THE DEVELOPMENT IMPACT OF A BEST PRACTICE SEASONAL WORKER POLICY

John Gibson and David McKenzie*

Abstract—Seasonal migration programs are widely used around the world, yet there is little evidence as to their development impacts. A multiyear prospective evaluation of New Zealand’s Recognised Seasonal Employer (RSE) seasonal worker program allows us to measure the impact of participating in this program on households in Tonga and Vanuatu. Using a propensity-score prescreened difference-in-differences analysis based on surveys fielded before, during, and after participation, we find that the RSE has had positive development impacts that dwarf those of other popular development interventions. It has increased income, consumption, and savings of households; durable goods ownership; and subjective standard of living. The results also suggest that child schooling improved in Tonga.

First and foremost it will help alleviate poverty directly by providing jobs for rural and outer island workers who often lack income-generating work. The earnings they send home will support families, help pay for education and health, and sometimes provide capital for those wanting to start a small business.

Winston Peters, New Zealand minister of foreign affairs, on the approval of the RSE program, October 2006

A guest worker program is the most effective contribution we can make to improving the lives of the world’s working poor.

Dani Rodrik, New York Times, June 1, 2007

I. Introduction

INTERNATIONAL migration is probably the most effective mechanism we know to rapidly increase the incomes of poor people (Clemens, Montenegro, & Pritchett, 2008), and to help narrow global income gaps (Hanson, 2009). It is also one of the most controversial, with migrant-receiving countries worried about the costs of assimilating workers and their families. Temporary or circular migration programs are seen as a way of overcoming such concerns and enabling poorer, less skilled workers to benefit from the higher incomes to be earned abroad as part of a “triple-win,” whereby migrants, the sending country, and the receiving country all benefit. Such programs have been recommended as one of the most promising ways to enhance the development benefits of migration by a wide range of international organizations (United Nations, 2004; Global Commission on International Migration, 2005; World Bank 2006), national governments (House of Commons International Development Committee, 2004), and academics (Winters et al., 2003; Pritchett, 2006; Rodrik, 2007).

Almost all OECD countries have temporary worker migration programs, with seasonal workers the largest category, totaling 576,000 workers in 2006 (OECD, 2008). Such programs remain controversial, especially when geared to low-skilled migrants. Some critics of such programs raise concerns that workers will overstay and they will compete down the wages of native poorer workers (e.g. Borjas, 2007), while others raise concerns about the possible exploitation of migrants and whether they can earn enough to make it worthwhile if the duration of work is short.

Lacking in this debate is credible evidence on the development impact of international seasonal worker programs. The few existing studies are based on ex post surveys of migrants and lack credible counterfactuals for what would have happened to households in the absence of migration. This paper seeks to provide credible evidence on the development impacts of seasonal migration by means of a prospective multiyear evaluation of New Zealand’s Recognised Seasonal Employer (RSE) program. The RSE began in 2007 and aims to ease labor shortages in New Zealand’s horticulture and viticulture industries, while also promoting economic development in the Pacific Islands. The policy was developed taking account of lessons from previous seasonal worker programs elsewhere and is viewed as a possible model for other countries. For example, the ILO good practices database states, “The comprehensive approach of the RSE scheme towards filling labour shortages in the horticulture and viticulture industries in New Zealand and the system of checks to ensure that the migration process is orderly, fair, and circular could service as a model for other destination countries.”

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A supplemental appendix is available online at http://www.mitpressjournals.org/doi/suppl/10.1162/REST_a_00383.

1 Quoted in “Seasonal Work Policy Benefits Pacific Says Peters,” Islands Business, October 26, 2006, http://www.islandsbusiness.com/news/index_dynamic/containerNameToReplace=MiddleMiddle/ focusModuleId=130/focusContentId=6691/tableName=mediaRelease/overrideSkinName=newsArticle-full.tpl.

3 For example, Basok (2000) conducted a snowball sample of Mexican workers in Canada’s seasonal worker program in one area of Canada and in one village in Mexico. Macours and Vakis (2010) use a cross-sectional survey of Nicaraguan households near the border with Honduras, where workers migrate seasonally.

2 These concerns are discussed in Ruhs (2006), Pritchett (2006), and OECD (2008), among others.

1 Quoted in “Seasonal Work Policy Benefits Pacific Says Peters,” Islands Business, October 26, 2006, http://www.islandsbusiness.com/news/index_dynamic/containerNameToReplace=MiddleMiddle/focusModuleId=130/focusContentId=6691/tableName=mediaRelease/overrideSkinName=newsArticle-full.tpl.

Our evaluation was designed prospectively, alongside the program launch. We conducted baseline surveys of households and communities in Tonga and Vanuatu before workers left to work in New Zealand and then reinterviewed these same households 6, 12, and 24 months later. These rich baseline data and institutional knowledge of how recruitment occurred let us use propensity score matching to identify an appropriate set of households to act as a comparison group for the households in the RSE. Following Crump et al. (2009) we use this propensity score to pre-screen the sample used for panel difference-in-differences and fixed effects estimation. With these methods, we assess the impacts of the RSE on household incomes, consumption, savings, durable assets, and subjective well-being and also measure broader community-level impacts.

The results show large positive effects on sending households in Tonga and Vanuatu; per capita income of participating households rises by over 30% relative to the comparison groups in both countries, while per capita expenditure and savings also rise. Subjective economic welfare increases by almost half a standard deviation for participants in both countries, who are also seen to have purchased more durable assets. In Tonga, RSE households also doubled the rate of home improvement, and in both countries, households became more likely to have a bank account, likely reflecting more formal savings. In addition, there is some evidence that school attendance rates for 16- to 18-year-olds increased in Tonga. An additional innovative feature of our analysis was to survey community leaders, who reported positive broader impacts on the community.

Overall these results show that the seasonal worker program has been a powerful development intervention for the participating households, with aggregate effects that are important relative to aid flows and export earnings. Thus, the RSE policy appears to have succeeded in its development objectives in the short run. There are very few rigorous impact evaluations that show large gains in income from development interventions, and the development impacts of this seasonal worker program on participating households dwarf those found in recent evaluations of other popular development interventions like conditional cash transfers, microfinance, business training, and grants to microenterprises.

A common issue for any in-depth evaluation of a single intervention is the extent to which the results may generalize to other settings. We note first that seasonal worker programs are an important policy lacking rigorous evidence from any setting. Second, we examine impacts in two countries that differ in many dimensions: Tonga and Vanuatu have different ethnic populations (one Polynesian, one Melanesian), different government structures, and different prior histories of migration. Finding similar results in both countries therefore suggests the results are not particular to a single context. Third, small countries are indeed one of the most relevant contexts to look at such programs: emigration rates are highest for small island countries, where the lack of market size and remoteness makes other development projects less attractive. Our results are most directly applicable to small countries, such as the 45 developing countries with populations below 1.5 million. But the findings may have even broader relevance since New Zealand’s program is sizable by international standards (greatly exceeding Australia’s fledgling seasonal worker scheme and about one-third the size of Canada’s SWAP program, for example) and is touted as a model for many destination countries.

The remainder of the paper is structured as follows. Section II describes the RSE policy and worker recruitment. Section III defines how we see development impact, and section IV describes our surveys and estimation methodology. Household-level impacts are estimated in section V, and impacts at the community and macro level are discussed in section VI. Section VII concludes.

II. The RSE Program

The RSE was launched on April 30, 2007, and initially let up to 5,000 seasonal workers come to New Zealand for a maximum of seven months per eleven-month period to work in horticulture and viticulture. Preference is given to workers from Pacific countries (except Fiji), with Kiribati, Samoa, Tonga, Tuvalu, and Vanuatu given special “kick-start” status that entailed deliberate and expedited efforts to launch the program and recruit in these countries. Vanuatu and Tonga, the focus of our impact analysis, supplied the most workers under the RSE in the first two seasons: 3,590 workers in the case of Vanuatu and 1,971 from Tonga (including return workers).5

Ramasamy et al. (2008) detail the origins of the RSE as a solution to the long-standing problems the horticulture and viticulture industries had in meeting seasonal labor needs, while contributing to New Zealand’s broad development goals in the Pacific region. Design of the RSE paid careful attention to previous experience with seasonal worker programs around the world to reduce risks of overstaying, displacement of New Zealand workers, and worker exploitation. The risk of overstaying is mitigated in a number of ways: workers may be reemployed in subsequent years, with the same or a new employer, which can be contrasted with single-entry programs that provide high incentives for workers to overstay; employers are required to pay the costs associated with worker removal from New Zealand if workers become illegal, giving employers incentives not to be complicit in their overstaying; and competition for places among communities and countries leads to social pressures not to jeopardize future possibilities for others by overstaying and thereby creating a negative reputation for one’s community.

The risk of displacement of New Zealand workers is mitigated through a New Zealanders first principle that

5 A return worker is an individual who participates in the RSE in one season and then returns again in a second season; he or she is counted as two workers in the administrative data.
requires employers to first lodge their vacancies with the Ministry of Social Development (which provides welfare benefits and job search services) before attempting to recruit offshore. The risk of exploitation is mitigated through regulations stating that workers must not be charged recruitment fees and that employers must pay market wages and offer workers at least a minimum remuneration, which depends on the length of the contract. Employers also must arrange suitable accommodation, internal transportation, access to personal banking services, provision of protective equipment, and opportunities for recreation and religious observance.

Recruitment options were fit to the needs of each country. In Tonga employers could recruit workers directly or from a work-ready pool of Tongans prescreened and selected by the Labour Ministry. In the first year, recruitment was mainly from the work-ready pool. Preselection and screening was by district and town officers and church and community leaders. There was tremendous interest in the program, evident from the fact that more than 5,000 Tongans registered for the work-ready pool within three months of the launch of the program. We describe this selection process (Gibson, McKenzie, & Rohorua, 2008) and show the main attributes used by village committees in preselection, which favored low-income applicants. The Tongan Labour Ministry ensured that all villages in the country had workers in the scheme.

In Vanuatu employers could hire directly or through an agent. Direct recruitment is facilitated by the Vanuatu Department of Labour, which in the first year also used a work-ready pool of workers from walk-ins who registered directly with the department. These workers were typically from the more urban areas. In rural areas, direct recruitment and agents relied heavily on community contacts through village councils, again using villages to prescreen workers. In McKenzie, Martinez, and Winters (2008), we study this process, and find that agents and villages looked for similar characteristics to those selected on in Tonga except that it was not the poorest households that applied and had workers selected. This perhaps reflected the newness of international migration in Vanuatu, with communities more concerned with sending workers who would represent the village; in addition, the poorest households lacked information about the program in the first year and lacked the resources to finance the costs of the travel process.

Typical work under the RSE includes working in vineyards to prune vines and pick grapes, harvesting apples and kiwifruit and other fruit picking, and working in the packhouse to sort, grade, and pack the fruit. The work was typically physically demanding and included work in both cold and hot conditions. In part due to the nature of the work, the majority of RSE workers recruited were male: in the first year in our sample, 82% of the ni-Vanuatu RSE workers and 87% of the Tongan RSE workers were male.6

The RSE has been viewed as a success from New Zealand’s point of view. An evaluation of the first two years conducted by the New Zealand Department of Labour (2010, p. xvii) concluded, “Overall, the RSE Policy has achieved what it set out to do.” The policy is found to have provided employers in the horticulture and viticulture industries with access to a reliable and stable workforce, with productivity gains starting to emerge as workers return for repeat seasons. The main concerns raised about temporary labor programs have been mitigated: the evaluation finds little displacement of New Zealand workers, overstay rates were only 1% in the first season and less than 1% in the second, and concerns about worker exploitation have not arisen in a couple of isolated cases. The question this paper addresses is then whether the RSE has also lived up to the policy goal of improving development in the Pacific.

III. What Do We Mean by Development Impact?

In order to measure whether the RSE has improved development outcomes in the Pacific Islands, we must first define what we mean by development impact for such a program. We see three related definitions of development impact and attempt to provide evidence on each. The first, and narrowest, views development as anything that raises the income of people from poor countries. The large wage differences between a developed and a developing country would strongly suggest we should expect to see an impact on household income from participating, and so the main question is then measuring how large this impact is. However, there is a view that guest-worker programs in agriculture are “close to slavery,” with workers being exploited and not paid the promised wages, so that after meeting expenses, they are no better off (Southern Poverty Law Center, 2007). Our analysis will help rule out this possibility.

A second, broader view sees development as increasing the utility of households in developing countries. The key issue in this view is whether the gain in income is more than offset by any negative issues to the household that arise from physical separation of household members. A related literature on the impacts of separation in military families has applied two approaches to addressing this issue. The first is a revealed preference approach, which examines the trade-offs soldiers make between time and money (Dunn, 2003), noting that to the extent the decision is a choice, households must have decided that expected disutility is more than offset by the additional income. This approach may be less useful for a new program in which information may be incomplete. A second approach attempts to measure impacts on specific outcomes without trying to aggregate up to an overall impact.7

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6 Ni-Vanuatu is the term used to refer to citizens of Vanuatu.
7 For example, Angrist and Johnson (2000) find that deployment in the First Gulf War led to no changes in divorce rates or child disability when men were deployed, but increases in divorce when women were deployed. Lyle (2006) finds that military deployments to Iraq and Afghanistan have a negative impact on children’s test scores, with the impact greater when women leave.
In our context, we look at a number of specific outcomes, as well as subjective well-being as a measure of overall utility.

A final view of development is more concerned about whether interventions have long-term impacts on households. The concern here is that either income earned abroad does not make it back to the household in the first place because it is consumed by the migrant, or that the money that is received is spent on things that increase short-term household utility but leave the household no better off in the long term. The question is, then, whether money received by the family is spent on durable asset accumulation, saved, or invested in children’s education and in businesses, as well as whether there are spillover benefits for the broader community. We examine these outcomes, although we also note that this view is based on the presumption that participating in seasonal migration for many years is not a possibility, whereas in practice, households could increase long-term well-being by participating in this program for many years.

IV. Our Surveys and Estimation Methodology

A. The Surveys

There was keen interest from national governments on both sides of the migration relationship and from the World Bank in learning whether the new RSE program would have the development impacts envisioned as one of its core rationales. It was therefore decided ex ante to have a rigorous evaluation of development impacts, focusing on Tonga and Vanuatu. These were expected to be the two countries participating most, and they offer an interesting contrast in prior migration history. Tonga (population 100,000) has traditionally had high emigration rates to New Zealand, Australia, and the United States, with most recent migration through family-sponsored categories and a special annual permanent migration quota to New Zealand, the Pacific Access Category. The 2006 New Zealand Census enumerated 20,520 Tongan-born in New Zealand. In contrast, Vanuatu (population 215,000) has had relatively little international emigration, with only 1.5% of its population abroad prior to the RSE (World Bank, 2008) and fewer than 1,000 Vanuatu-born in the 2006 New Zealand Census.

Given that recruiting occurred at the employer level, the interests of some employers in screening workers themselves, and the large number of employers involved, it was never going to be feasible to attempt to get employers to randomly select workers. Therefore, we decided the most credible impact evaluation strategy would be a matched difference-in-differences approach. This would entail a baseline survey of households that would participate in the RSE before the workers left, along with surveys of nonparticipating households, and then following these households over time. Nonparticipating households would be separated into whether they had a member of the work-ready pool who had applied for the program but not been selected.

Ex ante it was not known how many individuals from each country would participate in the scheme, but the numbers likely to be involved were certainly too small for a simple random sample of households to pick up enough RSE households in a cost-effective way. Hence, we needed to know RSE status before surveying. Survey design was then complicated by the fact that approvals to recruit workers and recruitment took place on a rolling basis. Once workers were selected for recruitment, they often had only two or three weeks before they departed for New Zealand, leaving a very short window of time to interview them and their household.

Therefore, we used a rolling sampling methodology, adding sample as we received updates of when, where, and who employers were recruiting, with the baseline survey conducted between October 2007 and April 2008. In Tonga, our survey has nearly national coverage, covering the islands of Tongatapu, Vava’u, and ’Eua (containing 90% of the population and 92% of year 1 RSE workers). Officials helped us identify households with RSE workers and households with members of the RSE work-ready pool who had not been selected yet. We also surveyed randomly selected households in the same villages who had no one yet apply for the program. In each village, we aimed for approximately five households with an RSE worker, three households with a member of the work-ready pool who was not selected, and four households with nonapplicants. The choice of a control sample from the same villages as the treated was informed by our previous experience matching for a different migration program in Tonga (McKenzie, Gibson, & Stillman, 2010) and work on evaluation of labor training programs, which stresses the importance of matching on the same or similar labor markets (Dehejia, 2005). Our resulting baseline survey covered 448 households containing 2,335 individuals in 46 villages.

Vanuatu’s rugged geography and high transportation costs made it infeasible to survey in all islands, so we limited the evaluation to three islands from which we believed there was a high chance of workers coming: Efate (population 50,000), containing the capital city, Port Vila; Ambrym (population 10,000); and Tanna (population 20,000). In contrast to Tonga, not all villages in Vanuatu initially participated in the RSE, so as well as sampling nonapplicant households in villages with participating RSE workers, we also sampled households from nearby villages that had not participated in the RSE. Ultimately our baseline survey covered 456 households containing 2,173 individuals in 48 villages or communities.

Three rounds of follow-up surveys were then conducted. The first took place between April and July 2008, approximately six months after the baseline survey. This was intended to be a time when RSE workers were still in the midst of their seven-month stint abroad. However, as in practice many contracts were for shorter than seven months,
approximately two-thirds of Tongan RSE workers and one-fifth of ni-Vanuatu RSE workers in our sample had returned by the time of this survey. The second follow-up survey took place between October 2008 and February 2009, approximately one year after the baseline, while the third and final follow-up survey took place between October 2009 and March 2010, two years after baseline.

Attrition was remarkably low in the Tongan sample. Of the 456 households in the baseline, we were able to reinterview 442 households in the second round survey, 444 in the third round, and 440 in the fourth round. Attrition was higher in Vanuatu. Of the 456 households in the baseline survey, 382, 388, and 348 households were reinterviewed in rounds 2, 3, and 4, respectively, while 33 households were interviewed only in round 1. In an online appendix, we show our main results are robust to this attrition.

### B. Estimation Methodology

Table 1 in the online appendix has the number of households participating in the RSE by survey round. Most new entries into the RSE in our sample came between the first and second rounds, with few additional households from our sample joining the RSE in the later rounds. Most RSE households (58% Tonga and 54% Vanuatu) had only one seasonal worker spell during the two years of our study, while the rest had multiple spells. Since the number of times a household participates in the RSE conditional on participating is potentially the result of household choices, we focus on a binary measure of RSE participation: \( RSE_{i,t} \), which takes a value of 1 if household \( i \) has at least one member who has worked in the RSE by time \( t \), where \( t = 1,2,3 \) and 4 corresponds to our four survey waves. Estimating the impact of \( RSE_{i,t} \) then involves estimating the average impact of ever participating in the RSE over the first two years of the program.

We then begin with panel data regressions of the impact of the RSE, using the full sample of households separately for each country. Letting \( Y_{i,t} \) be an outcome of interest for household \( i \) in survey round \( t \), we begin with the following difference-in-differences specification,

\[
Y_{i,t} = \alpha + \beta \text{EverRSE}_i + \sum_{t=2}^{4} \delta_t + \gamma RSE_{i,t} + \epsilon_{i,t}, \tag{1}
\]

where \( \text{EverRSE}_i \) indicates whether household \( i \) ever participates in the RSE over the four waves of our sample and \( \delta_t \) are survey round dummies. The coefficient of interest is then \( \gamma \), which gives the average treatment effect of participating in the RSE. We do not include additional time-varying controls in this regression, since we have few time-varying variables that are not potentially themselves affected by the RSE. Standard errors are clustered at the

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**TABLE 1.—MEANS OF BASELINE CHARACTERISTICS OF HOUSEHOLDS FOR TONGAN SAMPLE**

<table>
<thead>
<tr>
<th></th>
<th>Full Sample</th>
<th>PS-1 in [0.1, 0.9]</th>
<th>PS-2 in [0.1, 0.9]</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>RSE</td>
<td>Non-RSE</td>
<td>RSE</td>
</tr>
<tr>
<td>Household size</td>
<td>5.70</td>
<td>4.82***</td>
<td>5.61</td>
</tr>
<tr>
<td>Number of males 18 to 50</td>
<td>1.50</td>
<td>1.25***</td>
<td>1.50</td>
</tr>
<tr>
<td>Share of males 18 to 50 who: Are literate in English</td>
<td>0.92</td>
<td>0.85**</td>
<td>0.91</td>
</tr>
<tr>
<td></td>
<td>0.46</td>
<td>0.47</td>
<td>0.47</td>
</tr>
<tr>
<td></td>
<td>0.68</td>
<td>0.60*</td>
<td>0.68</td>
</tr>
<tr>
<td></td>
<td>0.42</td>
<td>0.39</td>
<td>0.41</td>
</tr>
<tr>
<td></td>
<td>4.56</td>
<td>3.97***</td>
<td>4.53</td>
</tr>
<tr>
<td>Share of adults who previously have worked or studied in New Zealand</td>
<td>0.38</td>
<td>0.20***</td>
<td>0.39</td>
</tr>
<tr>
<td>Number of relatives in New Zealand</td>
<td>5.41</td>
<td>4.80*</td>
<td>5.33</td>
</tr>
<tr>
<td>Household durable assets index</td>
<td>0.07</td>
<td>-0.06</td>
<td>0.05</td>
</tr>
<tr>
<td>Number of pigs</td>
<td>5.57</td>
<td>5.49</td>
<td>5.52</td>
</tr>
<tr>
<td>Number of chickens</td>
<td>5.11</td>
<td>5.12</td>
<td>5.05</td>
</tr>
<tr>
<td>Number of cattle</td>
<td>0.45</td>
<td>0.47</td>
<td>0.48</td>
</tr>
<tr>
<td>Have a traditional-style dwelling</td>
<td>0.15</td>
<td>0.13</td>
<td>0.13</td>
</tr>
<tr>
<td>Located on Tongatapu or Efate</td>
<td>0.81</td>
<td>0.80</td>
<td>0.82</td>
</tr>
<tr>
<td>Semiannual per capita income (pa'anga)</td>
<td>979</td>
<td>1,342***</td>
<td>991</td>
</tr>
<tr>
<td>Semiannual per capita consumption (pa'anga)</td>
<td>829</td>
<td>1,184***</td>
<td>831</td>
</tr>
<tr>
<td>Proportion with income per capita below $US1 per day</td>
<td>0.19</td>
<td>0.12**</td>
<td>0.19</td>
</tr>
<tr>
<td>Proportion with income per capita below $US2 per day</td>
<td>0.49</td>
<td>0.50***</td>
<td>0.49</td>
</tr>
<tr>
<td>Had a male aged 18 to 50 work for pay in early 2007</td>
<td>0.21</td>
<td>0.27</td>
<td>0.21</td>
</tr>
<tr>
<td>Mean change in weekly wage income</td>
<td>8.91</td>
<td>14.67</td>
<td>13.83</td>
</tr>
<tr>
<td>2006 to 2007 (pa'anga)</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
</tr>
<tr>
<td>Median change in weekly wage income</td>
<td>197</td>
<td>251</td>
<td>183</td>
</tr>
</tbody>
</table>

* **, and *** indicate that differs in mean from the RSE households at the 10%, 5%, and 1% levels, respectively. PS-1 and PS-2 are the two propensity-score matched groups. Both match on household demographics, characteristics of the 18- to 50-year-old males in the household, the household’s previous experience and network in New Zealand, household baseline assets and housing, geography, and past household wage and salary history in the first half of 2006 and 2007. PS-2 additionally restricts the sample to households that had a member apply to participate in the RSE.

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9 Of course, other households from the general population also joined the RSE during the time of subsequent survey rounds, but these households were outside our initial sample.
household level to account for autocorrelation in the error term $e_{i,t}$ across survey waves.

Difference-in-differences controls for any baseline-level differences in the outcome $Y_{i,t}$ at the group level. An alternative approach is to control for baseline differences at the household level with household-level fixed effects. We estimate this using

$$Y_{i,t} = \mu_i + \sum_{t=2}^{4} \delta_t + \gamma RSE_{i,t} + e_{i,t},$$

where $\mu_i$ is the fixed effect for household $i$.

In both specifications, $\gamma$ measures the average impact of participating in the RSE over the two-year period of our study. Pooling multiple rounds of posttreatment data provides more power to identify the effect of interest, especially for outcomes like income and consumption that are not highly autocorrelated (McKenzie, 2012). $^{10}$

The difference-in-differences and fixed-effects specifications let us estimate differential effects on RSE households but cannot capture macro effects benefitting the non-RSE households. The RSE has broader effects at the community level, as shown in section VI, but mainly through remittances earmarked for community projects rather than through the absence of migrants, leading to more job opportunities or higher wages for those remaining. Similarly, there appears to be little effect on prices; many of these villages lack markets, with imported durable goods and even food markets often available only in the main large towns.

A second underlying assumption of difference-in-differences and fixed effects is that after controlling for level differences, households would have exhibited similar trends in the outcome variables in the absence of the RSE. This is less credible if the households we are comparing have very different characteristics. We therefore follow the recommendations of Crump et al. (2009) of estimating a propensity score and dropping observations with estimated propensity scores outside the range $[0.1, 0.9]$. $^{11}$ This systematic approach to prescreening the sample ensures the regression is estimated only for the sample where the covariate distribution overlaps for the RSE and non-RSE households. Angrist and Pischke (2009) show that this approach works well in approximating the experimental results obtained in a U.S. work experience program. An alternative approach would be to use the propensity scores directly in estimation. The online appendix shows our results are robust to this alternative, although we prefer the prescreened regression approach given that we have multiple rounds of posttreatment data to use.

Our surveys of RSE and non-RSE households were fielded at the same time in the same villages (and hence local labor markets) using the same questionnaire. We know the characteristics that villages and employers used in selecting workers and can include these in the matching specification. Because this was a new program, employers relied largely on the prescreening and observable characteristics like English literacy to choose workers. Also, we have more than one period of pre-RSE wage earning data (though not many earned wage income). Furthermore, we know whether households tried to participate in the RSE (by having a member register for the work-ready pool or apply directly to an employer). Finally, we have a plausible reason that some households participated in the RSE and other households with similar characteristics did not: there was excess demand for RSE employment, so not all households that wanted to participate were able to.

We estimate two versions of the propensity score PS-1 and PS-2 which differ only in that PS-2 restricts the sample to RSE-applicant households, removing nonapplicant households. This lets us explicitly screen on demand for the RSE, although given that the reason many nonapplicant households said they did not apply was lack of information about the program (Gibson et al., 2008; McKenzie et al., 2008), failure to apply need not imply lack of demand, and the exclusion of nonapplicants reduces our sample size, which has costs in terms of power.

We use six main categories of variables we believe may influence participation in the RSE to estimate the propensity score: demographic variables (household size, numbers of adults, school-aged children, and males aged 18 to 50); characteristics of the 18- to 50-year-old males in the household, who are the individuals most likely to participate (share literate in English, share with schooling beyond grade 10, share with self-reported health rated as very good, share who drank alcohol in the past month, and the mean number of days of hard labor carried out in the past month); the household’s previous experience and network in New Zealand (share of adults who had previously been to New Zealand, number of relatives in New Zealand); household baseline assets and housing infrastructure (an asset index from the first principal component of durable goods, pigs, cattle, and chickens and whether the dwelling was traditional style); geography (on Tongatapu or Efate as opposed to one of the other islands); and past household wage and salary history (household wage income for the first half of 2006 and 2007, and whether the household had any male aged 18 to 50 who worked for pay in 2006 and 2007). For each variable, we include both the variable and its square in estimating the propensity score.

For Tonga, estimating the propensity score and restricting to the range $[0.1, 0.9]$ reduces our sample of 448 households (197 RSE, 251 non-RSE) to 379 households using PS-1 (183 RSE and 196 non-RSE) and 283 households using PS-2 (153 RSE, 121 non-RSE). In Vanuatu the sample of 456 households (147 RSE, 309 non-RSE) reduces to

$^{10}$ An alternative approach would be to use each of the subsequent survey rounds separately with the baseline and attempt to estimate the trajectory of treatment effects. But our power to do this is very low, and hence we focus on the average impact over the two years.

$^{11}$ In both Tonga and Vanuatu, the common support of our propensity score distributions is wider than the $[0.1, 0.9]$ range. Crump et al. (2009) show that the $[0.1, 0.9]$ cut-offs have good optimality properties and closely approach data-dependent optimal thresholds in most contexts.
360 households using PS-1 (129 RSE, 231 non-RSE) and to 269 households using PS-2 (123 RSE, 146 non-RSE). Trimming mainly removes non-RSE households too dissimilar to RSE households to be appropriate comparators, plus a few RSE households that differ too much from any non-RSE household. We reestimate equations (1) and (2) for households with propensity scores in the range [0.1, 0.9]. Again the differencing or fixed effects will eliminate both observed and unobserved time-invariant differences among households, and the assumption of a common underlying trend in the absence of the RSE is likely to be more credible for households with propensity scores within this range.

We use equations (1) and (2) to look at the impact of the RSE on flow variables of interest like income and consumption. For impacts on stock variables like assets owned, we instead estimate, for households within the propensity score range [0.1, 0.9], the following equation:

\[ Y_{t,4} = \alpha + \beta Y_{t,1} + \gamma RSE_{t,4} + e_{t,4}. \]

For example, estimating equation (3) without including the baseline lag since these variables were not asked at baseline.

C. Summary Statistics

The main outcomes of interest are household income and expenditure, savings, asset ownership, and schooling. The online appendix details how we measure income and expenditure, savings, asset ownership, and schooling. Following Paxson (1992), we measure savings as a flow measure, defined as the difference between income and expenditure in a period.

Table 1 reports baseline means of household characteristics in Tonga for RSE and non-RSE households for the full sample and for the PS-1 and PS-2 screened subsamples. Table 2 presents the same comparisons for Vanuatu. Asterisks show the results of tests for difference in means. Consider first the Tongan sample. The average RSE household has 5.7 members. The rural subsistence farming nature of these households is seen in only 21% of these households having any male wage or salary worker in the six months prior to the launch of the RSE, as well as in their ownership of pigs and chickens. Semiannual per capita income and consumption, including the value of goods produced for own consumption, averaged 830 pa’anga (around US$430). This is less than an RSE worker could earn in a good week in New Zealand.
Tongan RSE households tend to be larger and poorer than the average non-RSE household in our sample, and their males worked more days of hard labor on average than in non-RSE households, reflecting selection of workers better able or more inclined to do physical work (table 1). The RSE households are also more connected to New Zealand, with adults in the household more likely to have previously visited New Zealand and the household having more relatives in New Zealand. We believe that in both countries, RSE households in some cases reported the current RSE episode, so the actual difference in pre-RSE experience in New Zealand is likely less than the gap shown here. The third through sixth columns of table 1 show that matching and restricting to households with propensity scores between 0.1 and 0.9 makes the RSE and non-RSE households more similar. The PS-2 subsample in particular does not differ significantly in baseline demographics, income, or consumption from the subsample of RSE households with propensity scores in the [0.1, 0.9] range and differs only in previous experience in New Zealand, which, as noted, may be overstated for RSE households.

A natural concern is then whether our estimation is also perhaps picking up the value of being more connected to New Zealand, and not just the impact of the RSE. We note that after matching, there is not a significant difference between groups in whether they have relatives in New Zealand or in whether other members of the household have been to New Zealand. Second, to the extent a difference remains and is not due to the measurement issue noted above, our difference-in-differences and fixed-effects estimation will still eliminate any time-invariant impact of this on household outcomes. There then does not seem to be a strong reason to think there should be a large time-varying impact of being more connected to New Zealand during our study period.

In contrast to Tonga, table 2 shows that RSE households in Vanuatu are richer than the average non-RSE household, with higher baseline asset ownership, income, and consumption. But many participants are still poor by international standards; 37% of the RSE households have per capita income below $US2 per day. Restricting to propensity scores between 0.1 and 0.9 makes the RSE and non-RSE households more similar, but in contrast to Tonga, using just applicant households in PS-2 does not seem to improve on PS-1. This may reflect the less widespread nature of the work-ready pool in Vanuatu and the fact that many people who would like to have applied did not do so because they lacked information, meaning that some nonapplicants may be better matches for RSE workers in Vanuatu than we can find among our sample of applicants.

Comparing the Tongan and ni-Vanuatu samples shows the much greater prior exposure of Tongans to international migration; the average Tongan RSE worker surveyed has 5.4 relatives in New Zealand, compared to 0.1 relatives for the average ni-Vanuatu RSE worker. With higher levels of schooling in Tonga, a larger share of adult males in Tonga are literate in English, and 46% of 18- to 50-year-old males in RSE households exceed ten years of schooling versus only 6% in Vanuatu. But the Vanuatu sample is more likely to have previously worked for pay, and in the end, the poverty rates are similar for our evaluation samples in both countries. These differences in contexts offer the possibility of examining how different the effects are in quite different starting circumstances, offering some degree of external validity for the results.

After prescreening with the propensity score, both the Tongan and ni-Vanuatu samples are balanced on initial incomes, consumption, and poverty, our key outcomes of interest. The income-generating processes in these countries were fairly stable over the period examined; households mainly do semisubsistence farming as they had been doing for years. Hence, assuming parallel trends in the absence of the RSE seems reasonable. Ideally one would have several rounds of preintervention outcome data to check this, but the difficulty of recalling consumption and agricultural income from previous years makes this infeasible in our case, as it likely is in any similar evaluation. Wage income is more readily recalled, so as a further check, the bottoms of tables 1 and 2 show no difference in the growth in wage income between RSE and non-RSE households over 2006–2007, a full year before the program began.

V. Household-Level Impacts

A. Impact on Incomes, Expenditure, and Savings

Table 3 presents the results of estimating equation (1) in columns 1 to 5 and equation (2) in columns 6 to 10 for Tonga in panel A and for Vanuatu in panel B. Full sample results are shown first and then those for the propensity-score screened samples. To check if results are driven by a few observations at the upper tail, columns 4 and 9 trim the top 1% of observations from the sample. Finally, columns 5 and 10 use nearest-neighbor matching with replacement based on PS-2 and include only the observations that are used in at least one match. This further reduces the sample size and power but serves as an additional robustness check.

Participating in the RSE has a large and statistically significant positive impact on household income per capita in both countries. In Tonga, semiannual income is 233 to 249 pa’anga higher as a result of the RSE, relative to a baseline income of 979 pa’anga. Trimming for potential outliers increases the gain to 300 to 325 pa’anga. Log income is less sensitive to outliers and also shows large and statistically significant increases. Using the estimates that screen on PS-2 and are thus restricted to RSE applicants, log income rises by 0.29 to 0.32, corresponding to a 34% to 38% increase in per capita income as a result of the RSE. Semiannual income in Vanuatu is approximately 44,000 vatu higher, relative to a baseline of 85,000 vatu. In log terms, per capita

13 Consistent with this view, the rate of previous experience in New Zealand is not significantly higher for other members of the RSE worker’s household than it is for the average member in a non-RSE household.
### Table 3.—Average Impact of RSE Migration on Household Income and Expenditure

<table>
<thead>
<tr>
<th>Outcome Variable</th>
<th>Baseline Mean for RSE Households</th>
<th>Difference-in-Differences</th>
<th>Fixed Effects</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>All (1)</td>
<td>PS-1 (2)</td>
<td>PS-2 (3)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>A. Tonga</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Per capita income</td>
<td>979</td>
<td>331.0***</td>
<td>278.4***</td>
</tr>
<tr>
<td></td>
<td>99.3</td>
<td>(105.3)</td>
<td>(129.5)</td>
</tr>
<tr>
<td>Log per capita income</td>
<td>6.57</td>
<td>0.355***</td>
<td>0.346***</td>
</tr>
<tr>
<td></td>
<td>(0.071)</td>
<td>(0.077)</td>
<td>(0.094)</td>
</tr>
<tr>
<td>Per capita expenditure</td>
<td>829</td>
<td>224.1**</td>
<td>127.1</td>
</tr>
<tr>
<td></td>
<td>(111.6)</td>
<td>(81.8)</td>
<td>(104.1)</td>
</tr>
<tr>
<td>Log per capita expenditure</td>
<td>6.58</td>
<td>0.124**</td>
<td>0.117**</td>
</tr>
<tr>
<td></td>
<td>(0.052)</td>
<td>(0.054)</td>
<td>(0.066)</td>
</tr>
<tr>
<td>Per capita savings</td>
<td>150</td>
<td>106.8</td>
<td>151.3**</td>
</tr>
<tr>
<td></td>
<td>(109.2)</td>
<td>(65.12)</td>
<td>(78.10)</td>
</tr>
<tr>
<td>Household size</td>
<td>5.70</td>
<td>0.098</td>
<td>-0.029</td>
</tr>
<tr>
<td></td>
<td>(0.159)</td>
<td>(0.152)</td>
<td>(0.224)</td>
</tr>
<tr>
<td>Number of observations</td>
<td>1,774</td>
<td>1,499</td>
<td>1,092</td>
</tr>
<tr>
<td>Number of households</td>
<td>448</td>
<td>379</td>
<td>274</td>
</tr>
<tr>
<td><strong>B. Vanuatu</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Per capita income</td>
<td>85,282</td>
<td>42,861***</td>
<td>44,441***</td>
</tr>
<tr>
<td></td>
<td>(15,201)</td>
<td>(15,659)</td>
<td>(16,388)</td>
</tr>
<tr>
<td>Log per capita income</td>
<td>10.73</td>
<td>0.320***</td>
<td>0.301***</td>
</tr>
<tr>
<td></td>
<td>(0.104)</td>
<td>(0.107)</td>
<td>(0.116)</td>
</tr>
<tr>
<td>Per capita expenditure</td>
<td>65,872</td>
<td>8,495</td>
<td>13,235**</td>
</tr>
<tr>
<td></td>
<td>(6,590)</td>
<td>(6,131)</td>
<td>(5,559)</td>
</tr>
<tr>
<td>Log per capita expenditure</td>
<td>10.63</td>
<td>0.240***</td>
<td>0.261***</td>
</tr>
<tr>
<td></td>
<td>(0.0745)</td>
<td>(0.0778)</td>
<td>(0.0761)</td>
</tr>
<tr>
<td>Per capita savings</td>
<td>19,410</td>
<td>34,366***</td>
<td>32,088**</td>
</tr>
<tr>
<td></td>
<td>(14,460)</td>
<td>(13,839)</td>
<td>(14,478)</td>
</tr>
<tr>
<td>Household size</td>
<td>4.72</td>
<td>0.0163</td>
<td>-0.141</td>
</tr>
<tr>
<td></td>
<td>(0.158)</td>
<td>(0.168)</td>
<td>(0.179)</td>
</tr>
<tr>
<td>Number of observations</td>
<td>1,574</td>
<td>1,225</td>
<td>977</td>
</tr>
<tr>
<td>Number of households</td>
<td>456</td>
<td>360</td>
<td>269</td>
</tr>
</tbody>
</table>

All outcomes are converted to six-month values. The subsamples used are (a) all: the full sample; (b) PS-1: the propensity-score screened subsample; (c) PS-2: the propensity-score screened subsample restricted to RSE applicant households only; (d) PS-2 after trimming obser-
vations above the 99th percentile for income in the full sample; and (e) nearest N: subsample restricted to RSE applicant households which are nearest neighbors. Robust standard errors in parentheses, clustered at the house-
hold level: ***p < 0.01, **p < 0.05, *p < 0.1.
Household expenditure per capita is also found to increase with participation in the RSE in Tonga. The increase is less than the increase in per capita income and, if we restrict ourselves to the PS-2 screened sample, is significant only after trimming outliers. The log per capita consumption results suggest increases of approximately 9% to 10%, just one-third the increase in per capita income. This is consistent with some of the additional income being saved, and we see that the flow savings per capita has an increase approximately equal to the baseline mean; that is, households double their savings. In Vanuatu, semiannual per capita expenditure is approximately 12,000 to 13,000 vatu higher, relative to a baseline of 65,000 vatu, and the effect on log expenditure is equivalent to approximately a 28% increase. The increase in savings of 15,000 to 30,000 vatu is equal to a doubling or tripling of baseline savings rates. Of particular note is that the point estimates for expenditures are smaller with fixed effects than with difference-in-differences in Vanuatu, which may reflect attenuation bias due to measurement error.14

Finally, table 3 also shows little change in household size as a result of participating in the RSE. The only marginally significant changes come from the fixed-effects specification in Vanuatu. This is consistent with our direct questions, in which few households reported changing household composition as an adjustment mechanism to cope with absent RSE workers.

The median after-tax income earned in New Zealand reported by the seasonal migrants is approximately NZ$12,000.15 This is several multiples of mean annual household per capita income of RSE households at baseline of about NZ$1,400 in Tonga and NZ$2,500 in Vanuatu. Despite the large increase in income from the RSE, one might then ask why the increase in per capita incomes is “only” 35%. First, workers face costs in New Zealand from living expenses (including rent and health insurance) and repaying their share of the airfare. From the NZ$12,000 in income, the average worker remitted or brought back an average of NZ$5,500; it was half remitted and half repatriated for Tongans, but just 10% in the form of remittances and 90% as repatriated savings for Vanuatu. Second, when we consider per capita income, this amount is divided by 5.7 in Tonga and 4.7 in Vanuatu. Third, we are looking at average impacts over two years, so since just over half the households sent a worker in one year only, the per capita per year effect for these households has to be divided by two. Finally, households also lose both the wage income and contribution to agricultural production the household member would have contributed while in New Zealand. Working through these calculations, NZ$5,500 in remittances and repatriated savings for 1.5 years of participation equates to a 550 pa’anga semiannual per capita income increase in Tonga, compared to the 250 to 350 pa’anga increase in semiannual per capita income seen in table 3; and to 32,000 vatu semiannual per capita increase in Vanuatu, compared to the 18,000 to 48,000 vatu increase measured in table 3. They are thus of the same order of magnitude, with differences between the estimated impact and the impact calculated from wages and remittances reflecting the opportunity cost of labor that the migrant would have provided (as well as approximation error).

Nevertheless, the gain in income (equivalent to US$260 per capita per annum in Tonga and US$860 per capita per annum in Vanuatu) still dwarfs other popular development interventions, which have struggled to generate large gains in income. For example, Banerjee et al. (2010) find no increase in average per capita income or expenditure from a microfinance expansion; conditional cash transfer programs in Nicaragua and Mexico (involving the government giving transfers rather than providing a means to generate income) have increased per capita incomes by only US$20 to US$40 (Fiszbein & Schady, 2009); grants to microenterprise owners in Sri Lanka (de Mel, McKenzie, & Woodruff, 2008) and Ghana (Fafchamps et al., 2011) increased microenterprise incomes for male owners at least, but per capita income gains are in the order of approximately US$20 per year; and recent business training programs for microenterprises have some effects on revenues in bad months but no significant impacts on average incomes (Karlan & Valdivia, 2011; Drexler, Fischer, & Schoar, 2011). Bryan, Chowdhury, and Mobarak (2011) examine the impact of grants and loans to induce seasonal internal migration during the hungry season in Bangladesh and find large percentage increases in per capita consumption from a small base, so monthly per capita consumption increases USS5 in the hungry season. Compared with all these other interventions, the gains from international seasonal migration are enormous, especially considering that this intervention involves removing a policy distortion and so, apart from set-up costs, is free, whereas the others require taxes or aid to fund.

B. Impact on Subjective Well-Being

In addition to measuring household welfare through income and expenditure, our final-round survey measured subjective well-being. This addresses the second view of development impact discussed above—that we should look at broader measures than income. One adult per household (the one with the next birthday) was asked to imagine a ten-step ladder, where on the bottom step were the poorest peo-

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14 The Vanuatu data are considerably noisier than the Tongan data. The baseline coefficient of variation of per capita income for the RSE households is 0.90 in Tonga compared to 1.40 in Vanuatu, while the correlation in per capita income from one wave to the next for the non-RSE households varies from 0.43 to 0.77 in Tonga, compared to between 0.19 and 0.27 in Vanuatu. There is thus more signal relative to noise in the Tongan data than in the Vanuatu data.

15 This number accords well with what migrants should have been earning given prevailing wage rates in the sector.
ple and the top step the richest people, and to state which step this person thought his or her household was on today and on which step two years ago. Ravallion and Lokshin (2001) refer to this as an economic ladder question and note that it leaves it up to the individual to define what constitutes “poor” and captures subjective economic welfare.

We estimate equation (3) without including baseline subjective well-being as a control since it is only measured ex post. The results are shown in the first row of table 4. In Tonga, participating in the RSE is estimated to increase subjective welfare by 0.43 standard deviations. This effect is strongly significant. Adding the household’s recalled subjective well-being from two years earlier only slightly reduces this coefficient, to 0.36 for the PS-2 screened group, and is still strongly significant ($p < 0.001$). Participating in the RSE is estimated to increase subjective welfare by 0.71 to 0.83 standard deviations in Vanuatu, which is 43% to 50% percent of a standard deviation and strongly significant. Adding the household’s recalled subjective well-being from two years earlier does not change these results, with coefficients in the 0.74 to 0.85 range and again strong significance ($p < 0.001$). Subjective economic welfare has therefore risen in both countries for households participating in the RSE. Moreover, the increase in subjective welfare is of similar magnitude in terms of standard deviations as the increases in income: the estimated impacts on per capita income in table 3 translates to a 0.24 to 0.43 standard deviation increase in per capita income in Tonga and 0.31 to 0.47 standard deviation increase in per capita income in Vanuatu.

A potential concern here would be if the increased income of migrant households lowered the subjective well-being of nonmigrant households due to relative consumption comparisons. However, subjective well-being did not fall in either country for the control group. In Tonga the PS-1 control group had essentially stable subjective well-being (4.30 in wave 4, with a recall of 4.26 for two years earlier); in Vanuatu subjective well-being rose slightly for the PS-1 control group (4.62 in wave 4, with a recall of 3.62 to two years earlier). As a result we do not believe our results are the result of negative spillover impacts on the control group.

### C. Impact on Dwelling Improvements and Durable Assets

The second row of table 4 shows that Tongan households participating in the RSE were 10 to 11 percentage points more likely to make a dwelling improvement over the period of our surveys almost double the rate for non-RSE households. Home improvements were the most commonly mentioned use of money from the RSE in Vanuatu, but the point estimates suggest that RSE households were just 7 to 8 percentage points more likely to make dwelling improvements, which is only marginally significant. Renovations are reported to be much more commonplace in Vanuatu than in our Tongan survey, with 79% of non-RSE households making a dwelling improvement over the two years of our surveys. The impact of the RSE may also then be for households to make more substantive changes, such as the transition from traditional to modern dwellings, which our surveys do not capture since dwelling type was recorded only at baseline.

In the baseline survey, 65% of Tongan RSE households reported having a bank account. By the fourth round, this had increased to 83%. This represents a statistically significant 10 to 14 percentage point increase relative to non-RSE households over the two-year period (table 4, row 3). This increase in bank account use likely reflects bank accounts being set up for the purpose of household savings rather than bank accounts directly being used to receive remittances. For the Tongan sample, over 90% of remittances were made via money transfer operators that did not require a bank account. In Vanuatu we find the share of RSE households with a bank account increasing from 55% in the baseline to 74% two years later. This is estimated to be a statistically significant 17 to 18 percentage point increase relative to the non-RSE households over the same period. As in Tonga, it is likely that this increase in household bank account use reflects the use of banks for savings rather than just to receive remittances.

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16 Table 4 shows just the PS-1 and PS-2 screened results for reasons of space. The results using the full sample are similar in terms of both magnitudes and statistical significance.

17 This reflects the much higher proportion of households living in traditional (bush material) dwellings in Vanuatu, which have short life expectancy compared with modern dwellings.
D. Impact on Children’s Education and Business Ownership

In addition to raising household incomes and assets, an important motive for many RSE participants was the chance to raise money to pay for school fees. In our baseline survey, 85% of Tongan RSE households and 98% of ni-Vanuatu RSE households said that earning money to pay for school fees was a very important or somewhat important motive for participating in the RSE. In addition, school fees are one of the most common earmarked purposes for RSE remittance income reported in our surveys, and when asked in the final round survey what the most important use of the money earned in the RSE has been, 40% of Tongans and 28% of ni-Vanuatu said school expenses. The question is whether this translates into higher schooling attainment for children. Schooling attainment is also useful to examine as a measure of whether parental absence is causing negative consequences on children.

Table 5 shows the impact of being in an RSE household on school attendance in the final-round survey, conditional on baseline school attendance status and age, that is, equation (3). This is carried out for children who were in the household at both baseline and follow-up. As we have seen, household size and composition did not significantly change with RSE participation.

In Tonga, schooling is compulsory from ages 6 to 15, and there is near universal school enrollment for children of these ages. It is therefore no surprise that we see no impact of the RSE on children aged 6 to 14 at baseline since over 97% of them are attending school whether or not their household is in the RSE. In contrast, the last three columns of panel A show large positive effects of the RSE on school attendance of youth 15 to 18 years old. These effects are statistically significant for the full sample and PS-1 samples and of similar magnitude but not significant in the smaller PS-2 sample (which has less power). The magnitude of the effect is sizable: a 10 to 14 percentage point increase in the proportion attending school, relative to 60% of youth in non-RSE households attending school on average over this two-year period.

In contrast, panel B shows no significant effect of the RSE on school attendance in Vanuatu. One reason may be that starting in 2010, when the final-round survey was in the field, primary schooling became fully subsidized, whereas previous fees were 7,000 vatu per year (10% of per capita average income). Moreover, many schools had allowed students to remain enrolled even with unpaid fees from previous years; the main incentive to clearing these debts was

<table>
<thead>
<tr>
<th>Ownership</th>
<th>Aged 6 to 14 at Baseline</th>
<th>Aged 15 to 18 at Baseline</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Full Sample</td>
<td>PS-1</td>
</tr>
<tr>
<td>A. Tongan children</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Households ever in the RSE</td>
<td>-0.004</td>
<td>-0.004</td>
</tr>
<tr>
<td>Number of observations</td>
<td>478</td>
<td>414</td>
</tr>
<tr>
<td>Proportion of non-RSE students attending school</td>
<td>0.983</td>
<td>0.984</td>
</tr>
<tr>
<td>B. Ni-Vanuatu children</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Households ever in the RSE</td>
<td>-0.006</td>
<td>-0.048</td>
</tr>
<tr>
<td>Number of observations</td>
<td>337</td>
<td>256</td>
</tr>
<tr>
<td>Proportion of non-RSE students attending school</td>
<td>0.816</td>
<td>0.817</td>
</tr>
</tbody>
</table>

Results show regression coefficients after controlling for baseline school attendance and age. Robust standard errors in parentheses: ***p < 0.01, **p < 0.05, *p < 0.1.
to allow students to sit the leaving examinations at the end of grades 6 and 8. Hence, those RSE workers who report the payment of school fees as a motivation may have been repaying school fee debts, which would not show up in current enrollment except for a possibly higher transition rate to high school, which also depends on examination performance.

The apparently divergent impacts of RSE participation on school enrollment also may reflect the nature of the selection into the RSE in the two countries. In Vanuatu, the households in the RSE are relatively better off, and their children have higher baseline school attendance rates than non-RSE households. It is therefore possible that credit constraints were not limiting schooling for this group. In contrast, Tongan RSE households were relatively poorer at baseline than non-RSE households, with lower school attendance rates. The extra income earned through the RSE allows them to then catch up to (and surpass) the school attendance rates of the non-RSE group.

Our data do not show any evidence that the RSE has fostered the development of nonagricultural businesses among the households in our sample in Tonga. None of the households surveyed mentioned investment in a business as a main use of the money earned in the RSE, and we do not observe any individuals in RSE households starting a new business over the two-year period of our surveys. In Vanuatu we had only five households in the round 4 survey say the most important use of the money earned through the RSE was starting a business or supporting an existing business. Given the low population densities and small local markets, it is not clear what the scope for such business start-ups actually is, but over the first two years of the RSE policy, there does not appear to be much evidence that it fostered self-employment. Indeed the recent Pacific Futures report (World Bank, 2011) cites small market sizes and long distances to other markets as reasons Pacific Island economies are unlikely to have much firm growth.

Our surveys do not directly measure agricultural production techniques or other work skills. However, there are reasons to believe that to date, the RSE is unlikely to have had a major impact on the productivity of workers on their own farms. The crops tended to in New Zealand (apples, grapes, kiwifruit) are different from those grown in the Pacific Islands (bananas, yams, cassava, squash), and the climate and soil conditions are also vastly different. Our surveys asked return workers directly if they had learned any new skills. The main one workers mentioned was pruning plants—important in the New Zealand fruit industries but not for the crops they produce. Absent any short-run evidence on purchases of livestock or farm machinery, it therefore seems unlikely that home production skills or technology have improved dramatically from participation in this program.

VI. Community-Level and Country-Level Impacts

Finally we consider the broader impacts of the RSE on the sending communities. The most direct impact is through the monetary contribution that migrants make to their communities, either through remitting to a community group while abroad or contributing some of their repatriated earnings to this group on return. We asked return migrants how much they had contributed in this way to the community. The mean response aggregated over the two years was 157 pa’anga in Tonga and 11,733 vatu in Vanuatu—or approximately US$80 to $130 per migrant. Our expenditure module also collected expenditure on community obligations, but only for a recall period of one month, thereby likely missing one-off contributions made by migrants on return. The difference-in-differences regression then gives a positive but insignificant impact on this item.

To further gauge the impacts at the community level, we conducted surveys of community leaders. This was done at baseline and at the time of the second-round survey in Vanuatu, and at baseline and at the times of the third- and fourth-round surveys in Tonga. These data are thus less useful for Vanuatu, since they measure only immediate effects while most workers were still away. The Tongan surveys reveal the mean (median) community saying it received 633 (500) pa’anga from RSE workers, which is consistent with the household surveys, given a median of five workers per village participating coupled with the amounts reported by workers. The main use of this fund in 83% of cases in Tonga was funding the village water supply in the first year. In the second year, villages were also using this for street lighting, a school scholarship fund, community halls, and, in one case, adding Internet to a community hall.

Village leaders were directly asked the main benefit and main disadvantages of the RSE for their community. In Vanuatu this was only asked in wave 2, six months into the RSE. The main benefits reported at this stage were job opportunities for people in the village, money to support the village church, and improvements in housing. Disadvantages were fewer people to do the community work, cases where a worker was not contributing to church or family, and concerns about the potentially bad influence of alcohol abroad. In Tonga these questions were asked in waves 3 and 4, approximately one year and two years into the RSE. The main benefit reported at one year is income for families, along with some saying income for the community and church donations. After two years, there are also a few mentions of improved skills and improved English of workers and positive impacts on school enrollments.

When asked the main disadvantage, more than half at one year, said none, the main other answer being family separation. At two years, one-third said family separation; 30% said less labor available for the village, church, and community projects; and about 15% said fewer members for church activities.

Online appendix table 5 summarizes the results of questions in both the household and community leader surveys intended to measure qualitatively the impressions of the broader community-level impacts of the RSE. The RSE workers themselves believe that participation in the RSE
either improved or left unchanged their family and community life. Non-RSE households in Tonga also saw benefits in terms of community life, availability of paid jobs, and schooling opportunities. To the extent that such benefits are really accruing to non-RSE households, our estimates comparing RSE to non-RSE households will be a lower bound on the positive development impact of the RSE program. Non-RSE households in Vanuatu were more likely than those in Tonga to say there had been no change in community life or in job or schooling opportunities. Finally, community leaders were asked their assessment of the overall impact of the RSE on their communities. In Tonga 92% of leaders said that it has had positive effects after two years, and in Vanuatu, even at six months, 72% of leaders said the overall impact was positive.

Finally, table 6 shows that the overall development impact of the RSE program is important for Tonga and Vanuatu. We take the per worker estimates of the average impact of the program on household income over the first two years and scale up by the total number of workers hired from each country to get total positive development impacts of NZ$5.3 million in Tonga and NZ$9.7 million in Vanuatu. This amount is equivalent to 42% to 47% of total annual bilateral aid from New Zealand to these countries and is still nontrivial when compared to bilateral aid from Australia, the other main donor. Moreover, this aggregate development impact of the RSE program is equivalent to almost 50% of annual export earnings for Tonga and 25% of annual export earnings for Vanuatu.

VII. Conclusion

New Zealand’s RSE program was designed with promoting development in the Pacific Islands as an explicit goal. The results of our multiyear prospective impact evaluation show that it has largely achieved this goal. We consider three views of development impact and find evidence to support impacts under each view. Under a narrow view of impact as increasing household incomes, we find the RSE led to sizable increases in income in both Tonga and Vanuatu. Under a broader utility-based view of development, we find subjective standards of living have improved, households are spending more, and community leaders view the policy as having an overall positive impact. Finally, under a view that what matters is whether the income gained from this program is spent or invested in ways that will continue to benefit the household in the longer term, we find increases in household durable asset accumulation, increases in savings, and, in Tonga, some evidence that it improved child school attendance for older children. However, we do not find evidence of increased self-employment or substantial accumulation of skills that can increase home productivity. This reflects the limited market sizes in the participating countries and is consistent with the view that migration is likely to be a long-term part of the economic organization of Pacific Island economies (World Bank, 2011).

These results make this seasonal migration program one of the most effective development interventions for which rigorous evaluations are available. In addition, although there has been nontrivial investment by both the New Zealand and Pacific governments in the set-up phase, it does not involve grants and appears to be benefiting both private employers and workers. The design features of the program and the low rate of overstaying have already led to this policy being heralded as international best practice. The large development impacts seen here should lead other countries to consider similar policies.

Nevertheless, there are several caveats to these conclusions. The first is that development is a long-term process, and some of the effects of the RSE may materialize only over many years of community involvement. These could include positive effects such as greater asset building, investments, and skill development if workers return for many seasons, and potential longer-term negative effects of continual absence of family members on family and community relations. Second, although the gains to households from this seasonal migration are large, they still pale in comparison to the gains from permanent international migration (Clemens et al., 2008; McKenzie et al., 2010). A key policy issue is therefore the extent to which seasonal migration can or cannot eventually open up avenues for permanent migration.

REFERENCES


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