons of wage rates or on earnings for existing immigrants may be misleading. Measuring the effects of migration is complicated by non-random selection of migrants from the general population, which makes it

---
1 To put this in context, estimates of the gains from a global elimination of immigration restrictions made by Moses and Letnes (2004) range from a minimum of 5.6 percent of world GDP to a maximum which exceeds world GDP.
2 Using the World Bank’s trade model and scaling the migration and trade reform scenarios to the same year, the gains from trade reforms are only 89 percent of those from the three percent rise in global migration.
3 For example, newspapers, politicians and opinion polls speculated that up to 20 million East Europeans would emigrate once the Eastern European Accession countries joined the European Union (Bauer and Zimmerman, 1999). In fact there appears to have been far less migration than many predicted (Traser, 2005).
hard to obtain an appropriate comparison group of non-migrants. Ideally, one must compare the earnings (or health or other characteristics of interest) of the immigrant to what they would have earned in their home country. The latter is unobserved, and is usually proxied by the earnings of stayers of a similar age and education to the immigrant. But if the two groups are really the same, they should have the same migratory behaviour (Lalonde and Topel, 1997). Simple comparisons of movers and stayers are therefore likely to be misleading, as income gains may just reflect unobserved differences in ability, skills, and motivation, rather than the act of moving itself.

In this paper, estimates of the income and health effects of migration that account for this selectivity problem are obtained by comparing immigrants who enter New Zealand through a random ballot with unsuccessful participants in the same immigration ballots who remain in their home countries in the Pacific Islands. This provides the only known experimental measure of the income and health changes due to migration. As not all individuals with successful ballots had migrated by the time of the survey used here, this estimate accounts for non-compliance to the “treatment” of migration.

Surveys of non-applicants are also used to see how well the non-experimental data and methods that are typically used by economists can replicate the experimental results. Five popular non-experimental methods for dealing with selectivity are considered. Each of these methods overstates the income gains from immigration by between eight percent and 83 percent compared to the experimental estimate. This comparison of experimental and non-experimental estimators provides a new variant to the literature started by Lalonde (1986), and continued by Heckman, Ichimura and Todd (1997), Dehejia and Wahba (2002) and Smith and Todd (2005), which examines how well non-experimental estimators replicate experimental estimates in the context of labour market training programs. In comparison with that literature where the “treatment” raises earnings by about 30 percent, there is a 250 percent increase in earnings following immigration to New Zealand and yet even this substantial treatment effect is overstated by the non-experimental estimators.

The overstatement by the non-experimental methods suggests that migrants are positively selected in terms of unobserved ability and skills. Direct evidence on this is reported in Section III, along with the experimental and non-experimental estimates of the income gains from immigrating to New Zealand. The description of the immigration process used as the natural experiment and the sampling method and survey data are described prior to that in Section II. Although the overstated income gains with non-experimental methods may suggest that increased people flows are less desirable than is sometimes suggested, a full evaluation has to consider other impacts of immigration as well. In this regard Section IV considers whether immigrants suffer from unmet income expectations and whether their health suffers as a result of immigration, which is a pattern often reported in the literature (Antecol and Bedard, 2006). The conclusions are reported in Section V.

II. The Pacific Access Category and PINZMS Data
The natural experiment used here is based on the Pacific Access Category (PAC) under New Zealand’s immigration policy. The PAC was established in 2001 and allows quotas of 250
Tongans and 250 Fijians to immigrate to New Zealand each year. Applicants to this category must be aged 18 to 45, meet a minimum level of English language ability, meet health and character requirements, and have an offer of employment in New Zealand. Applicants to the PAC first register for the quota by filling out a form within a one month window each year. Many more registrations are received than the quota provides for, and so the New Zealand Department of Labour conducts a computer ballot to randomly select amongst the registrations. The odds of success in this ballot are less than 10 percent. Individuals who are selected in this ballot are then notified and invited to apply for residence within six months. It is at this stage of the residence application that applicants must provide evidence of the job offer in New Zealand. Because of the low odds of success, few applicants arrange the job offer before this stage. Once applications are approved, those with successful ballots can then move to New Zealand with permanent residence, and bring their spouse and dependent children with them.

Thus, there is a group of immigrants and a comparison group similar to the immigrants but remaining in the Pacific Islands because they were not successful in the random ballot. It is shown by McKenzie, Gibson and Stillman (2006) that, at least in the case of Tonga, the other options for emigrating are fairly limited unless applicants have close family members abroad. Such individuals would probably have already emigrated via these other arrangements given the low odds of having a successful PAC ballot. Hence, substitution bias, which will occur if PAC applicants with unsuccessful ballots emigrate through an alternative visa category such as the family or skills category, should not be a serious concern in this setting.

The Pacific Island-New Zealand Migration Survey (PINZMS) is a comprehensive cross-country survey based on the PAC and designed to measure multiple aspects of the migration process. Eventually PINZMS will track immigrants and non-immigrants over a 3-5 year period but only the first wave of the survey is currently available. There are five samples in the overall survey:

1. 120 migrant households in New Zealand who were successful participants in PAC ballots between 2002/03 and 2004/05 and who had migrated to New Zealand by the time of the survey in 2005/06 (101 from Tonga and 19 from Fiji),
2. 56 households who were successful participants from the same PAC ballots but were still in either Tonga (n=55) or Fiji (n=1), either because their application for New Zealand residence was not approved (typically because of lack of a suitable job offer) or was still being processed. These are non-compliers to the immigration “treatment”,
3. 153 households who were unsuccessful participants from the same PAC ballots and who were still either in Tonga (n=103) or Fiji (n=50). These are the experimental controls,
4. 127 households who had never entered the PAC ballots and who were sampled from the same villages as the successful and unsuccessful PAC applicants (85 from Tonga and 42 from Fiji). These are the non-experimental control group,

---

4 Smaller quotas were created for Kiribati and Tuvalu and there is a long-standing (and larger) quota for Samoa. In addition to labour market considerations, these programs reflect broader development and political relationships between New Zealand and these Pacific Island countries.
5 Applicants with dependents must also meet a minimum income requirement. The person who registers is a Principal Applicant. If they are successful, their immediate family (spouse and children under age 18) can also apply to migrate as Secondary Applicants. The quota of 250 applies to the total of Primary and Secondary Applicants.
6 In addition to the author, the survey has been designed and overseen by David McKenzie and Steven Stillman and implemented by Halahingano Rohorua, Azmat Gani and their assistants.
5. 68 households who were family members of the immigrants in New Zealand and who were living in the same dwelling that the migrants had been living in prior to leaving for New Zealand (60 from Tonga and 8 from Fiji). These households are mainly sampled to look at the effect of emigration on source families and communities, and are not used in the analyses reported below.

Survey weights have been constructed to reflect the different sampling rates used for each group. Many of the analyses reported below are restricted to the Tongan component of the PINZMS which has a larger sample size and has been available for longer. The relationship between these samples and the PAC ballot process is illustrated in Figure 1.

Figure 1: The Immigration Ballot and the Four Household Samples

The topical coverage of the survey includes: household demographics, education, labour supply, income, asset ownership and food consumption, based where possible on the most widely used surveys in New Zealand and the Pacific Islands to enhance comparability. It also has a very detailed module on health, containing subjective questions on health status, questions on smoking and alcohol use, self-reports of diabetes and hypertension, and measurement of blood pressure, waist circumference, height and weight of all household members. In addition there are a number of distinct modules on the migration process, remittance transfers, knowledge and use of the financial system, expectations of future income and of future remittance patterns, and other questions on linkages between immigrants and their families.

Table 1 examines how random the sample is in the case of the initial Tongan component of the PINZMS, by comparing means of ex-ante characteristics for successful and unsuccessful ballots. The point estimates of the means are similar in magnitude for the two groups and equality of means cannot be rejected for any of the variables. This is as would be expected with the random selection of ballots among applicants to the Pacific Access Category.
Table 1: Evidence for Randomization in the Samples of PAC Ballot Applicants

<table>
<thead>
<tr>
<th></th>
<th>Successful Ballots</th>
<th>Unsuccessful Ballots</th>
<th>T-test of equality of means</th>
<th>p-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age</td>
<td>33.6</td>
<td>33.7</td>
<td></td>
<td>0.91</td>
</tr>
<tr>
<td>Years of schooling</td>
<td>11.9</td>
<td>11.5</td>
<td></td>
<td>0.37</td>
</tr>
<tr>
<td>Proportion male</td>
<td>0.55</td>
<td>0.51</td>
<td></td>
<td>0.52</td>
</tr>
<tr>
<td>Proportion born on Tongatapu</td>
<td>0.75</td>
<td>0.79</td>
<td></td>
<td>0.54</td>
</tr>
<tr>
<td>Proportion who had been to NZ before 2000</td>
<td>0.39</td>
<td>0.35</td>
<td></td>
<td>0.63</td>
</tr>
<tr>
<td>Proportion who are married</td>
<td>0.60</td>
<td>0.62</td>
<td></td>
<td>0.77</td>
</tr>
<tr>
<td>Height</td>
<td>171.6</td>
<td>169.3</td>
<td></td>
<td>0.16</td>
</tr>
<tr>
<td>Income in 2003/before moving</td>
<td>103.7</td>
<td>88.0</td>
<td></td>
<td>0.32</td>
</tr>
<tr>
<td>Total Sample Size</td>
<td>120</td>
<td>78</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: Authors’ calculations from the Tongan component of the PINZMS data collected in 2005.

Valid comparisons of the experimental and non-experimental estimators depend on successfully sampling non-applicant households from the same areas as the immigrants and unsuccessful ballot applicants. Figure 2 shows results of this strategy according to the spatial distribution of the four samples of households on the main island of Tongatapu (smaller samples were collected on the outer islands and are not illustrated here).

Figure 2: Evidence for spatial matching of the four samples
III. Experimental and Non-experimental Estimates of the Income Gains from Migration

The natural experiment provided by the use of a random ballot to allow Pacific Islanders to apply to immigrate to New Zealand provides a unique opportunity to estimate the gain in income from migration. Other studies of migration are forced to use non-experimental methods that attempt to deal with the selectivity issues inherent in comparing migrants to non-migrants. The extent to which results based on this natural experiment can generalize to these non-experimental studies depends partly on whether Tongans immigrating to New Zealand are typical of developing country migrants elsewhere in the world. In this regard, they have 11.7 years of education, compared to 11.0 years for the average new arrival of the same age (18-45 years) in the United States, are as equally likely to work as new immigrants to the United States, and have similar age, marital status and gender mix to immigrants into the U.S. and Canada.

In terms of immigrant selectivity, the Tongan immigrant sample averages 1.2 more years of schooling than the non-applicants, a similar degree of positive selection on observables to the 0.8 years higher education of Mexican immigrants moving to the U.S. However, the concern with non-experimental estimators is not the selectivity on observable variables like education, but instead on unobservables. In this regard, the pre-migration income in Tonga of the immigrants is from 34-88 percent higher than that of the non-applicant sample (where the range depends on the comparison group), even after controlling for age, gender, marital status, height and place of birth. It is therefore expected that this positive selection on unobservables will cause non-experimental estimators to overstate the income gains from migration.

Experimental Methods

In an ideal experiment outcomes for the experimental control group provide a counterfactual for what would have happened to the treatment group if they had not been treated. In the current case, comparing earnings for immigrants (Group 1) and PAC applicants with unsuccessful ballots (Group 3) seems to fit this model. However this simple experimental estimator of the treatment effect on the treated (SEE-TT) is biased if control group members substitute for the treatment with a similar program or if treatment group members dropout of the experiment (Heckman, Hohmann, Smith and Khoo, 2000). As discussed above, substitution bias is unlikely here because individuals with the ability to migrate via other arrangements will likely have done so previously given the low odds of having a successful PAC ballot. However, dropout bias is a more relevant concern because many PAC applicants with successful ballots had not yet immigrated to New Zealand at the time of the survey. Most were in the process of moving, with the rest unable to move due to the lack of a valid job offer.

The impact of dropout bias on the SEE-TT can be illustrated by writing the income of PAC ballot applicant \(i\) as:

\[
\text{Income}_i = \alpha + \beta \text{BallotSuccess}_i + \nu_i, \text{ where } E(\nu_i)=0. 
\]

---

7 Full coverage of the results in this section is in McKenzie, Gibson and Stillman (2006) and is based on the Tongan component of the PINZMS collected in 2005.

8 Specifically, the immigrants had pre-migration earnings that were $29 per week higher than for the sample of all 18-45 year olds in the non-applicant households (whose mean earnings were $33 per week). If attention is restricted to the oldest member aged 18-45 in the non-applicant households, mean earnings for immigrants were $22 higher than for these pseudo-applicants (whose mean earnings were $61 per week).
Where BallotSuccess\textsubscript{i} is a dummy variable taking the value one if the PAC applicant has a successful ballot and zero if it is unsuccessful, and alternatively as:

\[ \text{Income}_i = \mu + \lambda \cdot \text{Migrate}_i + \varepsilon_i, \text{ where } E(\varepsilon_i) = 0. \]  

(2)

Where Migrate\textsubscript{i} is a dummy variable taking the value one if person \textit{i} immigrates and zero otherwise, and \( \lambda \) is the effect of immigration (the average treatment effect on the treated).

The SEE-TT of the gain in income from immigration is calculated as the difference in mean income between those with successful ballots who immigrate and those with unsuccessful ballots:

\[ \text{SEE-TT} = E[\text{Income}_i | \text{Migrate}_i=1] - E[\text{Income}_i | \text{BallotSuccess}_i=0] \]  

(3)

Substituting in equation (2) shows that:

\[ \text{SEE-TT} = \lambda + E[\varepsilon_i | \text{Migrate}_i=1] - E[\varepsilon_i | \text{BallotSuccess}_i=0] \]  

(4)

Thus, the SEE-TT will only be an unbiased estimate of \( \lambda \) if the last two terms in equation (4) sum to zero. Because ballot success is determined randomly, \( E(\varepsilon_i|\text{BallotSuccess}=0) \) can be replaced with \( E(\varepsilon_i|\text{BallotSuccess}=1) \). Hence the condition for the SEE-TT to be an unbiased estimate of the treatment effect on the treated is:

\[ E[\varepsilon_i | \text{Migrate}_i=1] = E[\varepsilon_i | \text{BallotSuccess}_i=1]. \]  

(5)

In other words, that there is no selection as to who migrates among those whose ballot was successful. This condition does not seem likely to hold, and in this case estimating the impact of migration requires using econometric methods even when experimental data are available.

The first econometric approach starts with the “intention-to-treat” (ITT) effect, which is the earnings difference between all ballot winners (regardless of whether migrated) and unsuccessful ballots:

\[ \text{ITT} = E[\text{Income}_i | \text{BallotSuccess}_i=1] - E[\text{Income}_i | \text{BallotSuccess}_i=0] \]  

(6)

The ITT is given by \( \beta \) in equation (1), while a more efficient estimate may result from adding control variables, \( X_i \) for the observable pre-existing characteristics of the two groups:

\[ \text{Income}_i = \alpha + \beta \cdot \text{BallotSuccess}_i + \delta X_i + \omega_i \]  

(7)

Heckman et al. (2000) demonstrate that if the following three assumptions hold: 1) there is no substitution for the migration treatment, 2) dropouts among the successful ballots are otherwise unaffected by having a successful ballot, and 3) dropouts among the successful ballots have the same mean outcome as unsuccessful ballots who would have been dropouts if they had a successful ballot, then an unbiased estimate of the average treatment effect on the treated can be calculated which is adjusted for dropout bias (ADJ-TT):

\[ \text{ADJ-TT} = \text{ITT} / p \]  

(8)

where \( p \) is the proportion of non-dropouts (which was 0.33 for the initial Tongan data collected in PINZMS).

The second approach to dealing with dropout bias is to use Instrumental Variables (IV), which gives unbiased estimates of average treatment effects (Angrist, Imbens and Rubin, 1996). The
ballot outcome is strongly correlated with migration, and is a valid instrument because randomization (see Table 1) ensures that ballot success is uncorrelated with unobserved attributes that might also affect earnings. This estimator gives the local average treatment effect (IV-LATE), interpreted as the effect of treatment on individuals whose treatment status is changed by the instrument. In the current application, this is the effect of migration on the income of individuals who migrate after winning the lottery. Angrist (2004) also demonstrates that in situations where no individuals who are assigned to the control group receive the treatment (i.e., there is no substitution) then the IV-LATE is the same as the average treatment effect on the treated (IV-TT).

**Experimental Results**

Experimental estimates of the income gains from immigrating to New Zealand from Tonga are estimated using both equation (8) and the IV approach. As a start, Figure 3 shows mean weekly earnings among the different samples in PINZMS. All of the estimates are in New Zealand Dollars, with earnings in Tonga converted at the market exchange rate of 1 Pa’anga=0.73 NZD (= 0.53 USD). A t-test strongly rejects the null hypothesis of equality of immigrant earnings with earnings of any of the other groups. The mean earnings for immigrants are $424, compared to $104 for unsuccessful applicants in the PAC ballot. So the (potentially biased) SEE-TT suggests that immigration to New Zealand raised earnings by $320 per week. However this estimator does not take account of the non-compliers (Group 2).

Figure 3: Mean Weekly Earnings (± 1 standard error)

---

9 Dropping the BallotSuccess dummy variable from the first-stage model for migration gives an F-statistic of 66.5.

10 Results are similar if PPP exchange rates are calculated from prices we gathered in Nuku’alofa and Auckland for various Tongan consumption bundles. See McKenzie, Gibson and Stillman (2006) for details.
Comparing ballot winners (the weighted average of Groups 1 and 2) and ballot losers gives the intention to treat effect. According to equation (6), ITT=$(195-104)=91$. In other words, having a successful ballot raises expected earnings by $91 per week. This result is also reported in column (1) of Table 2 as estimated in a simple OLS regression model (equation (1)). After adjusting the ITT for non-compliance, using equation (8), immigration is estimated to have raised the weekly earnings of Tongans by $274. Similarly, the simple IV-TT estimate of the average treatment effect on the treated is presented in column (3) of Table 2 and shows the same treatment effect of $274.

In Columns (2) and (4) controls for pre-existing characteristics of applicants are added to the regression models. Controlling for age, sex, marital status, years of education, height (as a pre-existing measure of health), being born on the main island of Tongatapu (as a measure of having more urban skills), and past income, only marginally changes the estimated ITT effect, from $91 to $87, and does not change the IV-TT estimate. The fact that the estimated program effects change only slightly in magnitude as controls are added is consistent with the result in Table 1, which showed the randomization achieved by the ballot.

Given that mean income of PAC applicants with unsuccessful ballots is $104, these results indicate that Tongans experience an 88 percent increase in expected income from having a successful PAC ballot. More relevantly to the study of migration, they experience a 263 percent increase in average weekly labour income from immigrating to New Zealand.

<table>
<thead>
<tr>
<th></th>
<th>OLS</th>
<th>OLS</th>
<th>IV</th>
<th>IV</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ballot Success Dummy</td>
<td>90.634</td>
<td>87.390</td>
<td>91.390</td>
<td>87.390</td>
</tr>
<tr>
<td></td>
<td>(3.68)***</td>
<td>(3.89)***</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Male Dummy</td>
<td>-23.855</td>
<td>-27.772</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.08)</td>
<td>(1.33)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Married Dummy</td>
<td>24.535</td>
<td>18.376</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.05)</td>
<td>(0.82)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Age Dummy</td>
<td>-0.886</td>
<td>-0.462</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.71)</td>
<td>(0.41)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Years of Education</td>
<td>4.605</td>
<td>3.274</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.18)</td>
<td>(0.91)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Born on Tongatapu Dummy</td>
<td>27.600</td>
<td>28.005</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.87)*</td>
<td>(2.04)**</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Height</td>
<td>0.381</td>
<td>0.353</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.92)</td>
<td>(0.93)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Past income</td>
<td>0.662</td>
<td>0.660</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(6.98)***</td>
<td></td>
<td>(7.31)***</td>
<td></td>
</tr>
<tr>
<td>Migration Dummy</td>
<td>273.996</td>
<td>273.736</td>
<td>274.996</td>
<td>274.736</td>
</tr>
<tr>
<td></td>
<td>(4.46)***</td>
<td>(4.49)***</td>
<td>(4.99)***</td>
<td>(4.99)***</td>
</tr>
<tr>
<td>Constant</td>
<td>104.051</td>
<td>-60.422</td>
<td>104.051</td>
<td>-48.595</td>
</tr>
<tr>
<td></td>
<td>(8.85)***</td>
<td>(0.74)</td>
<td>(8.90)***</td>
<td>(0.66)</td>
</tr>
<tr>
<td>First stage F-statistic on instrument</td>
<td>66.53</td>
<td>61.51</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>197</td>
<td>190</td>
<td>197</td>
<td>190</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.04</td>
<td>0.27</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Robust t statistics in parentheses; statistically significant at 10% (*), 5% (**) and 1% (***), level
Non-experimental Methods

Five non-experimental estimators are used to see how well methods typically used in migration studies cope with the positive selection on unobservable attributes and replicate the experimental estimate of an income gain due to immigration of $274 per week. These methods are:

1. a single difference estimator which compares immigrants’ post-migration income to their pre-migration income. Possible biases in this estimator are that it ignores changes in the counterfactual income since immigration such as from aggregate factors affecting earnings in the source country and time-varying skill levels (e.g. the accumulation of labour market experience);

2. OLS regression, which assumes selection on observables and hence will be biased if there is also selection on unobservables. To implement this method the sample of immigrants in New Zealand is combined with the non-applicant sample (Group 4) in Tonga. The oldest member aged 18-45 in the non-applicant household was used as the pseudo-applicant and the control variables are the same as used above in Table 2;

3. a form of difference-in-differences regression estimation, which uses retrospectively reported earnings for immigrants (pre-migration) and pseudo-applicants. Although this should control for time invariant individual attributes, some unobserved characteristics like drive and ability may be rewarded differently in the New Zealand and Tongan labor markets, so in effect they become time-varying. Also, to the extent that this involves comparing wage changes for immigrants with changes for, potentially, not-very-similar non-immigrants, the assumption of a common underlying trend in labour income may not tenable;

4. propensity-score matching, which attempts to ensure that the immigrants are compared only to observationally “similar” non-immigrants. In addition to the standard approach of using a probit equation for the probability of migrating, and matching each immigrant to non-applicants with similar predicted propensities to migrate,\footnote{Nearest-neighbor matching where each migrant is matched to the four nearest non-migrant neighbors is used.} the bias-adjusted matching estimator of Abadie and Imbens (2006) which matches directly on the covariates is also used. The same covariates listed in Table 2 are used, with results calculated with and without past income used in the set of matching variables;\footnote{The literature suggests that difference-in-difference matching out-performs cross-sectional matching (Smith and Todd, 2005), so the inclusion of past income provides an estimator similar in spirit to difference-in-difference matching.}

5. instrumental variables, which in principal can deal with the omission of unobservable determinants of migration and incomes, if suitable instruments can be found. Two are considered: the pre-existing family network in New Zealand which follows the use of networks as an instrument for migration in other settings (Woodruff and Zenteno, 2006) and the pre-migration distance from place of residence to the office in Tonga where PAC ballot information is obtained and registrations deposited. The distance instrument is motivated by the survey finding that 98 percent of non-applicants indicated that their not knowing the requirements of the PAC program was a very important reason for not applying. The validity of the network instrument is threatened by the immigrants relying on their family network to find a job offer in New Zealand, which gives the network a direct effect on earnings rather than operating only via the migration decision. The validity of the distance instrument would be threatened if people who live close to the capital city in Tonga (where the ballot office is) adapt more readily to urban life in New Zealand, giving distance a direct effect on post-immigration earnings through its role as a proxy for accumulated urban experience.
Non-experimental Results

Table 3 contains a summary of the non-experimental estimates. Each non-experimental method overstates the gain in income from immigration compared to the experimental estimate. This suggests that methods typically used in migration studies cannot fully cope with positive selection on unobservable attributes.

The instrumental variables estimator using the distance instrument performs best, giving an estimate of $295 per week which only overstates the experiment estimate of the gains by a statistically insignificant eight percent.\(^{13}\) The distance from pre-migration residence in Tonga to the office where ballot information is obtained is highly relevant to migration outcomes, with a first-stage F-statistic of 15.9. The exclusion restriction concern about distance being a proxy for accumulated urban experience does not seem to matter because amongst the immigrants there is no relationship between New Zealand earnings and where they lived in Tonga. Another concern with this instrument is that the results may be driven by the large distances from the outer islands to the ballot office but the results are almost identical (first stage F-test of 15.8 and an immigration effect of $280 per week) if the sample is restricted to people from the main island of Tongatapu. Results are also the same if actual road network distance (on Tongatapu) is used instead of crow-fly distance (first stage F-test of 23.9 and immigration effect of $281 per week).

<table>
<thead>
<tr>
<th>Method:</th>
<th>Estimate</th>
<th>% difference from experimental estimate</th>
</tr>
</thead>
<tbody>
<tr>
<td>1) Single difference using pre-migration income</td>
<td>341.3</td>
<td>24.6</td>
</tr>
<tr>
<td>2) Selection on Observables: OLS regression</td>
<td>383.5</td>
<td>40.0 **</td>
</tr>
<tr>
<td>3) Difference-in-Difference Regression</td>
<td>375.2</td>
<td>36.9 **</td>
</tr>
<tr>
<td>4a) Propensity Score Matching(PSM): No past income</td>
<td>364.0</td>
<td>32.8 **</td>
</tr>
<tr>
<td>4b) Bias-adjusted PSM: No past income</td>
<td>346.3</td>
<td>26.4</td>
</tr>
<tr>
<td>4c) Propensity Score Matching using past income</td>
<td>352.2</td>
<td>28.5 *</td>
</tr>
<tr>
<td>4d) Bias-adjusted PSM: Using past income</td>
<td>333.4</td>
<td>21.7</td>
</tr>
<tr>
<td>5a) IV using migrant network in New Zealand</td>
<td>498.8</td>
<td>82.0</td>
</tr>
<tr>
<td>5b) IV using distance to ballot office</td>
<td>294.5</td>
<td>7.5</td>
</tr>
</tbody>
</table>

Note: Authors’ calculations from sample Group 1 and Group 4 of the Tongan component of PINZMS data collected in 2005 (n=118).
Significantly different from experimental estimate at 10% (*), 5% (**) or 1% (***)) levels.

In contrast to the good performance of the IV estimator when using the distance instrument there is very poor performance when using the network instrument. The estimated immigration effect of $499 per week overstates the experimental estimate by 82 percent. The family network in New Zealand is a relevant instrument, with a first-stage F-statistic of 14.2, but it seems that it fails the exclusion restrictions because the network is also a source of job offers and so directly affects immigrant incomes. In terms of implications for other studies, it suggests that with the IV estimator the devil is in the detail, in the sense that it can be a very good estimator for measuring the income gains from migration but with an invalid instrument it can be a very bad estimator.

\(^{13}\) The overstatement is even smaller, at two percent, if the non-experimental control group is comprised of all 18-45 year olds in the non-applicant households (treatment effect of $279) rather than just the pseudo-applicant.
The single-difference estimator, which relies on immigrants’ retrospectively recalling their pre-emigration earnings, overstates the gains by 25 percent. The difference-in-differences estimator compares this change in immigrants’ earnings with the similarly calculated change in non-applicants’ earnings and overstates the gains by 37 percent. When OLS is used, taking account of selection on the observable characteristics listed in Table 2, the immigration effect is estimated as $384 per week, which is a 40 percent overstatement.

Propensity-score matching, which uses the characteristics listed in Table 2 in a more flexible way to match immigrants to ‘similar’ non-immigrants, overstates the gains by 22-33 percent. The overstatement is less (and statistically insignificant) when past income is included in the matching variables. The overstatement is also less when the bias-adjusted method of Abadie and Imbens (2006) that matches directly on covariates is used.

Although these microeconomic non-experimental estimators overstate the income gains, they still do better than the macro estimators which underpin calculations of the sort undertaken by World Bank (2006). These macro estimators involve comparing per capita income or wages across countries. In 2004, New Zealand’s GDP per capita was NZ$30,469, while Tonga’s was NZ$2,044, which equates to a weekly per capita difference of NZ$546 per week, or twice as large as the actual gain, as measured by the experimental estimators. The difference in manufacturing wages is shown by McKenzie, Gibson and Stillman (2006) to be even larger, overstatement the experimental estimate by 133 percent.

IV. Other Effects on Immigrants: Expectations and Health

The above results suggest that increased people flows resulting from lower barriers to immigration may not bring quite such large global welfare benefits as are calculated from either macro approaches or micro approaches that do not fully account for immigrant selectivity. Nevertheless, immigration still appears to have substantial positive effects for the immigrants, with labour income increasing by over 250 percent in the current case. However, any broader welfare assessment should also take account of the claimed negative effects of immigration that are highlighted especially in the non-economic literature. Two of these negative effects concern immigrant health and unmet expectations about incomes abroad.

Immigrant Expectations

The tendency for the non-experimental data and estimators to overstate the income gains from migration is a problem for economists and econometricians, but not necessarily for the immigrants. It is doubtful that would-be emigrants use econometric models to forecast what their overseas earnings would be, were they to emigrate. Instead what matters for the immigrant is how their income realization compares with the expectations that they had prior to moving. In this regard there is certainly anecdotal evidence that would-be emigrants are exposed to information that could cause exaggerated expectations of the incomes they can earn abroad:

---

14 Full coverage of the results in this section is in McKenzie, Gibson and Stillman (2006a).
15 The issue of how expectations about the returns to human capital investments form and are used has been studied more often in the context of schooling decisions, where Manski (1993) shows that incorrect assumptions about whether students condition expectations on ability can lead to incorrect inferences about whether schooling choices imply that youth are uninterested in the returns to schooling.
“Fortunes are being made by taking the life savings off gullible people in return for getting them, illegally, into a country like Britain. The sales talk is doubtless about a land flowing with milk and honey, and streets paved with gold.”

*The Campaign for Political Ecology*¹⁶

“…for our relatives who live in the isle, in their small minds they think that money grow[s] out of trees, and thus expect people overseas to provide them with their need[s] also on top of all the financial obligations of those overseas…Tongans returning home for visits make the situation worse by exaggerating their success and wealth and creating unrealistic expectations”

*Tongan online discussion group* (quoted in Lee, 2003, p.36)

One way of testing whether expectations of immigrant incomes abroad are inflated would be a panel of immigrants with observations taken before and after their move. In the absence of such data, the randomization provided by the PAC ballot provides another method of estimating what the expectations of immigrants would have been prior to their moving. Specifically, expectations about employment and income in New Zealand are obtained from the sample of Tongans who applied to emigrate but whose names were not chosen in the ballot (Group 3). Randomization means that these expectations should be an unbiased estimate of the expectations that the actual immigrants held prior to emigrating.¹⁷ The subjective, conditional earnings expectations of ballot losers are then compared to the distribution of earnings realized by the ballot winners who emigrated. This comparison indicates whether immigrants are likely to have held overly optimistic expectations prior to moving to New Zealand.

The approach pioneered by Dominitz and Manski (1997) is used to measure expectations. After first explaining the concept of probabilities, expectations about employment in New Zealand were obtained by asking the following question:

“I would now like you to think about what you would be doing right now if you were living in New Zealand. What do you think is the percent chance that you would be working for pay?”

All individuals who expressed a percent chance greater than zero of working for pay were then asked what they thought were the lowest weekly amount and highest weekly amount that they could possibly be earning if they were working for pay in New Zealand right now. These questions serve to decrease overconfidence problems in which respondents tend to focus too much on central tendencies and not consider the uncertainty in potential outcomes (Dominitz and Manski, 1997). They also act to decrease anchoring problems whereby respondent’s beliefs are influenced by the amounts that the interviewer asks about.

The average of the answers to the highest and lowest weekly incomes were then used by the interviewer to read a set of threshold levels of income, $Y_1$, $Y_2$, $Y_3$, and $Y_4$, from a predetermined table on the questionnaire. Respondents were then asked:

¹⁶ http://eco.gn.apc.org/Population/immigration.html

¹⁷ There is a timing difference, with the unsuccessful applicants interviewed after some of the immigrants had left Tonga. However, there had been little experience of wage inflation in Tonga, with many public sector wages held constant in nominal terms from 1996 until late 2005, when substantial increases followed a six week strike.
“Thinking about the income that you would be earning if you were working in New Zealand right now, what do you think is the percent chance that your own weekly income from work would be less than Y1 New Zealand dollars?”

The same question was then asked for thresholds of Y2, Y3 and Y4 dollars. For example, an individual whose average of the highest and lowest weekly incomes was $375 would be asked what the percent chance was that their income would be less than $300, $350, $400 and $450. An advantage of this over traditional survey questions that use phrases like “very likely”, “not too likely” and so forth is that these verbal keys may not be inter-personally comparable, with different respondents interpreting the same verbal phrases in different ways (Manski, 2004).

The four responses about the percent chances of being below the income thresholds are divided by 100 and then interpreted as points on the subjective cumulative distribution function (CDF) of weekly labor income if they were working in New Zealand. Thus for each individual \( i \):

\[
F_{i,k} = P(y_i < Y_{i,k}|z=1, \varphi_i) \quad k=1,2,3,4
\]

is observed, where \( y_i \) denotes earnings in New Zealand, \( Y_{i,1}, Y_{i,2}, Y_{i,3} \) and \( Y_{i,4} \) are the earnings thresholds that \( i \) is asked about, \( \varphi_i \) is \( i \)'s information set, and \( z=1 \) denotes that the expectations are conditional on working in New Zealand. Let \( G(Y; \mu, \sigma^2) \) denote the CDF of a log-normal distribution, where \( \log Y \sim N(\mu, \sigma^2) \). For each respondent, the estimates \( \mu_i, \sigma_i^2 \) are found by solving the least squares problem:

\[
\min_{\mu, \sigma^2} \sum_{k=1}^{4} \left( F_{i,k} - G(Y_{i,k}; \mu_i, \sigma_i^2) \right)^2
\]

Once a distribution has been fitted for each respondent, moments and quantiles of interest (25th, 50th, and 75th percentiles) are obtained. The log-normal distribution fits the elicited points very closely with a mean absolute difference between the elicited and fitted distributions of 0.017, which is closer than the fit achieved by Dominitz and Manski (1997) with one year ahead labour income in the United States.

In contrast to the concern that immigrants might have held over-optimistic expectations, there is evidence that they underestimate both the odds of being employed in New Zealand and earnings if employed. The mean percent chance of being employed in New Zealand expressed by the ballot losers is 57 percent, compared to a 75 percent actual employment rate amongst immigrant ballot winners. The mean expected weekly earnings in New Zealand conditional on being employed is $337, which is much less than the actual mean earnings ($564) of the ballot winners who were employed in New Zealand.

The top panel of Table 4 reports the mean and quantiles for several points on the fitted distribution of subjective conditional earnings expectations. The underestimation occurs across the whole distribution, but appears proportionately larger amongst would-be emigrants at the bottom of the distribution. The 10th percentile of mean expected earnings is only 31 percent of the 10th percentile of actual earnings, while the 90th percentile of mean expected earnings is 88 percent of the 90th percentile of actual earnings. Even drawing from the 75th percentile of each respondent’s subjective conditional CDF gives expected earnings that are below actual earnings at all points except the 90th percentile (and the mean of the 75th percentile of expected earnings is only $394, which is just 70 percent of the actual mean earnings).
Table 4: Actual and Expected Labour Incomes

<table>
<thead>
<tr>
<th>A. Conditional on being employed</th>
<th>Percentiles of actual or expected distribution</th>
</tr>
</thead>
<tbody>
<tr>
<td>Actual Income:</td>
<td>Mean 10th 25th 50th 75th 90th</td>
</tr>
<tr>
<td>Actual labour income for employed immigrants in NZ</td>
<td>564 360 431 515 622 700</td>
</tr>
<tr>
<td>Expected Income:</td>
<td></td>
</tr>
<tr>
<td>Lowest amount expected by those in Tonga</td>
<td>212 100 100 200 300 300</td>
</tr>
<tr>
<td>Highest amount expected by those in Tonga</td>
<td>551 200 300 500 900 1000</td>
</tr>
<tr>
<td>Subjective, expected, conditional earnings distribution</td>
<td></td>
</tr>
<tr>
<td>Mean of expected earnings</td>
<td>337 111 172 298 501 615</td>
</tr>
<tr>
<td>25th percentile</td>
<td>238 49 94 196 341 495</td>
</tr>
<tr>
<td>50th percentile</td>
<td>303 85 132 265 479 576</td>
</tr>
<tr>
<td>75th percentile</td>
<td>394 135 211 364 578 713</td>
</tr>
</tbody>
</table>

B. Unconditional

Actual Income:

Actual labour income for all immigrants in NZ | 423 0 72 445 600 692 |

Subjective, expected, unconditional earnings distribution |                                               |
| Mean of expected earnings         | 196 58 103 186 263 344                      |
| 25th percentile                  | 140 19 52 134 188 301                       |
| 50th percentile                  | 178 33 81 174 243 333                       |
| 75th percentile                  | 230 64 123 219 313 384                      |

Note: Authors’ calculations from PINZMS data, where mean and percentiles of expected incomes are calculated from procedure described in the text. Actual income in panel A is based on 49 employed Principal Applicants in NZ and in panel B on all 65 Principal Applicants. Expected earnings are based on 77 Principal Applicants in Tonga who were unsuccessful in the PAC ballots.

The bottom panel of Table 4 reports the results of combining the expectations about employment with the conditional earnings distribution. The mean unconditional expected earnings are only $196 per week, compared with actual unconditional earnings of $423 per week. Thus the average would-be emigrant expected only 46 percent of the actual labour income of immigrants. To the extent that this is an unbiased measure of the expectations that the immigrants held prior to emigrating it does not seem likely that there is a widespread problem of immigrants suffering from unmet income expectations.

Mental Health Changes

While immigration raises immigrant’s incomes (and apparently by more than they had expected) it is also claimed to have a number of adverse health effects. These are interesting both because if true they could lower the net welfare benefit of increased people flows and also because they seem at variance with the widely studied positive gradient between income and health (Case, Lubotsky and Paxson, 2002). Mental health disorders may be one of the earliest detectable changes following immigration since elevated stress from the process of migration is one hypothesis for the higher incidence of mental health disorders amongst immigrants (Cochrane and Bal, 1987). While there is debate about which source of this elevated risk is most important (Bhugra, 2004), many health researchers argue that migration itself is the prime cause:

“the adverse effects on mental health of migration have been widely researched and established”. Pernice, Trlin, Henderson and North (2000), p. 24

---

18 Full coverage of the results in this section is in Stillman, Gibson and McKenzie (2006).
19 This higher incidence of mental disorder amongst immigrants has been documented at least since the study of schizophrenia in Norwegian immigrants in the United States by Ödegaard (1932) and is argued to also exist amongst Pacific Island immigrants in New Zealand (Pernice and Brook, 1994).
To assess these claims, Tongan adult immigrants in New Zealand and the Tongan non-immigrant ballot applicants (Groups 2 and 3) were given a standard mental health questionnaire: the Mental Health Inventory 5 (MHI-5) of Veit and Ware (1983). This is a five item scale with a maximum score of 25 (indicating psychological wellbeing) and minimum score of 5. The MHI-5 measure was developed for use in the general population and has been used as part of general surveys of health and quality of life in addition to specific studies on mental health. The measure compares well to other more detailed measures (Berwick et al., 1991).

In contrast to some results in the medical literature, there is no apparent evidence of immigration impairing mental health in the current setting. The first two columns of Table 5 report the intention-to-treat (ITT) effect and the instrumental variables estimate of the average treatment effect on the treated (IV-TT). Adjusting the ITT for non-compliance (using equation (8)) gives an estimate of the treatment effect of $0.443/0.33=1.34$. Similarly, the IV-TT estimator, which uses the ballot outcome as an instrument for immigration, indicates a 1.31 point increase in mental health from immigrating, although this is still outside usual levels of statistical significance.

<table>
<thead>
<tr>
<th>Table 5: Regression-based Treatment Effect Estimates of the Effect of Immigration on Mental Health</th>
</tr>
</thead>
<tbody>
<tr>
<td>ITT</td>
</tr>
<tr>
<td>Ballot Success Dummy</td>
</tr>
<tr>
<td>Migration Dummy</td>
</tr>
<tr>
<td>Controls</td>
</tr>
<tr>
<td>First stage F-test on instrument</td>
</tr>
</tbody>
</table>

Note: Authors’ calculations from PINZMS data, where the dependent variable is the mental health score on a 5-25 scale (25=best) for 364 observations (171 male, 193 female). Robust standard errors in () are clustered at household level, * significant at 10% level. The control variables are past income, religious denomination, height, birthplace, age, education, marital status and gender. The instrumental variable for migration is the ballot outcome.

The remaining columns of Table 5 report results that look for differences in treatment effects amongst the immigrant population. The first disaggregation finds that the estimated impact of immigration is twice as large for females as for males (IV-TT=1.8 for females compared to 0.9 for males). Moreover, the effect for females is statistically significant at $p=0.08$ or higher. While the IV-TT considers shifts in the mean of the mental health scores, Figure 4 suggests the presence of non-linear effects. Specifically, the mental health of non-immigrant females is distributed fairly normally but the mental health of female immigrants is strongly modal and skewed towards the upper part of the mental health distribution. A somewhat similar pattern occurs for the male immigrants.
To capture this non-linearity the final columns of Table 5 report the results of using the methodology of Chernozhukov and Hansen (2005) for estimating instrumental variables quantile treatment effects. Specifically the impact of migration on mental health is estimated at the 25th, 50th, and 75th percentile of the mental health distribution. The key assumption behind these results is that individual unobservables are rank invariant to the potential treatment status, e.g. individuals who have better (worse) mental health than certain individuals without migrating will also have better (worse) mental health than these individuals if they all migrate. The treatment effects appear to be larger at the lower part of the mental health distribution with immigration increasing mental health by 2.81 points for individuals at the 25th percentile of the mental health distribution, but only by 1.05-1.10 points for individuals higher in the distribution.20

Thus in contrast to some of the concerns coming from the medical literature, there do not appear to be negative effects of immigration on mental health in the current setting. In fact the effects are apparently positive for women and for individuals with low mental health. Thus, rather than there being a mental health cost offsetting some of the economic gains from immigration in an overall welfare assessment, these results suggest that immigration may bring psychological benefits beyond the economic gains.

Child Health Changes
Another health indicator that may respond to migration is the physical stature of children. Anthropometric status is generally known to be a sensitive indicator to the quality of economic and social environments (Steckel, 1995), while child weight for height has been demonstrated to

20 The statistical significance of the treatment effect at the 25th percentile is apparent using the derived standard errors from the bootstrapped coefficients but significance falls to an 80 percent level when using the non-symmetric bootstrapped confidence interval.
be a good measure for identifying short-run effects on health (Strauss and Thomas, 1998). It is typically thought that child height-for-age adjusts more slowly to changes in environment but given the large income gains and other substantial changes in diets and health care, there may also be identifiable effects of emigration to New Zealand on the height of children.

In other contexts immigration is associated with a diverse but largely negative set of effects on child anthropometric status. For example, children in Maya immigrant families in the U.S. are approximately 10 centimeters taller than Maya children of the same age in Guatemala but are also significantly more likely to be obese (Smith, Bogin, Varela-Silva and Loucky, 2003). High rates of obesity in immigrant children are also observed in Europe (Kirchengast and Schober, 2006). On the other hand, some studies also show micronutrient deficiencies and malnutrition amongst immigrant children (Meulmeister, Berg and Wedel, 1990). Changes in diet and particularly in the likelihood of continued breastfeeding are likely to be important short-term factors explaining these nutritional changes (Carballo, Divino and Zeric, 1998).

To examine these effects, the children of the Tongan and Fijian immigrant families in New Zealand and the children of the non-immigrant PAC ballot entrants (i.e., Groups 2 and 3) in the Pacific Islands were weighed and measured. Both height for age and weight for height were calculated, and in each case are expressed as z-scores showing how many standard deviations each child is away from the age- and gender-specific median height or weight in the reference populations. The results are reported in Table 6 for three separate age groups: 0-2 years, 3-6 years and 7-12 years because there is evidence that the effects of income on anthropometric status vary with the age of the child (Sahn and Alderman, 1997).

Table 6: Regression-based Treatment Effect Estimates of the Effect of Immigration on Child Health

<table>
<thead>
<tr>
<th></th>
<th>Height for Age Z-scores</th>
<th>Weight for Height Z-scores</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0 - 2 years</td>
<td>3 - 6 years</td>
</tr>
<tr>
<td>Immigrant (=1 if in NZ, 0 otherwise)</td>
<td>3.118**</td>
<td>-0.228</td>
</tr>
<tr>
<td></td>
<td>(1.489)</td>
<td>(0.616)</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.196</td>
<td>0.017</td>
</tr>
<tr>
<td>First stage F-test on instrument</td>
<td>38.62***</td>
<td>100.09***</td>
</tr>
<tr>
<td>Number of observations</td>
<td>43</td>
<td>101</td>
</tr>
</tbody>
</table>

Note: Authors’ calculations from PINZMS data, where the dependent variables are the standardized height for age relative to growth standards in the United Kingdom, and the standardized weight for height relative to growth standards in the United States. Robust standard errors in ( ) are clustered at household level, * = significant at 10% level, ** = 5% level and *** = 1% level. The control variables are a quadratic in age (in months), gender, and whether the child is from Tonga or Fiji. The instrumental variable for immigration is the ballot outcome.

There appears to be a substantial, positive, effect of immigration to New Zealand on the anthropometric status of young Pacific Island children. Specifically the 0-2 year old children of immigrants are 3.12 standard deviations longer than children of the same age and gender in their home country (the effect is statistically significant at \( p<0.04 \) level). This estimate comes from the

---

21 The reference standards for the United Kingdom are used for height for age because they cover the 0-2 age group, which the standards from the United States do not. The reference standards for weight for height are for the United States and are only available for heights up to 121.5cm. This restricts coverage to only 14% of children in the 7-12 age group, so results for that age group are not shown.
instrumental variables estimator of the average treatment effect on the treated (IV-TT), where the ballot outcome for the child’s parent who was the Principal Applicant in the PAC is used as the instrument. The results also show an effect of lower weight-for-height in the same age group but this is not statistically significant.

Amongst older children there are no significant effects of immigration on height for age, which supports the notion that it is the stature of the youngest children that is the most sensitive to the quality of the economic and social environment that their families face. The older children do exhibit some increase in weight for height, although this is not statistically significant.22 Thus, at least in the short run, for PAC immigrants in New Zealand there does not appear to be the same negative effect on child health that has been noted amongst some immigrants in other countries.

**Adult Blood Pressure**

Although adult anthropometric indicators are unlikely to change as quickly as those for young children, since adults are already fully grown, the literature does suggest scope for rapid changes in blood pressure following migration. For example, migrants in Nairobi who had come from a rural area of Western Kenya had significantly higher blood pressure than their rural counterparts within one month after urban migration (Poulter et al., 1990). The switch to urban diets with higher sodium content is one likely cause of this rapid elevation in blood pressure. Moreover, longitudinal studies of Tokelauan immigrants in New Zealand who were observed over 14 years show that the rise in blood pressure around the time of migration is subsequently maintained (Salmond, Prior and Wessen, 1989). These changes in blood pressure matter because, globally, hypertension has been identified as the leading risk factor for mortality and is ranked third as a cause of disability-adjusted life-years (Kearney et al., 2005).

Usually there are concerns that immigrant selectivity interferes with interpretation of the ‘healthy immigrant effect’ of immigrants arriving healthier than the host country population but then become less healthy (Antecol and Bedard, 2006). However, the studies of blood pressure described above had measurements taken on the same individuals before and after immigration, which should allow selectivity to be controlled for. The randomization provided by the PAC ballots provides another method of dealing with immigrant selectivity. Specifically, blood pressure readings were taken from adults in the Tongan and Fijian immigrant families in New Zealand and in the families of the non-immigrant PAC ballot entrants (i.e., Groups 2 and 3) who remained in Tonga and Fiji.

No statistically significant differences in either systolic or diastolic blood pressure were observed between the immigrant and non-immigrant groups, for both males and females. Male immigrants have larger increases in systolic blood pressure than in diastolic blood pressure, which is consistent with the pattern from the Tokelau Migration Study (Salmond et al, 1989) while the opposite occurs for females. These estimates in Table 7 come from IV-TT estimator, where the ballot outcome is used as the instrument for immigration. In addition there are also results for instrumental variables probit models for the probability of the individuals having high blood pressure, which is defined as systolic blood pressure above 140 mmHg and diastolic blood

---

22 If the controls for age and gender are removed from the treatment effects regression then the effect of immigration on weight for height becomes statistically significant.
pressure above 90 mmHg. The probability of having high blood pressure falls for immigrant males and rises for immigrant females, although both effects are statistically insignificant.

Table 7: Regression-based Treatment Effect Estimates of the Effect of Immigration on Adult Blood Pressure

<table>
<thead>
<tr>
<th></th>
<th>Males</th>
<th>Females</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Systolic</td>
<td>Diastolic</td>
</tr>
<tr>
<td>Immigrant (=1 if in NZ, 0 otherwise)</td>
<td>6.921 (4.953)</td>
<td>1.190 (4.655)</td>
</tr>
<tr>
<td></td>
<td>[-0.038]</td>
<td>[0.174]</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes Yes Yes Yes</td>
<td>Yes Yes Yes Yes</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.086</td>
<td>0.027</td>
</tr>
<tr>
<td>First stage F-test on instrument</td>
<td>141.51***</td>
<td>141.51***</td>
</tr>
<tr>
<td>Number of observations</td>
<td>269</td>
<td>269 269</td>
</tr>
</tbody>
</table>

Note: Authors' calculations from PINZMS data. Robust standard errors in ( ) are clustered at household level. * = significant at 10% level, ** = 5% level and *** = 1% level. The control variables are a quadratic in age and whether the person is from Tonga or Fiji. The instrumental variable for immigration is the ballot outcome.

In summary, this examination of immigrant health and expectations about incomes abroad does not suggest that there are substantial psychological and health costs for immigrants that offset some of the economic gains. However, two caveats to this conclusion are that these immigrants are observed less than three years after moving, so short term outcomes may reverse in the longer run, and the Pacific Islands may be unusual amongst immigrant-source countries in having high prevalence rates of some non-communicable diseases (Coyne, 2000).

V. Conclusions

Measuring the value of increased international migration as a development policy requires estimating what workers in developing countries could earn in rich countries. The earnings of existing immigrants may be a poor measure of what a randomly selected worker from a developing country would earn in a rich country. Existing immigrants are likely to have different abilities, skills and motivations than non-migrants in their home countries. The results reported here suggest that popular approaches for dealing with this positive selection on unobservable attributes overstate the gains from migration, compared with the benchmark of an experimental estimate. Indeed, if the results found here apply to other migrant groups, assessments of global benefits from increasing people flows may only be half as large as cross-country wage differentials suggest and three-quarters as large as would be calculated from microeconometric methods applied to non-experimental samples of immigrants and non-immigrants.

Results in the non-economic literature imply that even these attenuated gains from increased international migration may be lower in overall welfare terms if immigrants suffer from unmet expectations and the adverse effects of immigration on health. However, these adverse consequences are not (yet) apparent for the Pacific Island immigrants studied here and there even appear to be some health improvements. Moreover, a more complete evaluation of the global welfare effects of increased people flows must also consider the impact of emigration and remittances on the population of the source countries. This is a particularly salient issue for Australia, New Zealand and our Pacific neighbours and is a topic that future research based on the data described in this lecture can hopefully contribute to.
References
Carballo, Manuel, Jose Divino and Damir Zeric (1998) “Migration and Health in the European Union”, *Tropical Medicine & International Health* 3(12): 936-944.


