

Access to Migration for Rural Households[†]

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This paper exploits a unique feature of China’s history, the “sent-down youth” (SDY) program, to study the effects of access to internal migration. We show that temporary migration due to the SDY program created lasting inter-province links. We interact these links with two time-varying pull measures in potential destinations. Decades after the SDY program ended, increased access to migration in cities that sent SDY leads to higher rates of migration from provinces where those SDY temporarily resided. We find that improved access to migration leads to lower consumption volatility and lower asset holding. Furthermore, household production shifts into high-risk, high-return activities. (JEL D13, J24, O15, O18, P25, P36, R23)

Migration offers a key form of arbitrage: especially for poor individuals, human capital may represent a large share of wealth, and migration allows this human capital to receive a higher return.¹ While much of the focus of the economic literature on migration has been on the impact of migration on migrants and on workers in receiving communities, the impact of migration opportunities on sending households and communities is less understood but extremely important for policy-makers in developing countries who are interested in reducing rural poverty and regional inequality. Using a novel identification strategy, our paper addresses the question of how changing incentives to migrate affect the economic choices and outcomes of agricultural households in communities sending the migrants.

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¹The potential income gains from rural-to-urban migration are substantial; the rural-urban wage gap is estimated to be 10 percent in China and as high as 45 percent in India (Munshi and Rosenzweig 2016). However, Young (2013) suggests that these gaps are driven by selection on unobservables, in which case, increasing migration would not close the wage gaps.

We exploit a unique feature of China's economic environment to understand how opportunities for internal migration affect agricultural households: historical patterns of urban-to-rural migration during the "sent-down youth" (SDY) campaign. In this program, the government mandated the temporary resettlement of roughly 18 million urban youth to rural areas from 1962 to 1980.² Our strategy builds on previous papers that use historical flows to instrument for migration (Card 2001, Munshi 2003, Hanson and Woodruff 2003, Hildebrandt et al. 2005, McKenzie and Rapoport 2007, and Dinkelman and Mariotti 2016), but there are key advantages to our analysis in the Chinese context. One is that the program sent urban youth to rural areas temporarily; the vast majority of the SDY returned to urban areas by the 1980s (Pan 2002), so we are able to examine whether ties persist once the original contacts have left. Moreover, whereas the previous research exploits connections determined by historical movements in the same direction, we examine the effect of urban to rural movements on subsequent *rural to urban* movements.³ Furthermore, our findings contribute to the study of the role of interpersonal ties in driving economic growth.⁴ To our knowledge, we are the first to demonstrate that the large-scale movements associated with the SDY program created lasting linkages between the provinces that sent and received SDY.

To address the concern that our cross-sectional measure of historical SDY flows may be correlated with other ties between origins and destinations, we interact the SDY flows with two time-varying pull measures for migrants. This allows us to absorb time-invariant correlations between sending and destination areas and recover causal estimates of the impact of incentives to migrate.⁵

Our first pull factor builds on Card and Lewis' (2007) work on Mexican migration to the United States and uses local labor demand shocks in potential migrant destination provinces, as measured by shocks to output in industries that employ a lot of migrants. This methodology exploits local labor demand shocks and historical migration patterns between states in Mexico and the United States. Here, following the same idea, we use interprovincial SDY flows interacted with time-varying labor demand shocks in potential destinations to examine internal migration within China.

The second pull factor that we exploit is recent variation in the ease of migration generated by province-level reforms in the *hukou* system. These reforms allowed some migrants to register and receive social services in urban areas.⁶ As discussed in Section IIB, several other papers have also used variation in the *hukou* system to

²See Section IIA and Bernstein (1977) for details on the SDY program.

³Another relevant feature is that the decision to migrate and the locations associated with the SDY were not choices of the migrating individuals; instead, the routes of this involuntary migration were chosen by government planners. However, given that we interact these historical flows with time-varying pull factors in urban areas, our identification strategy does not *require* exogeneity of SDY flows with respect to outcomes in the rural areas to which they were sent.

⁴Our findings are related to Burchardi and Hassan (2013), who show that interpersonal relationships between East and West Germany persisted over time and were an important driver of growth after reunification.

⁵Other previous work on migration has attempted to overcome the associated selection problems using various strategies: controlling for observable differences (Adams 1998), propensity score matching (e.g., Acosta 2011), natural experiments (e.g., Clemens 2010; McKenzie, Stillman, and Gibson 2010), randomized experiments (e.g., Bryan, Chowdhury, and Mobarak 2014), or using instrumental variable strategies based on exogenous factors such as shocks at the migration destination (e.g., Yang 2008).

⁶See Section IIB and Chan and Zhang (1999) for details on the *hukou* system.

study migration; to our knowledge, ours is the first to use its interaction with ties created by the SDY program. This allows us to use cross-province variation while relaxing the assumption made in the previous literature that timing of *hukou* reforms in a province is orthogonal to economic conditions in that province.

Examining two sources of variation that affect the incentives to migrate has several advantages. First, it allows us to examine different margins of selection into internal migration. The labor demand shocks are year-to-year fluctuations and represent changes in the short-run returns to migration. In contrast, the *hukou* reforms are relatively rare, but persistent changes to the long-run cost of migration. This comparison allows us to understand how the benefits and the economic and institutional barriers to migration affect the outcomes of interest. Second, while each source of variation requires a different set of assumptions (which we discuss below), interestingly, both pull factors yield qualitatively similar conclusions.

We combine our key regressors—the interaction of the SDY flows with the two migration pull factors—with outcomes from a detailed panel dataset on production activities of rural households from the Chinese Ministry of Agriculture. The panel nature of our data allows for the inclusion of household fixed effects, which absorb time-invariant heterogeneity in the propensity to invest in risky activities, propensity to migrate (including any time-invariant ties between the area of residence and other areas), etc. The time-varying nature of the pull factors allows for the inclusion of time fixed effects, which absorb secular changes in the attractiveness of migration or of different investments. Thus, we run difference-in-difference-type regressions in order to identify the impact of increased access to migration. We compare the change over time in outcomes among households unexposed or less exposed to changes in pull factors to the change among households (more) exposed to changes in pull factors. The identifying assumption behind our analysis is that the changes in pull factors in provinces that sent SDY in the past, and that now represent potential destinations for rural-to-urban migrants, are uncorrelated with potential outcomes in *other* provinces, namely those that received those SDY in the past. We test this identifying assumption, and find that it is supported in our data.

Our paper is among the first that analyzes the effect of incentives to migrate on outcomes related to agricultural production decisions of rural households. There are several reasons that one might expect production decisions to change when households have better access to migration opportunities. One mechanism is a wealth effect, wherein migrants' earnings increase relative to what they would have earned in the absence of migration, and this income is shared with households via remittances. A positive wealth effect may lead to an increase in leisure and a corresponding decline in total production. Alternatively, a negative wealth effect is possible, due to the cost of migration itself or the loss of a productive household member (if that member's wages are not fully shared with the non-migrating household members). A second possible mechanism is an insurance effect: the migrants are exposed to different shocks than the agricultural households that they left, and this diversification allows households to shift into riskier activities (Rosenzweig and Binswanger 1993). Alternatively, migration itself may be risky (Bryan, Chowdhury, and Mobarak 2014), so rural households' portfolios could shift *away* from risky activities.

Our results are similar regardless of whether we use the variation driven by increased returns to migration opportunities or by lower barriers to migration. We find that increased access to migration leads to an increase in the level of rural households' consumption and a decrease in the variability of consumption. Agricultural production decisions also change, with a shift toward riskier activities including animal husbandry and fruit farming. These results are consistent with a positive wealth effect or with insurance derived from having a migrant working outside of the household. However, a further finding suggests that insurance effects are at work: we observe a substantial fall in assets without a corresponding fall in income or labor. Similar to the findings of Kaboski and Townsend (2011), where an expansion of credit in Thailand led to a fall in assets, the results on assets in our setting are consistent with the interpretation that households decrease their holdings of low-yield buffer stock savings in response to improved access to insurance via migration.

The results are consistent with prior research suggesting an insurance mechanism related to migrants. Rosenzweig and Stark (1989) shows that in rural India, internal migration for the purpose of marriage facilitates consumption smoothing by spreading family networks over locations with less covariate weather shocks. Using Tanzanian panel data, De Weerd and Hirvonen (2012) finds that migrants insure nonmigrants, but not the other way around. Giles and Yoo (2007) uses long lags of rainfall to instrument for the size of the migrant network, and show that households with a larger migrant network engage in less precautionary savings. More recently, Morten (2015) uses a structural model to examine the interaction between internal migration and insurance in rural India. Our setting is novel, however, in that we have detailed data on the production decisions of rural households, and can examine how these decisions change due to improved access to migration.

In addition to the production outcomes that we examine, we are able to offer a new perspective on the impact of migration on the well-being of remaining household members. Our panel dataset spans eight years, and the relatively long time frame allows us to look at measures of welfare, such as consumption smoothing, which are difficult to study in shorter panels. Prior research has demonstrated that remittances received from migrants correspond with an increase in remaining household members' income, asset ownership, and consumption of normal goods including education and health.⁷ However, other research suggests that migration may generate negative impacts on sending families stemming from the loss of a family member and household laborer.⁸ Prior papers also find mixed evidence on children's education (Antman 2012, Edwards and Ureta 2003, Yang 2008, McKenzie and Rapoport 2011, and Dinkelman and Mariotti 2016) and adult employment of remaining members (Funkhouser 1992, Yang 2008, Amuedo-Dorantes and Pozo 2006). Relatedly, Gibson, McKenzie, and Stillman (2011) finds that in the short-run, in Tongan households in which a member won a lottery to migrate to New Zealand, income falls as does asset ownership and access to finance, suggesting that migration may be costly to remaining households in the short run.

⁷ See Rapoport and Docquier (2006) for a good review on this literature.

⁸ There are also other potential difficulties associated with split families, including problems with hidden income (Joseph, Nyarko, and Wang 2018).

Finally, we contribute to the growing literature on internal migration (e.g., Beegle, De Weerd, and Dercon 2011; Bryan, Chowdhury, and Mobarak 2014; and Bazzi et al. 2016). While there exists a larger literature on international migration, there are many reasons to believe that impacts for international migration cannot be simply extrapolated to internal migration. Internal migration is more often short-term and over smaller physical distances, so the ease of and incentives for remitting may be greater, potentially yielding greater benefits for non-migrating household members. However, income differentials are likely smaller for internal than international migration, and the correlation between earnings of migrants and their sending households may be higher.⁹ Questions related to internal migration are highly relevant: of an estimated 1 billion migrants worldwide, almost 75 percent are internal migrants (Klugman 2009). Moreover, the form of internal migration we study, interprovincial migration, is highly relevant in China: according to 2010 census data, 50 percent of internal migrants were interprovincial migrants (Liang 2012).

I. Conceptual Framework

There are several channels through which improved access to migration might affect rural households. We begin with two possible direct channels: wealth effects and insurance effects.

If migrants provide remittances to household members who remain in the origin communities, this increase in wealth can lead to more consumption by the rural households. If households were not credit constrained prior to migration, because leisure is a normal good, income earned by rural household members, and their corresponding investment in agricultural production, may fall. If households were credit constrained prior to migration, the hours worked and earnings of rural household members may rise as the migrant may be able to finance higher investment; moreover, if investment exhibits fixed costs or non-convexities, nondurable consumption may fall.¹⁰ If households exhibit decreasing absolute risk aversion, such as in the commonly used constant relative risk aversion (CRRA) utility function, an increase in wealth will be associated with increased investment in high-risk, high-return assets. However, if the remittances sent by the migrant are less than the amount that the migrant contributed to household earnings before migration, migration may cause a *negative* wealth effect for households and a corresponding decline in their consumption and risk-taking.

In addition to a wealth effect, there may be an insurance effect from migration given that migrants' income will typically be uncorrelated or less correlated with the income of the remaining household members; in other words, the overall portfolio of household activities becomes more diversified when a member migrates. If migrants can provide state-contingent remittances, gifts, or loans (De Weerd and Hirvonen 2012), this increases the household's ability to insure risk associated with

⁹The extent of potential negative effects on nonmigrants, such as divorce, isolation between parents and children, and negative spillovers on villages due to the loss of prime-aged workers, may also differ between international and internal migration.

¹⁰Banerjee, Karlan, and Zinman (2015) shows how access to credit may cause consumption to fall if non-divisible investment increases; the effect of remittances is similar.

their income. The insurance from having a migrant, in turn, may lead to increased investment in high-return, risky activities, if the household was not previously able to insure income risk fully (see Karlan et al. 2014). However, insurance provided by migrants may reduce households' overall investment/savings due to a reduction in buffer-stock savings (Deaton 1991).¹¹ Moreover, migrants' income may itself be risky (Bryan, Chowdhury, and Mobarak 2014). Rural households may be exposed to the risk faced by migrants either directly, because they provide transfers to migrants, or indirectly, because they receive reduced remittances from migrants when migrants' income is low. If households are exposed to risks faced by migrants in a way that increases the total risk they face, they may reduce the riskiness of their own production activities, potentially at the cost of accepting lower average returns (Rosenzweig and Binswanger 1993).

The preceding discussion was framed in terms of the direct effect of migration; however, anticipation of the ability to send a migrant in the future may cause rural households to change their behavior even before sending a migrant. If migration is a valuable ex post smoothing strategy (Morten 2015), households can increase investment in risky assets and/or liquidate buffer stocks even before sending a migrant. However, if households need to save up for migration, they may reduce consumption and/or increase labor supply prior to sending a migrant. Effects of migration may also persist after the migrants return, due to changes in wealth, information, household dynamics, etc. For these reasons, our empirical analysis will focus on the effect of changes to *access* to migration rather than the effect of migration *per se*; thus, we will report intent-to-treat effects rather than treatment on the treated effects.¹²

In addition to anticipation effects, there is another potential indirect effect whereby increased access to migration may reduce aggregate volatility in those rural areas. Improved access to migration could increase the local labor supply elasticity by allowing households to respond to negative shocks by migrating instead of "selling labor low" in the local labor market (Jayachandran 2006). Such a general equilibrium effect on aggregate volatility could change the outcomes of households that neither send a migrant nor anticipate ever sending a migrant.

Given the theoretically ambiguous effects of access to migration on investment, consumption, and welfare of rural households, empirical evidence is needed. The remainder of the paper will attempt to shed light on the mechanisms that are relevant in the context of agricultural households in China.

II. Institutional Background

A. The Sent-Down Youth Policy

Between 1962 and 1978, nearly 18 million urban youth, mainly aged 16 to 20, were sent to rural areas to live and work. These youth were referred to as "sent-down youth" or *zhiqing*. The policy's official goal was to promote rural development and to have urban bourgeois youth learn from living in rural poverty; an underlying

¹¹ A decline in buffer-stock savings could finance an increase in consumption.

¹² We briefly discuss treatment on the treated effects in Section VIF.

objective appears to have been to address high urban unemployment (Bernstein 1977, Gu 1997). Some sent-down youth were sent to rural areas near their home city, but others, especially those from large cities, were sent to other provinces, sometimes thousands of kilometers away.¹³ In total, 1.5 million sent-down youth were sent outside their home provinces. Some sent-down youth stayed only a year or two, while others stayed for more than a decade before the policy was discontinued in 1979. On average, they stayed three to four years and performed manual, agricultural labor (Bernstein 1977). While small numbers of sent-down youth stayed in the rural areas they were sent to, the vast majority (over 90 percent) returned to the urban areas from which they came (Zhou and Hou 1999).

Allocation of SDY to Destinations.—As noted above, our identification strategy does not require that the allocation of SDY from urban areas to rural destinations was exogenous. Nonetheless, it is of interest to understand the mechanism by which SDY were assigned to destinations.

In determining locations for the SDY, administrators focused on needs by gender and age (Rene 2013). For example, in an area where the work was particularly physical, they would assign more men. But in areas where there were no differences in physical needs, they would try to balance the numbers by gender and age. Transfers of SDY occurred in batches over time, so the specific location a person went to was in large part determined by when they were sent down. Rene (2013) cites several examples of families where multiple siblings were sent down at different points in time and ended up in very different locations.¹⁴

In Section VB, we show that our first-stage results on the persistence of SDY ties are robust to adding controls for measures of geographic distance, baseline trade flows, and baseline sectoral composition. This demonstrates that the differential migration response of migrants from a SDY receiving province to the province from whence those SDY came is not merely proxying for the fact that sending and receiving provinces are geographically close, or were connected by trade flows at the start of the National Fixed Point (NFP) panel, and furthermore, demonstrates that neither proximity nor complementarities in sectoral composition drove SDY allocations.

Persistence of SDY Ties.—We investigate the possibility that receiving sent-down youth (SDY) from a large city may create personal connections and knowledge about that city, which may persist over time and increase the desirability or salience of that city as a possible migration destination. For instance, Yunnan received 56,600 SDY from Shanghai, which may have created connections or provided information that facilitated the subsequent voluntary migration of Yunnanese people to Shanghai. Marriage rates between local residents of the rural areas and SDY from urban areas provide some suggestive evidence that the SDY formed strong bonds

¹³ See online Appendix Figure A1 for a map showing the interprovincial origins and destinations.

¹⁴ Further supporting the exogeneity of the assignment of urban SDY to rural areas, You (2017) analyzes the county-level allocation of SDY within the province of Heilongjiang and shows that their allocation is uncorrelated with local education levels and participation in nonagricultural labor. You (2017) also notes that the chaotic political environment in 1960s and 1970s China meant that there was little or no capacity to sort SDY into destinations based on any criteria beyond gender and age.

with locals during their stay; data from a government conference report on the SDY in 1978 suggests that about 7 percent of the sent-down youth remaining in rural areas in 1978 were married to local individuals in the rural areas (Gu 2009). In addition, there is anecdotal evidence that ties due to SDY flows persisted over time. For example, *Nie Zhai*, a novel written by a former SDY, Xin Ye, and later adapted into a television series, depicts the persistence of sent-down youth ties. The novel and show focus on children of SDY who remained in rural areas, and who later went to Shanghai to look for their relatives.

While previous economic research has examined parental choices over which child to send-down (Li, Rosenzweig, and Zhang 2010) and the impact on being sent down on the outcomes of the individuals directly experiencing the migration (Fan 2017, Meng and Gregory 2002, Zhou 2014), to our knowledge, we are the first to explore the extent to which migration patterns associated with the sent-down movement generated lasting connections between rural and urban regions.

B. Hukou System

China's *hukou*, or household registration system, was set up in the 1950s as a system of monitoring population flows (Chan and Zhang 1999). After the implementation of the Communist Party's economic plan, the Great Leap Forward (1958–1960), the *hukou* system was repurposed to control rural-to-urban migration given the government's desire to keep food prices low and provide welfare benefits for urban residents. While the first constitution of the Peoples Republic of China (PRC), issued in 1954, guaranteed citizens the freedom to migrate and settle in the area of their choice, the subsequent issuance of "Hukou Registration rules of the PRC," issued in January 1958, began the dual-*hukou* system, which divided people into those holding a rural *hukou* and those with an urban *hukou*. This essentially voided the "freedom to migrate" specified by the first constitution.¹⁵

An individual's *hukou* determines their eligibility for jobs, schooling, housing, and other rationed goods in a specific city or county. In particular, an individual with a rural *hukou* cannot legally work for a state-owned enterprise or the government or receive state services in an urban area. Thus, most rural *hukou* holders can only work in the city as "temporary workers" (*linshi gong*), and do not get insurance, retirement benefits, housing subsidies, or other allowances and are not subject to the same labor protections as urban *hukou* holders. Moreover, many private firms in urban areas are reluctant to permanently hire an individual without the corresponding urban *hukou*.

Initially, it was impossible for the holder of a rural *hukou* to convert to an urban *hukou*. However, in July 1985, the Ministry of Public Safety issued the "temporary regulation on the town and city *hukou* registration system," which allowed conversions of rural to urban *hukou* (*nongzhuanfei*); however, this was quite rare in this period.¹⁶ The annual quota (*zhibiao*) for *nongzhuanfei* was minuscule at

¹⁵The first constitution was revised in 1975 and the "freedom to migrate" clause was deleted.

¹⁶Furthermore, a worker who successfully obtains an urban *hukou* cannot necessarily get the same for their spouse or dependents.

0.02 percent.¹⁷ In other words, a city with 100,000 city *hukou* holders could allow 20 rural people to obtain a *hukou*. However, starting in the early nineties, provinces began to open the conversion process to more people. The main factors influencing the timing of these reforms were increases in urban labor demand, the desire by the local government to improve demand for local real estate, and political incentives (Wang 2009; Peng, Zhao, and Guo 2009). The timing of these reforms varied across provinces and across time, providing a key source of variation that we exploit.

Several previous papers have inferred the effects of the *hukou* system on labor mobility using aggregate data (Bosker et al. 2012, Whalley and Zhang 2007) but have not used cross-province variation to address potentially confounding aggregate trends. An exception is Sun, Bai, and Xie (2011), who code and use variation across provinces and time in reforms of the system as we do; they find that *hukou* reforms adopted by a province correspond to an increase in intra-provincial migration. Unlike Sun, Bai, and Xie (2011), the identification strategy in our paper does not rely on the exogeneity of *within*-province *hukou* reforms.¹⁸ Rather, we show and exploit the idea that *hukou* reforms in a province which historically sent SDY to another (recipient) province correspond to subsequent increases in migration rates between the provinces in the reverse direction of the SDY flow. In a concurrent working paper building on an earlier national reform, de Brauw and Giles (2014) exploits differences in the timing of access to national identification cards; they argue that these cards make it easier for rural residents to temporarily work in urban areas. Their strategy relies on the assumption that the timing of access to identification cards is exogenous to other economic conditions within provinces that affect migration.

There are many anecdotes supporting the hypothesis that individuals migrated in response to the provincial-level *hukou* reforms. This is also true among individuals who could not immediately qualify to apply for an urban *hukou*. For example, after a *hukou* reform in Guizhou in 1997, “36-year-old Mrs. Aifen Wang left her hometown that year and opened a flour mill in Honghe city. Since then, she kept saving for ten years. After a more aggressive *hukou* reform in Honghe in 2006, she bought an apartment in the city and became *chengliren* [a resident with urban *hukou*]” (Xue and Chu 2007).

C. Labor Demand

Urban labor demand grew rapidly in China over the period of our analysis (1995 to 2002). Calculated from the Chinese Statistical Yearbooks, the cumulative province-level growth rate in urban GDP per capita was 73.4 percent (which translates into an average annual growth rate of 10.5 percent). While growth also occurred in rural areas, the gains were not as striking; the cumulative province-level growth rate of *rural* GDP per capita was 25.3 percent (about 3.61 percent per year).

¹⁷ See <http://www.mps.gov.cn/n16/n1252/n1657/n2107/96328.html> (accessed February 2015).

¹⁸ Indeed, we demonstrate that the timing of a province's *hukou* reforms is correlated with other characteristics of the province.

III. Data

Our analysis combines outcomes from household-level panel data from the Ministry of Agriculture with variation at the province-level from datasets that we assembled on historical SDY flows, *hukou* reforms, and labor demand shocks. The SDY flows data is a cross-sectional dataset of the total interprovincial flow of individuals under the program, while the data on *hukou* reforms and labor demand shocks vary over time and province.

A. National Fixed Point Survey

Our primary data source for the outcomes of agricultural households is the National Fixed Point (NFP) Survey, a panel survey collected by the Research Center of Rural Economy (RCRE) of the Chinese Ministry of Agriculture, beginning in 1986. We use annual waves of data between 1995 to 2002 for data comparability, as the questions and the structure of the survey changed substantially in 1995 and again in 2003. The dataset used in our analysis covers over 14,000 households from 234 villages in 19 provinces.¹⁹

NFP villages were selected for representativeness based on region, income, cropping pattern, population, and nonfarm activities. The NFP contains detailed information on household agricultural production, consumption, asset accumulation, employment, and income. Benjamin, Brandt, and Giles (2005) provides a detailed description of the data and show evidence that the data are of good quality. Its particular advantages for our purposes are its panel structure and detailed data on household production decisions.

Over the period 1995 to 2002, the data only include household-level information. In other words, with the exception of a few characteristics of the household head (e.g., age and education), the data do not include individual-level characteristics. For example, we do not know the gender and education of each member of the household, but we know the number of household members, the number that are male, and the number in each bin of education. Thus, if a household has four members, two of which are male and two who completed primary school, we do not know if the males were the ones to complete primary schooling. One major implication of this lack of individual-level information is that we do not know the individual identity of the household member who migrates. While we do not know the characteristics of the migrant, we do know whether or not any household members have migrated. We also do not know where exactly migrants go, which month they leave or return, or what kind of work they are engaged in. Finally, we do not have a good measure of remittances that the household receives.

The summary statistics for the NFP data are presented in Table 1, where there is one observation per household, corresponding to the first year that the household appears in the data. We present levels in the summary statistics for ease of

¹⁹The provinces are Chongqing, Gansu, Guangxi, Guizhou, Hebei, Heilongjiang, Henan, Liaoning, Ningxia, Qinghai, Shandong, Shanghai, Shanxi, Sichuan, Tianjin, Xinjiang, Yunnan, Xizang (Tibet), and Zhejiang.

TABLE 1—SUMMARY STATISTICS

	Mean	Standard deviation	Observations
Migrant (0/1)	0.162	0.368	14,011
Migration (days)	29.763	79.224	14,011
Migration (days, not including zeros)	184.029	102.081	2,266
Year	1,995.639	1.691	14,016
Total consumption (per person)	508.444	428.214	13,793
Food consumption (per person)	262.434	150.914	13,686
Non-staple food consumption (per person)	145.830	131.372	13,703
Agricultural income (per worker)	2,846.757	2,329.061	11,456
Nonagricultural income (per worker)	3,343.349	5,156.641	11,457
Agricultural labor inputs (per worker)	166.936	97.908	11,438
Household laborers (aged 18–65)	2.421	0.978	11,319
Nonproductive assets (per worker)	1,079.634	1,532.838	11,495
Agricultural assets (per worker)	470.609	689.273	11,426
Nonagricultural assets (per worker)	98.405	1,059.414	11,322
Positive days on fruits (0/1)	0.220	0.414	14,013
Days on fruits (per worker, not including zeros)	29.101	44.657	2,820
Days on fruits (per worker)	5.584	17.533	11,460
Income from fruits (per worker)	120.310	494.498	11,432
Positive days on animal husbandry (0/1)	0.710	0.454	14,014
Days on animal husbandry (per worker, not including zeros)	55.905	45.079	9,049
Days on animal husbandry (per worker)	42.090	40.609	11,468
Income from animal husbandry (per worker)	699.334	970.264	11,433
High education (middle school degree or higher)	0.475	0.499	14,012

Note: The table presents summary statistics of the NFP data where each observation refers to the first period that a household appears in the data.

understanding the magnitudes but the regressions use log measures.²⁰ We drop the top and bottom 1 percent of values to deal with outliers; however, the results in the paper are very similar if we do not drop outliers.

To address the issue that household size changes mechanically with migration, we examine most outcomes in per capita terms. We calculate per capita values using the number of residents in the household at the time of the survey, as reported by the survey respondent. To ensure that our results are not driven by the fact that migrants may not be reported as household residents if they are migrating at the time of the survey, we construct two additional measures of household size. The first adds one to the household size if the household reports that a member spent any days working as a migrant worker the past year. This is a conservative measure that will reduce per capita values of income and consumption; if our results are driven by migrant-sending households spuriously appearing smaller than they are, this measure will address the issue. A second measure assigns household members younger than 18 or older than 65 an equivalence weight of 0.5 relative to those aged 18 to 65. This will address any differences in the age structure between migrant-sending households and those who do not send migrants. The results using these two measures of household size, which are available on request, are very similar to the main

²⁰ Given that some values of assets, consumption, and income may be zero, the log measures are all measured as the log of the variable plus one. The results are all very similar if we use the inverse hyperbolic sine (Burbidge, Magee, and Robb 1988) instead of the log.

results, confirming that our results are not driven by mechanical changes in household size or composition.²¹

Our key measure of migration is an indicator for whether a household member spent time working outside of their home county. While our identification strategy focuses on cross-province migration, we do not observe the exact location of migrants' work in our primary dataset so we cannot distinguish migration within the province from migration across provinces or international borders.²² While this adds noise to our measure of migration, it does not invalidate our identification strategy. It is important to note that the rate of interprovincial migration is fairly high. According to the 2010 census data, interprovincial migrants constitute 50 percent of internal migrants in China (Liang 2012).

The base rate of migration in this population is not trivial: in the first year that they appear in the data, about 16 percent of households have a migrant. Households experience on average 30 days of a migrant working away from home. This includes the 84 percent of households who reported zero days of migration. Conditional on positive days of migration, the mean is 184 days.²³

Total consumption is 508 RMB (or US\$64 at market exchange rates) per person in the base year.²⁴ We have several measures of household consumption in RMB. One subcategory of total consumption is food consumption. Representing about 50 percent of total consumption, food consumption is broken down into staple and non-staple, where staple food includes corn, wheat, rice, and beans, and non-staple food includes vegetables, meat, seafood, oil, sugar, wine, and condiments. About 56 percent of the value of food consumption is on non-staple items.

The average per capita agricultural income in the sample (in the base year) is 2,847 RMB (US\$356) per year.²⁵ Agricultural income makes up about 46 percent of the total income.²⁶ Agricultural labor input is measured as the number of days that all of the members of the household and hired labor work in agricultural production divided by the number of workers in the household. The average is 167 days per worker. The number of household laborers averages 2.4 workers and does not include the migrants who are away from home at the time of the survey. The average

²¹This is as expected since, as discussed below, we do not see large differences in the household structure of migrant-sending and other households (see online Appendix Table A1).

²²To address the limitation that we do not know the destination of the migrant in the main NFP data, we supplement our analysis with two datasets in which migrants report both their origin and destination provinces: the China Household Income Project (CHIP) in 2002 and the NFP from 2010 to 2012. We discuss these results in online Appendix Section 1.

²³While partial-household migration is relatively common, total-household migration, as measured by attrition from our dataset, is very rare at 0.81 percent per year. If an entire family migrates in this period, they risk losing their land in the village, which may explain why this is a rare event.

²⁴Over the year, households kept daily diaries of their consumption, and the measures represent annual consumption of the household. This figure, which is lower than total income (discussed below), is likely to be a significant underestimate of true consumption because the NFP used procurement prices to value agricultural products that are both produced and consumed by the household (Benjamin, Brandt, and Giles 2005). While we do not have data on procurement prices for our full sample period to adjust our regressions, we analyze this issue using data on prices collected from provincial statistical yearbooks for 1995 to 2000. Online Appendix Table A2 demonstrates that ratio of procurement to market prices does not move significantly with our two sources of variation. Thus, this issue does not introduce bias into our estimates of the effects on consumption.

²⁵Agricultural income includes products that the household consumes; they are asked to estimate the value based on the quota price of the products.

²⁶This includes income from crops and plantation as well as from animal husbandry and fruits, pods, and tea.

household owns about 1,080 RMB of nonproductive assets per worker.²⁷ The average household owns 471 RMB of agricultural assets per worker and 98 RMB of industrial assets per worker.

We denote by “fruits” a bundled category, including orchard fruits, pods, and tea.²⁸ At baseline, 22 percent of households spent positive labor days on orchard fruits, pods, and tea. Conditional on participating in these activities, the number of days per worker that are spent on fruits is 29 days per year. Unconditional on participation, this number drops to 5.6 days per worker per year. Households earn an average of 120 RMB per worker per year in this category.

A majority (71 percent) of households worked in animal husbandry in their first year in the survey. Conditional on participation in these activities, households spend an average of 56 days per worker in this category. Unconditional on participation, the corresponding average is 42 days. Corresponding to the patterns in labor, households earn substantially more on average from animal husbandry than from fruits, pods, and tea; they earned 699 RMB per worker from animal husbandry.

Education is an indicator variable for whether the head of household has a middle school education or higher. Approximately half (47.5 percent) of household heads have at least this level of education.

We present the summary statistics broken down by households who ever have a migrant or not in online Appendix Table A1. Compared to those who never have a migrant, households who have a migrant between 1995 and 2002 are slightly better off in terms of income, assets, and consumption in the first year that they appear in the data; this emphasizes the need for exogenous variation to identify the effects of migration opportunities. We do not observe notable differences in household structure.

B. *Sent-Down Youth Flows*

For data on inter-province sent-down flows, we use data in the publication “Statistics on sent-down youth in China,” compiled by the Sent-down Youth Office of the State Council of China in 1983. We collect inter-province sent-down information for all of the provinces in our sample. We use the total number of people sent from one province to another across time: our measure of SDY flows is time-invariant. Panel A of online Appendix Table A3 shows the total sent-down youth flows to the provinces in the NFP dataset aggregated over the sent-down youth period.

Our identification strategy relies on the idea that historical migration flows associated with the temporary relocation of urban youth to rural areas created lasting linkages across provinces. These lasting linkages can occur for several reasons, including the maintenance of networks created during the sent-down period and the transmission of information or attitudes about particular places. Online Appendix Section 1.1 uses data with destination and origin of migrants to show that the sent-down flows between provinces predict interprovincial migration several decades later. This

²⁷This category includes items like bicycles, furniture, and electronics but does not include real estate. To be included, a durable asset must have at least two years of life and be valued above 50 RMB, or about US\$6.

²⁸As we show in Table 10, fruit cultivation as well as animal husbandry are relatively risky activities.

provides support for the idea that the SDY program created lasting linkages and emphasizes the suitability of using SDY flows in our identification strategy.

C. Hukou Reforms

For our main analysis, we focus on two pull factors affecting the returns to migration. One is *hukou* reforms that occur in the provinces from which the sent-down youth originated. To compile data on the timing of each province's *hukou* reforms, we used an algorithm with specific combinations of keywords to methodically search through several databases that cover local laws and regulations in China. We focused on city-level reforms that would affect migrants from rural areas.²⁹ See Appendix Section A1 for more details on our algorithm for coding these reforms. Chinalawinfo, maintained by the Law School of Peking University, provides the most comprehensive coverage of local laws and regulations, covering about half a million local laws and regulations in China since 1949. We cross-checked three other data sources for local laws and regulation rules in China for completeness. Appendix Table A1 details the reforms.

To check the quality of our *hukou* coding strategy, we cross-check our coding of the reforms by comparing the results of our algorithm with reforms identified in Sun, Bai, and Xie (2011). They code the *hukou* reforms using *Baidu* (a Chinese search engine similar to Google), and one of the databases that we use, Chinalawinfo. Over the period in which our analyses overlap, 1998 to 2002, and for the provinces that overlap, our algorithm yields 100 percent of the provincial reforms that they identify. We find three additional reforms.

D. Migrant Labor Demand Shocks

Building on Card and Lewis' (2007) work on Mexican migration to the United States, we use local labor demand shocks in destination provinces as a pull factor for migrant labor. More specifically, we focus on GDP across two sectors—manufacturing and construction—in a destination province. We focus on these two sectors because, according to data from the National Bureau of Statistics (2013), they are the top two industries in which rural migrants are employed. Thus, the level of economic activity in these two sectors is a measure of the potential demand for migrant labor in a given province, and hence the attractiveness to migrants of that province. We construct this variable for each province-year corresponding to our primary dataset.³⁰

Our construction of labor demand shocks differs slightly from Card and Lewis' primarily for data reasons. Using US census data, they construct the labor demand shock at the city level using first-differences over a ten-year period. However, we have province-level manufacturing and construction output information every year,

²⁹Thus, reforms that targeted a very narrow population, such as individuals with PhDs, were omitted.

³⁰To avoid confusion with the aggregate GDP measure we use elsewhere in the paper (see Section VA), we will refer to this measure as "manufacturing and construction output."

so we construct our shock as deviations from the long-run average.³¹ Second, Card and Lewis use employment as their demand measure, but official employment statistics in China have been found to be lower quality than official GDP statistics (Feng, Hu, and Moffitt 2015), so we use manufacturing and construction output instead of manufacturing and construction employment.

One possible concern is that there may be an endogeneity issue with this measure. For example, the arrival of migrants could lower the cost of labor and spur growth. Card and Lewis address this by using lagged values to instrument for contemporaneous ones, in addition to the contemporaneous labor demand measure. The results in our paper do not change when we used lagged measures, so for parsimony, we focus on the contemporaneous measure.³²

IV. Identification and Estimation

Our identification strategy isolates exogenous variation in barriers and returns to migration from province p using *hukou* reforms and labor demand shocks in provinces s , which had previously sent SDY to province p . Thus, we exploit the interaction of cross-sectional variation resulting from the fact that SDY-recipient provinces received SDY in different magnitudes and from different destinations, and time variation resulting from *hukou* reforms and labor demand shocks in SDY-sending provinces. As a result, we are able to include both province and year fixed effects in our estimates. These allow us to flexibly control for many potential confounds. For instance, time-invariant ties between an SDY recipient province and its sending province(s) will be absorbed into the fixed effect for the recipient province. Relatedly, if SDY from s were sent to p because of preexisting cultural or transportation links between s and p , any direct effect of these links on our outcomes of interest will be absorbed. If a *hukou* reform or labor demand shock in a particular province makes it generally more attractive as a destination to migrants from all origins, this will be absorbed by year fixed effects.

A. Variation from the Hukou Reforms

We define $f_{s \rightarrow p}$ to be the historical level of SDY flows from urban areas in province s to rural areas in province $p \neq s$. Note that we are focusing on *hukou* reforms that occur in province s . We define h_{su} as an indicator for a *hukou* reform at time u in province s . The variable Z^{hukou} represents an interaction between the historical SDY flows from s to p and the contemporaneous *hukou* reforms in province s .³³ In other words, the main source of identifying variation is defined as

$$(1) \quad Z_{pt}^{hukou} = \sum_{u \leq t} \sum_s f_{s \rightarrow p} h_{su}.$$

³¹This is constructed using fixed effects rather than the first-differences used in Card and Lewis (2007).

³²We present the instrumented results in Appendix Table A3. Using the lagged measures to instrument for current growth does not substantively change Card and Lewis' estimates either.

³³Section IIIB uses two other Chinese datasets that provide information on both the origin and destination provinces of migrants to demonstrate the validity of the identification strategy.

Thus, Z_{pt}^{hukou} is a cumulative weighted sum of all the *hukou* reforms that have occurred in provinces linked to p via SDY flows up until t , where the weights are the historical SDY flows from the reforming province to p . The way that the SDY enter into the formula is motivated by the idea that SDY arriving in an area may increase the salience or information about the city from which they arrived. Alternatively, the main mechanism of the SDY may be in developing personal connections with locals. In the latter case, the appropriate formulation may be to divide the SDY flows by the population in province p to capture the idea that interactions with SDY are more frequent if they are a larger fraction of the population.³⁴

To understand the accumulation of the *hukou* reforms, consider the simplest case, when there is only one reform in year $n \leq 2002$ (where 2002 is the last year in our main dataset) among the provinces s that are linked by historical flows to p . Then the variable is equal to the quantity of SDY flows from s to p for the period from n to 2002 and 0 for the periods $t < n$. Now consider the case where in period $m > n$, there is a reform in another province s' that is also linked by SDY flows to p . In this scenario, in periods m and thereafter, the value of Z^{hukou} is the sum of the SDY flows from provinces s and s' . The key idea is that a reform in province s has a larger effect on the decision of households in province p to migrate to s if there were greater flows of SDYs, and hence stronger historical ties, between s and p .³⁵

B. Variation from Labor Demand Shocks

To construct the measure of labor demand shocks, we define d_{st} as the level of the demand shock at time t in province s . As before, $f_{s \rightarrow p}$ is the historical level of SDY flows from urban areas in province s to rural areas in province p . Note that we are focusing on demand shocks in province s . The variable Z^{demand} represents an interaction between the historical SDY flows from s to p and the contemporaneous demand shocks in province s , and is defined as

$$(2) \quad Z_{pt}^{demand} = \sum_s f_{s \rightarrow p} d_{st}.$$

Thus, Z_{pt}^{demand} is a weighted sum of labor demand shocks occurring at t in provinces linked to p via SDY flows, where the weights are the historical SDY flows from the province s to p . Consider the simplest case, when there is only one province s that is linked by historical flows to p . Then Z_{pt}^{demand} is equal to the quantity of SDY flows from s to p times the demand shock in s at t .³⁶ Now consider the case where there are two provinces, s and s' , that are linked by SDY flows to p . In this scenario, in period t , the value of Z_{pt}^{demand} is the weighted sum of the labor demand shocks in s

³⁴The estimate presented in Appendix Table A4, which uses this specification, is similar in sign and significance but larger in magnitude.

³⁵To make the construction of the identifying variation more concrete, in Appendix Section A2 we present the case of Shanxi, a province in Northwestern China, detailing the SDY flows to Shanxi and *hukou* reforms in the corresponding sending provinces.

³⁶Note that Z_{pt}^{hukou} reflects a cumulative measure of *hukou* reforms, because reforms, once in place, were not rolled back during our sample period. Z_{pt}^{demand} , however, measures the “flow” demand, not the cumulative “stock,” since only current demand reflects the ability of a migrant to work in province s at time t .

and s' , where the weights are the SDY flows from provinces s and s' . The key idea, which we test below, is that a demand shock in province s has a larger effect on the decision of households in province p to migrate to s if there were greater flows of SDYs, and hence stronger ties, between s and p .

V. The Impact of Reforms and Labor Demand Shocks on Migration

We begin by estimating the following equation of the impact of the interaction between pull factors (*hukou* reforms and labor demand shocks) in province s interacted with SDY links from provinces s to p on migration:

$$(3) \quad migrant_{ipt} = \alpha + \beta Z_{pt}^j + \gamma_i + \delta_t + \epsilon_{ipt},$$

where $migrant_{ipt}$ is a binary variable for whether the household had a migrant in the past year, Z_{pt}^j is Z_{pt}^{hukou} or Z_{pt}^{demand} (our measures of pull factors), γ_i are household fixed effects, δ_t are year indicators, and ϵ_{ipt} is the error term, clustered at the province level.³⁷ This provides the relationship between the pull factors, interacted with SDY flows, and migration. Note that SDY flows are rescaled by their conditional-on-positive mean so that a one-unit change in the key regressor corresponds to a reform in a sending province that sent the mean amount of SDY to the recipient province.³⁸

Table 2 shows the results of these regressions where the dependent variable is whether any member of the household has migrated that year.³⁹ Column 1 uses *hukou* reforms as a change in the incentive to migrate. At the mean level of SDY connections, a reform that relaxed the constraints for an individual to get an urban *hukou* in a place in which a household may have connections from the SDY program increases the probability of migration by 0.9 percentage points. This effect is significant at the 1 percent level. Column 2 uses labor demand shocks as the pull factor. A 1 standard deviation increase in output in the manufacturing and construction sectors, in a province at the mean level of SDY connections, increases the probability of migration by 1.8 percentage points, significant at the 5 percent level. Thus, both *hukou* reforms and labor demand shocks in SDY-sending provinces lead to significant changes in the likelihood that rural households in the corresponding SDY-receiving provinces will send members to migrate.

³⁷The data contain 19 provinces. For these regressions and the estimates of equations (6) and (8), in addition to presenting the standard errors clustered at the province level in parentheses, we address the small number of clusters by including the p -value from correcting the Wald statistic with a t -distribution with $G - L$ degrees of freedom, where G is the number of clusters and L is the number of invariant variables within the clusters (Cameron and Miller 2015, Angrist and Pischke 2010, and Donald and Lang 2007).

³⁸The mean is 9,874, i.e., conditional on a province in our sample receiving any SDY, the province would on average receive roughly 10,000 SDY.

³⁹The corresponding impacts on whether the entire household migrates (as measured by attrition from the survey) is very small in magnitude and not significantly different from zero. This is not surprising given that less than 1 percent of households attrite from the survey.

TABLE 2—THE IMPACT OF PULL FACTORS INTERACTED WITH SDY FLOWS ON MIGRATION

	(1)	(2)
Reform tally \times SDY flows	0.009 (0.003)	
Demand shock \times SDY flows		0.018 (0.007)
<i>p</i> -value	0.003	0.016
Observations	89,374	89,373

Notes: The dependent variable is a binary measure of whether the household has a migrant. The regressions include household fixed effects, year indicators, and a constant term. The regressors are Z^{hukou} and Z^{demand} defined in equations (1) and (2). Standard errors clustered by province are in parentheses. The *p*-value indicates the significance of the coefficient, using the $G - L$ degrees of freedom correction for number of provinces.

A. Excludability of Hukou Reforms and Labor Demand Shocks

We examine whether the timing of the *hukou* reforms and labor demand shocks may be capturing other characteristics of the provinces linked through the SDY program, rather than isolating changes in the costs and returns to migration. For instance, a threat to our identification strategy would be if the timing of *hukou* reforms in one location, such as Tianjin, was influenced by the level or growth of GDP in locations that historically sent SDY to Tianjin, such as Heilongjiang. In that case, we would misattribute the change in outcomes in Heilongjiang to increased access to migration to Tianjin, when in fact the change in outcomes of Heilongjiang's residents would have driven Tianjin's reforms, rather than the other way around.

To rule out this concern, we examine whether economic conditions in SDY-receiving provinces (such as Heilongjiang in the example above) at time $t - 1$ affect the likelihood that the locations that sent SDY (such as Tianjin in the example above) enact *hukou* reforms at time t ; or whether economic conditions in SDY-receiving provinces at $t - 1$ are predictive of labor demand shocks in locations that sent SDY at t . Specifically, we estimate the following regressions where t denotes year and p and s denote the provinces that received and sent SDY, respectively:

$$(4) \quad y_{s,t} = \alpha + \beta x_{p,t-1} \times SDY_{s \rightarrow p} + \delta_s + \delta_t + \epsilon_{pst}$$

where y is an indicator for the passage of a reform, or the labor demand shock, in province s ; $x_{p,t-1}$ is the lag of the logarithm of GDP per capita or the growth rate of GDP per capita, and $SDY_{s \rightarrow p}$ is the historical SDY flows from s to p . We include province and year fixed effects. Motivated by our identification assumption, we examine regressions where s refers to provinces that are reforming or experiencing labor demand shocks (and historically sent out SDY) and where p refers to provinces that received SDY in the past. The coefficient, β , tests whether economic conditions in year $t - 1$ in provinces p linked to s by SDY flows predict the timing of reforms and/or labor demand shocks in province s in year t . We exclude periods after the first year that an

TABLE 3—DIFFERENCES IN THE LEVEL AND GROWTH RATE OF GDP PER CAPITA

	<i>Hukou</i> reform		Demand shocks	
	SDY (1)	Own (2)	SDY (3)	Own (4)
<i>Panel A. Level</i>				
log GDP per capita × SDY flows	0.0056 (0.0140)		0.0009 (0.0005)	
log GDP per capita		0.7765 (0.3438)		5.2429 (0.6352)
Observations	75	112	140	143
<i>Panel B. Growth</i>				
Growth rate × SDY flows	-0.0154 (0.0357)		0.0084 (0.0051)	
Growth rate		-0.5568 (0.6265)		-3.8035 (1.1723)
Observations	75	112	140	143

Notes: The data for log GDP per capita is from the National Bureau of Statistics. In columns 1 and 3, the dependent variable refers to the GDP measure in the SDY-linked province. In columns 2 and 4, the dependent variable refers to the GDP measure in the province itself. The GDP measure is the logarithm of GDP in panel A and the growth rate of GDP. The regressions include year fixed effects, province fixed effects, and a constant term. In columns 1 and 2, the dependent variable is an indicator for a reform being implemented in the following year; the sample is restricted to periods that are pre-reform or the first year of a reform in the province. In columns 3 and 4, the dependent variable is the demand shock measure in the following year. Standard errors clustered by province are in parentheses.

s province implements a reform; thus, the correlation cannot reflect the impact the reform or labor demand shock on the level of growth rates of GDP per capita.

The results are presented in columns 1 and 3 of Table 3, where y is an indicator for the *hukou* reform in column 1 and the demand shock in column 3. The key regressor is the lag of the level or growth rate of GDP per capita in provinces s linked to p via SDY flows. The estimates in column 1 show that the level (panel A) and growth rate (panel B) of GDP per capita in province s are not correlated with a *hukou* reform being implemented the following year in SDY receiving provinces, denoted by p .

Turning to the labor demand shocks, column 3 of Table 3 examines whether the level of economic activity in SDY-receiving provinces is correlated with labor demand shocks in locations to which those provinces are linked through the SDY program. The level and growth rate of GDP per capita in province s do not predict the demand shock in the following year in provinces p that are linked to s via SDY flows.⁴⁰

An alternative empirical strategy would be to use labor demand shocks and *hukou* reforms in a household's own province to examine intra-province, rural-to-urban migration patterns. To consider the validity of this strategy, we estimate the following equation:

$$(5) \quad y_{s,t} = \alpha + \beta x_{s,t-1} + \delta_s + \delta_t + \epsilon_{st}.$$

⁴⁰For this reason, we do not instrument the time t demand shock with lagged values, as Card and Lewis (2007) do. However, the first stage estimate when we instrument the time t demand shock with its value at time $t - 1$ are very similar; see Appendix Table A3.

In other words, we examine whether the pull factors in a province follow economic conditions in that province. The results are presented in columns 2 and 4 of Table 3. Column 2 suggests that GDP per capita in a province is significantly correlated with the decision to pass a *hukou* reform in that same province the following year. This is perhaps not surprising; areas with more economic activity may have a greater demand for labor in urban areas and this motivates the subsequent passing of *hukou* reforms within the province. Column 4 shows, as expected, that both the level and growth of GDP per capita in province is predictive of the labor demand shock in that same province in the following year.

In sum, the use of own-province *hukou* reforms or labor demand shocks to identify the impact of access to migration would yield biased estimates because the exclusion restriction would fail. By, instead, using *hukou* reforms and labor demand shocks in other provinces, linked via past SDY flows, to identify the impact of access to migration, we avoid this failure of the exclusion restriction and are able to recover unbiased estimates.

B. Robustness Checks on SDY Flows

One concern is that the variation in the SDY flows may be correlated with other variables that drive the results. In this section, we consider whether the results are robust to including controls for distance and trade flows between provinces interacted with the two pull factors (*hukou* reforms and labor demand shocks) in the estimates of equation (3). Finally, we also consider whether there are similarities in the factor endowments of origin and destination provinces of the SDY.

If the cost of moving urban youth to the rural countryside was a key determinant of the rural location to which sent-down youth were assigned, then the greatest flows of SDY would also minimize the distances between provinces.⁴¹ We measure the distances between provinces using the road distance between the provincial capitals (based on Google maps in 2015).⁴²

The results are presented in column 2 of Table 4 where the *hukou* reforms are presented in panel A and the labor demand shocks in panel B. Using the NFP data from 1995 to 2002, the sample is limited to province-pairs for which there are positive SDY flows.⁴³ The coefficient on the interaction between the distance between provinces and the *hukou* reforms is positive but not significant. The impact of the interaction between *hukou* reforms and the sent-down flows remains positive and significant with the inclusion of the distance control. The interaction between distance and the labor demand shock is insignificant and the interaction between the labor demand shock and SDY flows remain significant. Thus, the results provide

⁴¹ It is unlikely that transportation costs were a primary determinant of where people were sent. The correlation between distance and the quantities of youth sent is low at 0.05.

⁴² These data are presented in panel B of online Appendix Table A3. We also examined other distance measures: as-the-crow-flies distance and road distance as measured by Poncet (2003). The three measures are highly correlated with correlation coefficients exceeding 97 percent; the results are very similar.

⁴³ This is a more conservative test than including values of distances in cases where SDY flows are zero. Column 1 replicates Table 2 with the subsample of province-pairs for which there are positive SDY flows.

TABLE 4—ROBUSTNESS CHECKS: THE IMPACT OF THE PULL FACTORS INTERACTED WITH SDY FLOWS WITH CONTROLS

	(1)	(2)	(3)	(4)
<i>Panel A. Hukou reforms</i>				
Reform tally \times SDY flows	0.014 (0.002) [0.000]	0.012 (0.003) [0.001]	0.013 (0.002) [0.000]	0.012 (0.004) [0.013]
Reform tally \times distance		0.001 (0.001) [0.337]		
Reform tally \times trade flows			-0.033 (0.013) [0.031]	
Time-varying sector effects	No	No	No	Yes
Observations	58,807	58,807	58,807	58,807
<i>Panel B. Demand shocks</i>				
Demand shock \times SDY flows	0.027 (0.006) [0.001]	0.019 (0.008) [0.037]	0.027 (0.005) [0.000]	0.030 (0.007) [0.002]
Demand shock \times distance		0.000 (0.000) [0.164]		
Demand shock \times trade flows			-0.000 (0.000) [0.021]	
Time-varying sector effects	No	No	No	Yes
Observations	58,806	58,806	58,806	58,806

Notes: The dependent variable is a binary measure of whether the household has a migrant. The regressions include household fixed effects, year indicators, and a constant term. The standard errors are clustered at the province level. The time-varying sector effects allow for time-varying effects of initial sectoral composition by interacting indicators for activity in three sectors (agriculture, production (including manufacturing and construction), and service) in 1995 with indicators for each year. Standard errors clustered by province are in parentheses. In square brackets, the p -value indicates the significance of the coefficient, using the $G - L$ degrees of freedom correction for number of provinces.

reassurance that variation in sent-down youth flows is not simply capturing geographic proximity.

As an alternative to distance, trade flows offer a good proxy for proximity between two provinces. We collected data on the volume of goods transported via railways between provinces from the Chinese Transportation Yearbook of 1995.⁴⁴ The estimates when we include the interaction between trade flows and the pull factors are presented in column 3 of Table 4. Interestingly, there is a significant negative impact of trade flows interacted with *hukou* reforms (labor demand shocks) on migration in panel A (panel B). More importantly for our analysis, the inclusion of these controls do not alter the magnitude or significance of the coefficients of interest on the pull factors interacted with SDY flows.

⁴⁴We also collected this data for the 1985 yearbook, which is the earliest wave of the data, and the results are essentially identical if we use 1985 data instead of 1995.

TABLE 5—ESTIMATES REMOVING THE EFFECTS OF THE PULL FACTORS BY DISTANCE

	No bordering (1)	$\geq 1,000$ km (2)	$\geq 2,000$ km (3)
<i>Panel A. Hukou reforms</i>			
Reform tally \times flows	0.010 (0.003)	0.010 (0.002)	0.013 (0.004)
<i>p</i> -value	0.001	0.000	0.002
Observations	89,374	89,374	89,374
<i>Panel B. Demand shocks</i>			
Demand shock \times flows	0.022 (0.006)	0.024 (0.006)	0.027 (0.007)
<i>p</i> -value	0.001	0.001	0.001
Observations	89,373	89,373	89,373

Notes: The dependent variable is a binary measure of whether the household has a migrant. The regressions include household fixed effects, year indicators, and a constant term. The time-varying sector effects allow for time-varying effects of initial sectoral composition by interacting indicators for activity in three sectors (agriculture, production (including manufacturing and construction), and service), in 1995 with indicators for each year. Standard errors clustered by province are in parentheses. The *p*-value indicates the significance of the coefficient, using the $G - L$ degrees of freedom correction for number of provinces.

To address the possibility that the results (particularly on labor demand shocks) are driven by factor endowments in origin or destination locations, we also include controls for the sectoral composition of origin and destination provinces at baseline (1995) and interact those with dummies for each year. As shown in column 4 of Table 4, the results are similar.

Finally, we consider whether the results are driven by spatial correlations in the labor demand shocks or *hukou* reforms. To do this, we restrict our attention to labor demand shocks and reforms in regions s that are not bordering regions p in column 1 of Table 5. Next, we exclude shocks and reforms where the provincial capitals are less than 1,000 kilometers (km) and 2,000 km apart, in columns 2 and 3, respectively. Reassuringly, the magnitude and significance of the estimated impact on migration are very similar to our main estimates.⁴⁵

C. Alternative Specifications for the Hukou Reforms

We consider several alternative methods for constructing the regressor that interacts SDY flows and the *hukou* reforms. First, rather than accumulating the effects of additional reforms in a province s , we consider an alternative specification where we look before and after the first *hukou* reform in province s . The coefficients, reported in column 1 of Appendix Table A5, are larger in magnitude than the main estimates and statistically significant at the 1 percent level.

⁴⁵ Note that the sample size remains the same as in Table 2. This is because we are excluding shocks and reforms occurring in nearby provinces, rather than dropping observations.

Next, we look at whether there is an increase in the impact of each incremental change in *hukou* reforms over time by interacting Z^{hukou} with the years since the last reform. In column 2 of Appendix Table A5, we see that each reform corresponds to a 1.3 percent increase in the probability of migration in the year immediately following the reform (for a mean level of SDY connections). Furthermore, the effects do increase over time, with the probability of migration associated with a reform going up 1.8 percent each additional year after at a mean level of SDY connections. Both estimates are significant at the 1 percent level.

Finally, we examine two specifications where we add one and two year leads in the interaction between the reforms and the SDY flows. The coefficients on the leads in columns 3 and 4 are small in magnitude and not significantly different from zero. This provides additional support for the idea that the *hukou* reforms are exogenous to pre-reform migration trends in SDY-linked provinces.

VI. Main Results

To examine the impact of changing incentives to migrate on the consumption, income, and investment of non-migrating household members, we estimate specifications of the form:

$$(6) \quad y_{ipt} = \alpha + \beta Z_{pt}^j + \gamma_i + \delta_t + \epsilon_{ipt}$$

where y_{ipt} is an outcome of interest, as before γ_i and δ_t are household and year fixed effects, respectively, and j is either *hukou* or *demand*: Z_{pt}^{hukou} is the reform tally weighted by SDY flows, and Z_{pt}^{demand} is the demand shock weighted by SDY flows. We trim the bottom and top 1 percent of outliers of the dependent variables. The results are all very similar without trimming the outliers.⁴⁶

For the main results, we focus on the reduced-form estimates because there is the potential for effects stemming from *hukou* reforms that operate through the expectation of migrating in the future.⁴⁷ For example, a household anticipating the ability to use migration as an ex post risk smoothing activity in the future (as in Morten 2015) might begin to reduce precautionary buffer stocks or increase risky, high-return investments in advance of actually sending a migrant. There could also be knowledge spillovers from migrants to other households in their community. There may also be effects of sending a migrant that persist after the migrant has returned, due, e.g., to changes in wealth, credit access, or information. Our reduced form effects will capture expectation and spillover effects as well as the direct effects of migration; thus, these reduced-form estimates capture the effect of increased access to migration.

⁴⁶These are available upon request from the authors.

⁴⁷The IV estimates are presented in online Appendix Tables A8 to A12 and discussed in Section VIF.

TABLE 6—ESTIMATES OF MIGRATION INCENTIVES ON THE LEVEL AND CHANGE IN CONSUMPTION

	log total consumption		log food consumption		log non-staple food	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Level of consumption</i>						
Reform tally \times SDY flows	0.013 (0.009)		0.013 (0.006)		0.014 (0.010)	
Demand shock \times SDY flows		0.017 (0.020)		0.013 (0.016)		0.006 (0.022)
<i>p</i> -value	0.149	0.412	0.058	0.421	0.166	0.773
Observations	87,458	87,455	87,496	87,493	87,497	87,494
<i>Panel B. Variability of consumption (absolute value first differences)</i>						
Reform tally \times SDY flows	-0.004 (0.004)		-0.010 (0.003)		-0.016 (0.003)	
Demand shock \times SDY flows		-0.005 (0.012)		-0.024 (0.005)		-0.044 (0.005)
<i>p</i> -value	0.319	0.660	0.003	0.000	0.000	0.000
Observations	74,221	74,221	74,218	74,218	74,214	74,214
<i>Panel C. Variability of consumption (indicator for drops > 15%)</i>						
Reform tally \times SDY flows	-0.007 (0.003)		-0.008 (0.002)		-0.013 (0.003)	
Demand shock \times SDY flows		-0.012 (0.008)		-0.022 (0.003)		-0.036 (0.008)
<i>p</i> -value	0.013	0.165	0.000	0.000	0.001	0.000
Observations	75,910	75,909	75,910	75,909	75,910	75,909

Notes: The dependent variables are per capita measures of consumption. The regressions include household fixed effects, year indicators, and a constant term. The standard errors are clustered at the province level. The *p*-value indicates the significance of the coefficient, using the $G - L$ degrees of freedom correction for number of provinces.

A. Consumption

We begin by considering the impact of changes in the costs of and returns to migration on the level and variability of consumption. Panel A of Table 6 examines effects on the log consumption.⁴⁸ Columns 1 and 2 indicate that total consumption increases by 1.3 percent and 1.7 percent in response to increased incentives to migrate via *hukou* reforms and labor demand shocks, respectively. However, the effects are not significant. Food consumption also increases by 1.3 percent, using either source of variation; this is statistically significant at the 10 percent level for *hukou* reforms (column 3) but not for labor demand shocks (column 4). Non-staple food consumption increases by a similar magnitude, however the effect is not significant at the standard levels (columns 5 and 6).

⁴⁸Throughout, the coefficient estimates the effect of one additional reform in a province s that sent the mean amount of SDY to province p , or the effect of a one standard deviation increase in construction and manufacturing output in an s province that sent the mean amount of SDY to province p ; however, for ease of exposition, we refer to these as the effect of “incentives to migrate” or “opportunities to migrate.”

For risk-averse households, the level of consumption is not a sufficient statistic to describe the effect on welfare; households also care about the variability of consumption. Panel B of Table 6 shows effects on consumption variability, defined as the absolute change relative to the previous year, $|\log(c_{it}) - \log(c_{i,t-1})|$. The variability of total consumption falls, but the effect is not significant. However, when we turn to log food consumption (a category of consumption that households may particularly value smoothing), there is a significant reduction in variability associated with access to migration: year-to-year food consumption changes are reduced in absolute magnitude by 1 percent and 2.4 percent in response to increased incentives to migrate via *hukou* reforms and labor demand shocks respectively, and these estimates are both significant at the 1 percent level. Consumption volatility for non-staple food is reduced by 1.6 percent following *hukou* reforms and by 4.4 percent in response to labor demand shocks. These estimates are also significant at the 1 percent level.

The ability to migrate ex post and/or receive remittances from migrants may be especially important in response to significant negative shocks, when marginal utility is particularly high. In panel C of Table 6, we examine whether access to migration reduces the likelihood of large consumption drops defined as drops greater than 15 percent. Column 1 shows that a large drop in total consumption is 0.7 percent less likely when households have increased access to migration via *hukou* reforms and this is significant at the 5 percent level. The effect for labor demand shocks is similar in magnitude but not significant (column 2). Columns 3 and 4 show that a large drop in food consumption is 0.8 percent and 2.2 percent less likely when households have increased access to migration via *hukou* reforms and labor demand shocks, respectively. These estimates are both significant at the 1 percent level. Columns 5 and 6 show that a large drop in non-staple food is 1.3 percent less likely using *hukou* reforms and 3.6 percent less likely using labor demand shocks. Again, these are both significant at the 1 percent level.

B. Income, Labor, and Assets

Panel A of Table 7 examines effects on the level and variability of income earned by nonmigrants. Columns 1 and 2 report the effect on the log of agricultural income. Agricultural income does not decrease; indeed, the point estimates of the change in income in response to increased incentives to migrate via *hukou* reforms (labor demand shocks) are 0.12 and 0.05, respectively, though only the latter is significant at the 10 percent level. Columns 3 and 4 examine the effect on nonagricultural sources of income. The effects are not significantly different from zero. The lack of any evidence of a significant drop in income is informative about the marginal return to household assets and labor, a point we return to below.

Panel B of Table 7 examines the effect of migration incentives on income variability, defined as the absolute change relative to the previous year, $|\log(y_{it}) - \log(y_{i,t-1})|$. The results for increased access to migration via *hukou* reforms show that the variability of agricultural income increases by 1.4 percent (column 1); the variability of nonagricultural income falls by 1.5 percent (column 3). Both effects are significant at the 10 percent level or lower. Using variation from

TABLE 7—ESTIMATES OF MIGRATION INCENTIVES ON THE LEVEL AND CHANGE IN INCOME

	Agricultural income		Nonagricultural income	
	(1)	(2)	(3)	(4)
<i>Panel A. Level of income</i>				
Reform tally \times SDY flows	0.012 (0.012)		0.012 (0.013)	
Demand shock \times SDY flows		0.050 (0.027)		-0.029 (0.025)
<i>p</i> -value	0.345	0.086	0.384	0.262
Observations	72,524	72,523	72,457	72,457
<i>Panel B. Variability of income (absolute value first differences)</i>				
Reform tally \times SDY flows	0.014 (0.008)		-0.015 (0.008)	
Demand shock \times SDY flows		0.001 (0.015)		-0.032 (0.018)
<i>p</i> -value	0.098	0.931	0.064	0.085
Observations	60,086	60,086	59,988	59,988
<i>Panel C. Variability of income (indicator for drops > 15%)</i>				
Reform tally \times SDY flows	-0.002 (0.004)		-0.003 (0.004)	
Demand shock \times SDY flows		-0.010 (0.009)		-0.007 (0.009)
<i>p</i> -value	0.639	0.285	0.412	0.447
Observations	75,910	75,909	75,910	75,909

Notes: The dependent variables are the log of per capita measures of income. The regressions include household fixed effects, year indicators, and a constant term. The standard errors are clustered at the province level. The *p*-value indicates the significance of the coefficient, using the *G* – *L* degrees of freedom correction for number of provinces.

labor demand shocks indicates a positive but insignificant effect on the variability of agricultural income (column 2), and a significant decrease in the variability of non-agricultural income (column 4). Panel C of Table 7 examines the effect of incentives to migrate on large drops in income (greater than 15 percent). No significant effects are seen. Overall, the results for the level and variability of income do not follow the effects on consumption; this suggests that the impact of opportunities to migrate on consumption and consumption smoothing do not operate through the changes in the earned income of remaining household members.

We next examine the effect of migration opportunities on the amount of labor used in household activities. Columns 1 and 2 of Table 8 examine the effect on agricultural labor inputs, defined as the number of days that all of the members of the household and hired labor work in agricultural production divided by the number of workers in the household. This is a measure of the intensity of labor inputs, scaled by the worker population of the remaining household. The magnitude of the estimated effect is small and not significant. Columns 3 and 4 of Table 8 test whether migration has an effect on the number of household laborers, excluding those working as migrants, in levels. There is a small, negative, but insignificant, effect associated

TABLE 8—ESTIMATES OF MIGRATION INCENTIVES ON LABOR

	log agricultural labor inputs		Number of household laborers	
	(1)	(2)	(3)	(4)
Reform tally \times SDY flows	0.001 (0.009)		-0.003 (0.008)	
Demand shock \times SDY flows		0.019 (0.019)		-0.016 (0.025)
<i>p</i> -value	0.878	0.316	0.718	0.531
Observations	72,528	72,527	72,614	72,612

Notes: The regressions include household fixed effects, year indicators, and a constant term. The standard errors are clustered at the province level. The *p*-value indicates the significance of the coefficient, using the *G* - *L* degrees of freedom correction for number of provinces.

TABLE 9—ESTIMATES OF MIGRATION INCENTIVES ON ASSETS

	Nonproductive assets		Agricultural assets		Nonagricultural assets	
	(1)	(2)	(3)	(4)	(5)	(6)
Reform tally \times SDY flows	-0.037 (0.007)		-0.040 (0.015)		-0.027 (0.009)	
Demand shock \times SDY flows		-0.058 (0.013)		-0.066 (0.035)		-0.042 (0.022)
<i>p</i> -value	0.000	0.000	0.016	0.080	0.008	0.066
Observations	72,570	72,567	72,739	72,736	34,401	34,399

Notes: The dependent variable is the log of assets. The regressions include household fixed effects, year indicators, and a constant term. The standard errors are clustered at the province level. The *p*-value indicates the significance of the coefficient, using the *G* - *L* degrees of freedom correction for number of provinces.

with access to migration. This result may not be that surprising, given that Table 1 indicated that migrants spend only half of the year away.

Table 9 examines effects of migration opportunities on assets. We examine non-productive assets (such as televisions and bicycles), agricultural assets (such as animals and farm equipment), and nonagricultural productive assets. All three categories of assets exhibit significant declines. Nonproductive assets fall by 3.7 percent (approximately 40 RMB at the mean level) and 5.8 percent (63 RMB) using the *hukou* reforms and labor demand shocks, respectively (both significant at the 1 percent level). Agricultural assets fall by 4 percent (approximately 18 RMB) and 6.6 percent (31 RMB) using the *hukou* reforms and labor demand shocks, respectively (both significant at the 10 percent level or lower). Nonagricultural assets fall by 2.7 percent (approximately 3 RMB) and 4.2 percent (4 RMB) using the *hukou* and labor demand shocks, respectively (both significant at the 10 percent level or lower).

The significant drop in productive assets, combined with the fact that we do not observe a corresponding fall in income, suggest that the liquidated assets were earning a low or zero return. We do not see an increase in labor use either, which suggests that the fall in assets is not explained by a shift away from capital-intensive activities towards labor-intensive activities. Thus, households may be holding these

low-return assets as buffer stocks (Deaton 1991; Anagol, Etang, and Karlan 2017), which do not contribute significantly to household productivity but could be liquidated in response to a negative shock that could not otherwise be smoothed. Households may then optimally liquidate these assets when they gain access to the consumption smoothing technology provided by access to migration. Alternatively, the fall in assets may be a cause rather than a consequence of migration; migration may be costly and financed by the liquidation of low-yielding assets. We next look for another sign that access to migration allows households to diversify: increased investment in high-risk, high-return activities.

C. Investment in Risky Activities

A corollary of households receiving better access to smoothing strategies via increased opportunities for migration is that the household can move along the risk-return frontier to invest in assets and activities that have a higher expected return, but are riskier. We examine two high-risk activities: growing fruits (orchard fruits, pods, and tea), and raising animals. Tea and orchard fruits are typically cash crops (Qian 2008), subject to fluctuations in the market price. Animals can also be expected a priori to be risky, as they are frequently sold at variable market prices and are subject to disease risk (Cai et al. 2015).

We also directly confirm in our NFP data that these activities are high risk; this implies that they must yield high returns to be held in the household portfolio alongside lower-risk investments.⁴⁹ Table 10 shows the coefficient of variation (CV) for total agricultural income, nonagricultural income, fruit income, and animal income. Panel A shows unconditional CVs and panel B shows within-household CVs.⁵⁰ Unconditionally, fruit and animal income have CVs of 6.2 and 6.7, respectively, compared to 1.3 for total agricultural income and 3.4 for nonagricultural income. Looking within households, the CVs fall because cross-household variation is removed, but the pattern remains the same: fruit income has a CV of 1.9 and animal income a CV of 1.2, while for total agricultural income the figure is 0.64 and for nonagricultural income it is 0.76. Thus, both measures suggest that there is at least twice as much income volatility in the fruit and the animal production categories as compared with total agricultural production and nonagricultural activities.

Panel A of Table 11 shows that households increase their investment in these high-risk activities in response to migration access. The dependent variable is the logarithm of the number of labor days a household expends in these activities plus one. We observe investment of labor in the form of person-days spent working on each type of activity. Column 1 shows that reductions in the barriers to migration lead to a significant 8 percent increase in the days worked in animal husbandry, or approximately 3.4 days per worker. Column 2 shows that an increase in the returns

⁴⁹ We do not directly estimate returns for these activities because we do not observe capital for animals and fruits separately from other household activities.

⁵⁰ Whether the cross-sectional or the within-household estimates is more informative for the amount of risk households face depends on whether persistent variations across households are ex ante forecastable. If not, these represent risk and the unconditional CV is informative, while if persistent variations are forecastable, the within-household CV is more informative (see Ligon 2010).

TABLE 10—COEFFICIENT OF VARIATION BY INCOME CATEGORIES

	Agricultural income (1)	Nonagricultural income (2)	Fruit income (3)	Animal income (4)
<i>Panel A. Unconditional CV</i>				
Coefficient of variation	1.335	3.415	6.189	6.723
Observations	91,193	91,193	91,193	91,193
<i>Panel B. Within household CV</i>				
Coefficient of variation	0.641	0.758	1.855	1.213
Observations	12,163	12,207	5,341	11,144

Notes: The coefficient of variation is the standard deviation divided by the mean. In panel A, it is calculated using the unconditional mean and standard deviation across all observations in the data. In panel B, it is calculated using the mean and standard deviation within households for households that have at least two years of positive income in the category.

TABLE 11—ESTIMATES OF MIGRATION INCENTIVES ON LABOR AND INCOME IN HIGH-RISK ACTIVITIES

	Animal husbandry		Fruit		Low-risk agriculture	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Labor days</i>						
Reform tally \times SDY flows	0.080 (0.015)		0.038 (0.013)		-0.009 (0.015)	
Demand shock \times SDY flows		0.161 (0.036)		0.060 (0.039)		0.001 (0.034)
<i>p</i> -value	0.000	0.000	0.008	0.139	0.575	0.986
Observations	72,395	72,393	71,961	71,959	72,490	72,489
<i>Panel B. Income</i>						
Reform tally \times SDY flows	0.117 (0.027)		0.049 (0.019)		-0.022 (0.024)	
Demand shock \times SDY flows		0.219 (0.057)		0.068 (0.053)		0.034 (0.053)
<i>p</i> -value	0.000	0.001	0.020	0.217	0.377	0.523
Observations	72,309	72,307	71,914	71,912	72,758	72,757

Notes: In panel A, the dependent variable is the logarithm of the number of days in that activity plus one. In panel B, the dependent variable is the logarithm of income plus one. The regressions include household fixed effects, year indicators, and a constant term. The standard errors are clustered at the province level. The *p*-value indicates the significance of the coefficient, using the *G* - *L* degrees of freedom correction for number of provinces.

to migration increases the days that households work in animal husbandry by 16 percent, or 7.6 days per worker (significant at the 1 percent level).

Columns 3 and 4 of Table 11 show the corresponding results for fruits. An additional *hukou* reform in an average SDY-linked province corresponds to a 3.8 percent increase in the time allocated to fruit cultivation (corresponding to approximately 0.2 additional days per worker), significant at the 1 percent level. A standard deviation increase in labor demand in an average SDY-linked province corresponds to

a 6 percent increase (or approximately 0.33 additional days per worker) in the time allocated to fruit cultivation, but this estimate is not significant at the standard levels.

Columns 5 and 6 of Table 11 report effects on less risky agricultural activities (i.e., agriculture other than animal husbandry and fruits). Unlike the results for high-risk agricultural activities, those for less risky agricultural activities are very small in magnitude and not statistically different from zero, illustrating that the effects seen in columns 1 to 4 are indeed concentrated in high-risk agricultural activities.

Finally, in panel B of Table 11, we examine the effect of migration on income from animal husbandry and fruits. The dependent variable, $\log(\text{income} + 1)$, captures both intensive and extensive margins. Consistent with the positive effects on days worked in animal husbandry, we see significant increases in income from animal husbandry of 12 percent in column 1 and 22 percent in column 2. These estimates are both significant at the 1 percent level. The effect on income from fruits in column 3 is 5 percent, significant at the 5 percent level. The impact on fruit income using the demand shock variation (column 4) is positive but not significant at the standard levels. As was seen with labor investments, the corresponding effects on income from less risky agricultural activities are small and statistically insignificant.

Overall, the results indicate that both investment in and income from high-risk activities increase due to migration opportunities (and that lower-risk agricultural activities do not show a similar effect), consistent with households reallocating their portfolios toward higher-risk, higher-return activities in response to the insurance provided by the option of sending migrants.

D. Quantitative Assessment of Precautionary Channel

The changes in household behavior observed in response to increased access to migration are qualitatively consistent with migration acting as an option that reduces income risk and, thus, reduces the need for precautionary savings. In this section, we assess whether the results are also quantitatively consistent with a precautionary channel.

Assume that households have CRRA utility with relative risk aversion σ and time preference rate δ , and let r be the net interest rate. As in Dynan (1993), by performing a second-order Taylor expansion of $u'(c_{t+1})$ around c_t , we can approximate the Euler equation $u'(c_t) = \frac{1+r}{1+\delta} E_t[u'(c_{t+1})]$, as follows:

$$(7) \quad E_t[\Delta \log(c_{t+1})] = \frac{1}{\sigma} \frac{r - \delta}{1+r} + \frac{1+\sigma}{2} E_t[(\Delta \log(c_{t+1}))^2].$$

Assuming that pull factors do not impact the interest rates faced by households, but offer an option which reduces the volatility of consumption growth, we can obtain an estimate of $\frac{1+\sigma}{2}$ by comparing the coefficients on pull factors interacted with SDY flows for consumption growth and the corresponding coefficient for squared consumption growth.⁵¹

⁵¹ A more recent paper to use this method is Fagereng, Guiso, and Pistaferri (2017), who find strong precautionary motives using Norwegian data, with $\sigma \approx 1$.

We first estimate our main empirical specification, equation (6), with $\Delta \log(c_{i,t+1})$ as the dependent variable. Panel A of online Appendix Table A7 presents the results. We can reject that there is no effect of the pull factors (interacted with SDY flows) on food consumption growth: the coefficients are negative and significant at the 1 percent level. The negative relationship is consistent with migration opportunities providing income risk reduction and, therefore, reducing precautionary savings motives.

Similarly, we estimate equation (6) with the square of log-consumption growth as the dependent variable. Again, the estimated coefficients are negative (and significantly different from zero at the 1 percent level), as expected (online Appendix Table A7, panel B). These results can be plugged into equation (7) to estimate σ . The implied parameter estimates are at the bottom of online Appendix Table A7. The results using *hukou* reforms imply a value of $\sigma = 0.54$ and the results using manufacturing and construction output imply a value of $\sigma = 1.12$, both of which are consistent with reasonable parameters for the utility function and similar to the value found by Fagereng, Guiso, and Pistaferri (2017). This suggests that the results are quantitatively as well as qualitatively consistent with the proposed mechanism of migration affording increased options for self-insurance and thus reducing the need for precautionary savings.

E. Alternative Explanations

Labor Market Frictions.—Another possible mechanism through which having a migrant affects the production decision of households is through the loss of a laborer. This may be important if rural labor markets are incomplete and households cannot hire labor to fully replace the lost labor supply of the migrant. However, the fact that we do not see a significant drop in labor used in households with a migrant (online Appendix Table A9) suggests that this mechanism is not first order in our setting. Of course, there is the possibility that the units of labor used (in worker days or in workers) doesn't reflect the fact that the labor that replaces the migrant is different in terms of quality if not quantity. However, the lack of a significant drop in earnings corresponding to migration (online Appendix Table A10) suggests that the labor quality is not much lower.

Decline in Aggregate Volatility.—We also consider the idea that the mechanism for the effects of increased access to migration is at least in part due to a general equilibrium decline in aggregate volatility (Jayachandran 2006). To formally test this, we examine whether our measures of increased migration access led to declines in the volatility of aggregate (village-level) wages.⁵² To construct aggregate wages, we use household reports on their expenditures on hired labor divided by the number

⁵² Another possibility is that increased migration flows led to increased information flows about prices, and, as a result, markets become more integrated with a resulting drop in price volatility. We have examined whether *hukou* reforms are associated with reductions in the dispersion of prices between SDY-sending and receiving provinces, and find no effect. Results are available from the authors on request.

of days that they employed hired labor.⁵³ Our first measure is the coefficient of variation across households. Following the analysis on consumption and income volatility, we also look at the absolute value of the first difference of the logarithm of the village-level wage rate and an indicator for drops over 15 percent. As shown in Appendix Table A6, there is no significant relationship between access to migration and any of the measures of the volatility of aggregate wages. Thus, the results do not support the idea that the results are explained by a decline in aggregate volatility.

F. Instrumental Variables Estimates

Estimates of the effects of increased migration opportunities for households that respond directly with an individual migrating may also be of interest. We present instrumental variables estimates of the form:

$$(8) \quad y_{ipt} = \alpha + \beta \text{migrant}_{ipt} + \gamma_i + \delta_t + \epsilon_{ipt},$$

where migrant_{ipt} , the indicator for sending a migrant, is instrumented with Z_{ipt} . For Z_{ipt}^{hukou} , this relies on the assumption that all of the effects of the *hukou* reforms in areas with SDY connections operate through the migration of a household member. Similarly, for Z_{ipt}^{demand} , the assumption of the IV estimates is that labor demand shocks in areas with SDY linkages operate solely through migration. As discussed above, this assumption will fail if anticipation or spillover effects are present; as such, the IV estimates are likely to be upward-biased to the extent they attribute all effects of migration access and returns to household-year observations when a household sends a migrant. The corresponding estimates are presented in online Appendix Tables A8 to A12. The signs and levels of significance are quite similar across the reduced form and IV estimates. The magnitudes of some of the IV estimates are perhaps too large to be plausible; this suggests that the exclusion restriction may not hold due to anticipation or spillover effects.

An advantage of the IV estimates is that we can compare the magnitudes of the effects using the two sources of variation with each other. Interestingly, the estimates are quite similar using variation from *hukou* reforms and from labor demand shocks. In virtually all cases, the two IV estimates for a given outcome variable are of the same sign and not significantly different from each other.

VII. Discussion and Conclusion

Our paper presents a new identification strategy for studying migration in China that exploits variation from multiple sources. We use preexisting ties between provinces arising from the sent-down youth program interacted with time-varying policies. The first of these is reforms of the *hukou* system. We then compare the long-run changes in barriers to migration associated with *hukou* reforms to short-run labor demand shocks that alter the returns to migration. Interestingly, using variation from

⁵³ Thus, the wage measure is a wage per day rather than per hour. We combine contract and temporary hired workers, but the results are similar if we construct separate wage measures for each group.

our new strategy, based on *hukou* reforms, produces results that are very similar to estimates that exploit the more commonly-used demand shock instrument for migration. One possible explanation is that a *hukou* reform in a province was received as that province being generally more tolerant of migrants, but the type of migration that responds to this change is still largely temporary or seasonal.⁵⁴ Perhaps this is not surprising given that most individuals from the poor, rural areas in the NFP survey may be unable to afford the expenditure necessary to obtain an urban *hukou* that would enable permanent migration, at least in the medium-run time frame of our analysis. Another possibility is that migrants in China frequently return home even when they have the option to migrate permanently, causing more permanent changes to access to migration to have similar effects as more transitory changes in migration access.

Our results suggest that, on net, increased access and returns to internal migration are beneficial for rural households. Food consumption becomes less variable. The findings rule out a negative wealth effect from having a migrant and rule out the possibility that the total consumption risk a household faces increases as a result of having a migrant. Furthermore, the results suggest that low-yielding assets are liquidated. The proceeds of the liquidation of the assets, potentially combined with net positive transfers from migrants, serve to increase households' cash on hand. The increased cash on hand may fund the observed increase in food consumption and the observed increase in investment in high-risk, high-return assets. An alternative interpretation of the liquidation of low-yielding assets is that they were used to finance the costly migration of a household member.

The finding of an increase in consumption following migration, an event that increases the ability of households to smooth their consumption, echoes the results of Kaboski and Townsend (2011), who study the response of Thai households to increased access to formal credit. Our finding that access to improved consumption smoothing increases investment in risky activities echoes the literature on income smoothing in developing countries (e.g., Rosenzweig and Binswanger 1993; Karlan et al. 2014; Cole et al. 2013; Emerick et al. 2014; and Carter, Laajaj, and Yang 2015). Moreover, the shock to consumption smoothing that we study here—internal migration—is notable in that there appears to be large demand for rural-to-urban migration, whereas other candidate smoothing policies such as crop or weather insurance, formal savings, and credit often appear to suffer from low demand (Cole, Giné, and Vickery 2017; Dupas et al. 2016; Banerjee, Karlan, and Zinman 2015). Nonetheless, previous evidence suggests that temporary, partial-household, internal migration appears to be suboptimally low (Bryan, Chowdhury, and Mobarak 2014). In our particular setting, the suboptimal level of rural-to-urban migration may reflect government restrictions on mobility in China, including the *hukou* policy. Our results suggest that efforts to promote internal migration are likely to benefit agricultural households.

⁵⁴ An interesting corollary may be when Chancellor Angela Merkel announced in 2015 that Germany would welcome refugees, leading to a surge of migrants from poor but not war-torn countries into Germany who would not qualify for refugee status (Gidda 2016).

APPENDIX

A. Hukou Reform Coding

We collect information on *hukou* reforms from several databases, each of which covers local and national laws, rules, and regulations in China. We search the following electronic databases that have information about local and national laws, rules, and regulations: Peking University's Chinalawinfo, Xihu Law Library (www.law-lib.com), Beijing Lawstar Tech Limited Company (www.law-star.com) and Zhengbao Online Education Company's database.

We use the following algorithm in each of the four databases to compile our data on *hukou* reforms across provinces and time. We used all combinations of the following two keywords for *hukou* and reform or administration in Chinese and searched the whole body (not just the title) of these records. The words for the *hukou* system used are *hukou* and *huji*. The words for reform or administration are: *gaige* and *guanli*.

We then examine the written description of the laws and regulations carefully to determine whether the record refers to a *hukou* reform that was issued for the first time. We focus on reforms that apply to a wide group of individuals and are likely to be relevant for the rural households in our sample. Thus, we exclude any policies that only allow a very restricted group of individuals to obtain a new *hukou*. If the document refers to a change that only targets PhDs, PhDs from Western universities, or owners of very high asset firms, we exclude these from our coding.⁵⁵ In some cases, the local government issued documents that discuss general principles of *hukou* reforms without implementing actual reform measures. These are also excluded from our analyses.⁵⁶

B. Example: Shanxi

To make the construction of the identifying variation more concrete, we discuss the case of Shanxi, a province in Northwestern China. The SDY flows to Shanxi and *hukou* reforms in the corresponding sending provinces are detailed in panel A of Table A2.

Shanxi received SDY from Beijing and Tianjin: 41,300 from Beijing and 7,300 from Tianjin. The reform and SDY interaction for Shanxi, $Z_{Shanxi,t}$ will equal 0 until 1998, when it will take the value 41,300, representing the SDY flows received from the Beijing, which implemented a reform in 1998. No additional reforms are implemented in provinces that sent SDY to Shanxi until 2002, so $Z_{Shanxi,t}$ remains

⁵⁵ For example, in 2002, the city of Beijing issued a policy document, titled "A notice on four measures to implement rules on opening further to domestic and further developing Beijing economy," that allows senior managers of large state-owned business groups and firm owners who invested at least 30 million RMB in Beijing to apply for Beijing *hukou*.

⁵⁶ For example, Sichuan provincial government issued a document in 1998 called "A note on solving several important problems in the Hukou system." The document indicates that Sichuan provincial government was thinking of doing some *hukou* policy experiment in a few cities. However, the document doesn't specify which places and when these experiments would be implemented.

TABLE A1—CITY-LEVEL *Hukou* REFORMS: 1993–2002

Province	Reform year	Description	Document name	Issue date
Beijing	1998	A migrant can get hukou in pilot satellite cities of Beijing if she buys an apartment and has a stable job.	JingZhengBanFa[1997] No.74	December 31, 1997
	2002	A migrant can get hukou in 14 satellite cities and 33 towns if she has* an apartment and a stable job.	JingZhengFa[2002] No.25	September 23, 2002
Zhejiang	1998	A migrant can get hukou in Hangzhou City, the capital of Zhejiang province, if she buys an apartment and has a stable job in Hangzhou.	HangZhengBan[1998] No.31	September 20, 1998
	2000	A migrant can get hukou in most cities in Zhejiang province (entry conditions are not specified in great detail).	ZheZheng[2000] No.7	September 1, 2000
	2002	A migrant should get hukou in most cities if she has* an apartment and also a stable job.	ZheZhengBanFa[2002] No. 12	March 29, 2002
Shanghai	1994	A migrant can get a temporary Shanghai hukou if she has* an apartment and a stable job.	Shanghai LanYin hukou Guanli Zanxing Guiding	February 1, 1994
	1998	A revision of the 1994 law by decreasing the entry bar further. [†] *	HuFuFa[1998] No.47	October 25, 1998
	2002	A migrant can apply for <Shanghai Resident Permit> if he has special skills, and this permit allows the holder to enjoy most benefits a Shanghai citizen has.	HuFuFa[2002] No. 122	April 30, 2002
Jiangsu	1995	A migrant can get a temporary city hukou if she has** an apartment in Nanjing City.	NingZhengBanFa[1995] No. 79	June 14, 1995
	2001	A migrant can get a city hukou if she has* an apartment or a stable job.	XuZhengFa[2001] No. 38	April 30, 2001
	2002	A migrant can get a city hukou if she has* an apartment or a stable job in most cities in Jiangsu province.	SuZhengFa[2002] No. 142	November 22, 2002
Shandong	1993	A migrant can get hukou in Yingkou City if she buys an apartment.	Yingkou Lanyin hukou Guanli Zanxing Guiding	December 1993
	2000	A migrant can get a city hukou in Shandong if she has* an apartment in most small- and medium-size cities.	LuZhengFa[2000] No.7	January 14, 2000
	2001	A migrant can get a city hukou in Shandong province if she has* an apartment and a stable job. This is a further reform with respect to the 2000 reform.	LuZhengFa[2001] No.107	October 10, 2001

*Renting or buying an apartment are allowed.

**Employer-provided dormitories are included.

[†]In the 1994 reform, if one wants to buy an apartment to obtain a Shanghai hukou, she has to buy at least 100 square meters; in 1998 this number was decreased to 70 square meters for Puxi and 65 square meters for Pudong. Also in 1998, Puxi was redefined to include several more remote areas: Jiading, Minhang, Baoshan, Jinshan, Songjiang, Nanhui, Fengxian, Qingpu, and Chongming.

at 41,300 until 2002. In that year Beijing implements another reform, so $Z_{Shanxi,t}$ increases to 82,600 ($41,300 \times 2$).

Tianjin did not implement any *hukou* reforms over the 1992 to 2002 time period, so the SDY flows from Tianjin to Shanxi do not enter into the construction of our instruments. Any time-invariant effects on Shanxi due to its historical ties with Tianjin will be absorbed into the fixed effect for households in Shanxi.

TABLE A2—SDY FLOWS AND *Hukou* REFORMS AFFECTING SHANXI

<i>Panel A. Provinces sending SDY to Shanxi and reform dates</i>		
Sending province	SDY to Shanxi	Hukou reform dates
Beijing	41,300	1998, 2002
Tianjin	7,300	None
<i>Panel B. Measure of access to migration for Shanxi</i>		
Year	$Z_{Shanxi,t}$	Source
1995	0	—
1996	0	—
1997	0	—
1998	41,300	Beijing
1999	41,300	—
2000	41,300	—
2001	41,300	—
2002	82,600	Beijing

TABLE A3—THE IMPACT OF LABOR DEMAND SHOCKS
(instrumented with lagged values) INTERACTED WITH SDY FLOWS

	OLS (1)	IV (2)
Demand shock \times SDY flows	0.018 (0.007)	0.017 (0.006)
<i>p</i> -value	0.016	0.013
Observations	89,373	75,904

Notes: The dependent variable is a binary measure of whether the household has a migrant. In column 2, the labor demand shock at t is instrumented with its value at $t - 1$. The regressions include household fixed effects, year indicators, and a constant term. The standard errors are clustered at the province level. The *p*-value indicates the significance of the coefficient, using the $G - L$ degrees of freedom correction for number of provinces.

TABLE A4—THE IMPACT OF PULL FACTORS INTERACTED WITH SDY
FLOWS AS A SHARE OF POPULATION ON MIGRATION

	(1)	(2)
Reform tally \times SDY/ N	0.015 (0.007)	
Demand shock \times SDY/ N		0.025 (0.014)
<i>p</i> -value	0.036	0.080
Observations	89,374	89,373

Notes: The dependent variable is a binary measure of whether the household has a migrant. The regressions include household fixed effects, year indicators, and a constant term. The standard errors are clustered at the province level. The *p*-value indicates the significance of the coefficient, using the $G - L$ degrees of freedom correction for number of provinces.

TABLE A5—ALTERNATIVE MEASURES AND SPECIFICATIONS FOR SDY-LINKED *Hukou* REFORMS ON MIGRATION

	(1)	(2)	(3)	(4)
Post reform \times SDY flows	0.036 (0.010) [0.002]			
Reform tally \times SDY flows		0.013 (0.003) [0.000]	0.019 (0.005) [0.001]	0.018 (0.006) [0.009]
Reform tally \times SDY flows \times years since reform		0.018 (0.003) [0.000]		
Reform tally _{<i>t</i>+1} \times SDY flows			0.003 (0.004) [0.522]	0.003 (0.005) [0.559]
Reform tally _{<i>t</i>+2} \times SDY flows				0.000 (0.005) [0.941]
Observations	89,374	89,374	77,899	66,258

Notes: The dependent variable is a binary measure of whether the household has a migrant. The regressor in column 1 only looks at the first reform in each province (rather than accumulating each additional reform within a province). Column 2 adds an interaction of Z^{hukou} and years since the last reform. Columns 3 and 4 add lagged values of Z^{hukou} . The regressions include household fixed effects, year indicators, and a constant term. Standard errors clustered by province are in parentheses. The *p*-values, in square brackets, indicate the significance of the coefficient, using the *G* – *L* degrees of freedom correction for number of provinces.

TABLE A6—ESTIMATES OF MIGRATION INCENTIVES AND AGGREGATE WAGE VOLATILITY

	Coefficient variation		Absolute first difference		Drop over 15%	
	(1)	(2)	(3)	(4)	(5)	(6)
Reform tally \times SDY flows	–0.019 (0.023)		–0.009 (0.016)		–0.007 (0.011)	
Demand shock \times SDY flows		–0.066 (0.058)		–0.041 (0.037)		–0.005 (0.022)
<i>p</i> -value	0.423	0.271	0.574	0.283	0.515	0.832
Observations	609	609	575	575	575	575

Notes: Each observation is a village-year. The coefficient of variation is across households in the village. The regressions include village fixed effects, year indicators, and a constant term. The standard errors are clustered at the province level. The *p*-value indicates the significance of the coefficient, using the *G* – *L* degrees of freedom correction for number of provinces.

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