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Essays in Labor Economics and International Economics

A dissertation presented

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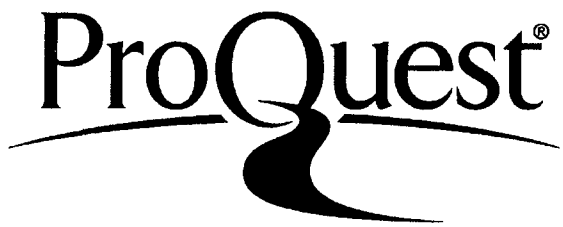

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Essays in Labor Economics and International Economics

Abstract

This dissertation consists of three essays in labor economics and international economics. The first essay, using originally collected data on a sample of 3,408 households, represents an initial attempt to evaluate the Housing Exchange Program in Jiaxing, which offers rural households the opportunity to exchange their previously untradeable homestead for urban residential property. Logit regressions point to a greater appeal of the program to economically disadvantaged households, evidencing its progressivity. Comparisons of participants vs. nonparticipants within the treatment towns suggest a positive program effect on agents' non-agricultural income and the opposite for agricultural income, induced by income variations at the intensive margin but more importantly intersectoral shift of labor. These findings are statistically and economically significant and robust to econometric tests such as IV models using distance to town center and difference-in-differences-in-differences estimation for treatment vs. control towns using propensity score matching.

The second essay, joint with Prakash Kannan, Prakash Loungani, and Bharat Trehan, studies the relationship between sectoral shocks and aggregate unemployment.

Unemployment duration has increased sharply during the Great Recession of 2007-09. Standard explanations based on the role of monetary and fiscal policies or oil shocks do not explain movements in long-term unemployment. We present evidence that sectoral shocks, which we measure using the dispersion of industry-level stock market returns, have played a significant role. These findings are robust to the inclusion of measures of uncertainty emphasized by Bloom (2009) and the extension of the sample to a broad set of advanced economies.

The third essay, joint with Bo Becker and David Greenberg, argues that by facilitating exporting firms' intangible, firm-specific investments, a developed financial system can promote exports. We test two new implications of this hypothesis. First, the impact of financial development is higher where fixed costs are large due to either product characteristics or the distance between exporter and importer. Second, we find that in countries with well developed finance, total exports and the allocation of exports across importers are more sensitive to exchange rates than in countries with lower financial development.

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Chapter 1

Who Moves to the City in China

– Evidence on Mobility and Occupation Choice from the Housing Exchange Program in Jiaxing

1.1 Introduction

At 3.3 billion, the world's urbanization population exceeded rural population for the first time in 2007, and is expected to increase further to 5 billion by 2030 (UNFPA, 2007). Over 90 percent of the world urban population growth is occuring in developing countries (UN-Habitat, 2006) at a rate of approximately 70 million *per annum*, or 2 billion in total over the next two decades. Asia, as a leading force in this process, will experience a doubling in its urban population size over the same period (World Bank, 2009a). The rapidity with which this is happening is distinct from the much milder experience of developed countries, making simple extrapolation of existing knowledge dubious (Hargroves and Smith, 2005).

In addition to a record-high speed, the East Asian urbanization process is also under the tension of a large population base against scarce land resources. On the one hand, the fast progression of industrialization and urbanization entails a continuous influx of rural population into the cities, which calls for a significant geographical urban expansion to accommodate this massive increment to the labor force, especially in larger cities. On the other hand, the rural-urban migration, though accelerating, still takes years, which

necessitates preservation of sufficient arable land to maintain the livelihoods of the still large rural population, to say nothing of food security considerations.

In line with the classical dual sector models (see for example Lewis, 1954; Ranis and Fei, 1961; Harris and Todaro, 1970), the cost of unskilled labor is kept low during this process, which sustains a competitive advantage in labor-intensive manufacturing sectors (World Bank, 1993). However, it also has other undesirable effects: 1) underemployment and job insecurity caused by an excess of new migrants over available employment opportunities (Todaro 1969; Rozelle et al., 1999); 2) incomplete urbanization, which is most saliently manifested in a locational separation of workplace and permanent residency, a continuation of agricultural production as a supplement to off-farm work, and under-consumption of migrants (Leff, 1969; Mason, 1997; Lu and Song, 2006);[1] 3) social injustice arising from longtime social exclusion, with far-reaching effects on sustainable economic development (Gill et al., 2007). These problems are largely tractable or even non-existent when urbanization is slow, but have their order of severity profoundly magnified under the fast pace of urbanization in many developing countries, in particular East Asian countries.

Another distinctive feature of the East Asian urbanization process is the active role of the government and an extensive use of administrative power - a typical stance they take toward most social and economic affairs (Krugman, 1994). This apparently results in a rapid GDP growth, which in turn prompts the governments to set rather ambitious goals

[1] These incompletely urbanized people typically have a rural *hukou*, and are allotted a rural homestead and a piece of farmland, but derive a large share of income from urban employment. They can be sorted into two groups. People in the first group live at home and work somewhere within commuting distance. The second group is made up disproportionately of those from inland China, who migrate (usually annually) to the more prosperous Southeast in search of work.

for the rate of urbanization (World Bank, 2009a). However, do people indeed benefit from this government-led expedited urbanization process?

China is a typical example of the situations discussed above. Its urbanization rate shot up from a meager 19% in 1980 to 36% in 2000, and then to 47% in 2009 (NSBC, 2001, 2010; United Nations, 2010).[2] At an unprecedented rate of around 18 million new rural-urban migrants annually, China is expected to be home to some 870 million urbanites in less than a decade, which is almost three times the current U.S. population or 1.5 times the sum total of the EU-27 (UNFPA, 2007). At the same time, the country is known for a ubiquitous influence exerted by the "visible hand". In the case of the urbanization process, this is embodied in a rigid household registration system and a tight control over land transactions, although the restrictions have somewhat loosened recently.

Generally speaking, urbanization in any country follows one of two routes, either through absorption of rural population into existing metropolitan areas, or through the establishment and expansion of new (and typically smaller) cities/towns in originally rural areas, also known as *in situ* urbanization (Zhu, 1999). The first channel, which China has largely followed since early 1990s (Zhao, Chan and Sit, 2003), arguably prevails in reaping economies of agglomeration (see for example Nakamura, 1985; Black and Henderson, 1999) and has attracted much media attention.[3]

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[2] Of the 47% population officially reported as urbanized, only 33% are completely urbanized in the sense of having an urban *hukou* (MCCBE, 2010). Simply put, the *hukou* system classifies all Chinese as urban and rural residents, and allocates almost all civil rights and benefits (including social security, employment benefits, eligibility for subsidized housing, education and health care resources, public service and utilities, etc.) accordingly, and almost invariably favors urban residents (at least until the recent past). Conversion between the two types of *hukou* status is strictly regulated.

[3] It is generally accepted that there are two distinct stages of urban development in the post-reform era in China. The first stage mainly followed the second route, with the rise of township

However, once population concentration exceeds a certain level, benefits of agglomeration dissipate and the nonrival property of public or quasi-public goods quickly diminishes (Henderson, 2010), at which point a continuing population inflow would stretch the mega-cities' accommodation capacity.[4] This intensifies the competition for scarce private goods, notably land, and consequently push up the cost of living (Hall, 1977; State Council Information Office, 2004). This automatically discourages further settlement by people with limited earnings ability, effectively placing a curb on the expansion of mega-cities.[5] It is under the antithesis between an unstoppable torrent of surplus rural labor and a nearly full reservoir of larger cities that the second route stands out as a potentially promising solution.[6]

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and village enterprises (TVEs) providing outlets to surplus rural labor. However, following the urban land reform in late 1980s, it soon transitioned into the second stage, featuring city-centered urbanization (McGee et al., 2007; Wu, Xu and Yeh, 2007). Consequently, while China accounts for around 18% of the world urban population, it holds a quarter of, or 236 out of the 961 cities with more than 500,000 inhabitants (United Nations, 2009; CASS, 2010).

[4] As a good exemplification of the textbook case of congested public goods, the excessive population size of many mega-cities in China is causing serious traffic congestion, an issue that besets residents and local government alike (Liu and Smith, 2006; ADB, JBIC and World Bank, 2005).

[5] Again taking China as an example, profit-maximizing real estate developers in established cities are mainly catering to those already urbanized, such as offspring of original residents, white-collar workers without local *hukou*, returnees from overseas studies, and employees of joint ventures, whereas the *real* newcomers to the city, i.e. new migrants from rural areas, cannot afford to settle down on a permanent basis (Wang, 2010; Wen, 2010)

[6] With *in situ* urbanization, development of secondary cities also better addresses the three above-mentioned side effects of rapid urbanization, at least in theory: 1) it lowers the hurdle to the urban sector for the rural population; 2) more spatial resources are available to accommodate the demand for urban residence and amenities, which makes more thorough urbanization possible; 3) the newly urbanized population, being local to the new cities, does not constitute a disadvantaged social group, which makes social exclusion less relevant.

Under these circumstances, small and medium cities and towns began to flourish in rural China, with the government behind the rise of some of them.[7] As this represents a deviation from the longtime focus on larger cities, most programs are carried out on an experimental basis, which provides a good research opportunity that will enlighten on the current round of urbanization across the developing world, particularly East Asia. Among them, the *Housing Exchange Program* in Jiaxing, which was first launched in early 2008 and offers rural households the choice to exchange their rural homestead for urban real property, is one of the earliest and most systematic of such attempts.

Given the breadth of this subject, a comprehensive evaluation of this program is beyond the scope of this study. Instead, I will attempt to answer two fundamental questions. First, *who* will choose to participate in this program? Or more specifically, what household/individual characteristics would render this program - essentially a relaxation of households' optimization constraints - net utility-enhancing in their cost-benefit analysis? Second, once a household decides to participate, *what* effects will it have on its members' employment and earnings? To answer these questions, I conducted a survey on a sample of 3,408 rural households in Jiaxing in March-April, 2010.

As participation in the program is on a voluntary basis, it should be individually optimal, hence discussion on the first issue actually identifies the demographic group to whom the urbanization program is the most effective. Note that this is not answering which group is the readiest for urbanization, but rather which group is the readiest *under this program*. While this potentially leads to a digression from the natural course of

[7] China is not unique in this. Urban expansion is slowing down in many larger cities globally, and the emergence of secondary cities has become a key driving force of urban population growth. In fact, about half of urban growth is now occurring in small- and medium- sized cities with population of 500,000 or fewer (World Bank, 2009a, 2009b).

urbanization, tests on program effects may be of greater practical relevance given the East Asian governments' consistently active stance, since the findings can be applied to other regions to assess their suitability for an analogous program – the exact purpose of a policy experiment. The second question singles out one of, if not *the* most fundamental aspect among various impacts of the policy intervention, which is key to the sustainability of the induced urbanization progress, no matter how favorable is the one-off payment from the program. And as such, it serves as a barometer of the extent to which transformation of the traditional agricultural society is complete.

Before turning to an outline of the findings, a distinctive feature of this round of urbanization has to be noted. From the planned economy era to the initial stage of market economy, urbanization in China was predominantly accomplished via recruitment of individuals with adequate physical or intellectual human capital into urban schools or state-owned enterprises, while the rest of their households remain rural. The emergence of TVEs and private businesses later on considerably broadens this path, yet with little impact on the basic pattern. This all started to change with the recent rise of secondary cities, where households undergo the rural-urban transformation as a whole under *in situ* urbanization.[8] The Housing Exchange Program, by facilitating the development of

[8] To be sure, individual migration may also be part of the household utility maximization process, with remittance from off-farm workers used to diversify household income and relax the liquidity constraint on agricultural investment (Yang, 1997; Rozelle, Taylor and deBrauw, 1999; Zhao, 1999; de Janvry and Sadoulet, 2001). Such optimization is however quite incomplete, whereas physical relocation of the entire household necessarily takes more factors into consideration (Becker, 1991).

secondary cities, also falls in this category. This paper therefore takes household as the basic unit of analysis, supplemented with investigation at the individual level.[9]

Regarding the first question of *who* participates in the program, basic demographic characteristics such as family size and age composition are found to have insignificant effects, while participants and non-participants differ widely along socio-economic lines, with the former worse off in general. This is manifested in several dimensions, including lower earnings, longer distance to town center, and inferior housing and living conditions. By implicitly offering the disadvantaged group a greater scope for betterment in career prospects and amenities of life, this program extends opportunities for urbanization to a broader socio-economic spectrum, hence is progressive.

Investigation on the program's occupational and income implications finds a strong positive effect of participation on household non-agricultural income and the opposite for agricultural income. For example, the DDD estimate suggests that other things equal, participation in the program augments the 2007-2010 growth in non-agricultural income for an average household by 3,432 *yuan*, which is over a third of the overall growth. The counterpart effect on household agricultural labor income (excluding farm rents) is a 7,200-yuan reduction, with many participating households quitting farm work altogether. The overall impact on total household income is ambiguous, with more evidence pointing to an increase. This however is not suggestive of a modest welfare change; rather, participants optimally choose to consume more leisure under the income effect of program compensation.

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[9] The emphasis on household also espouses the preeminence of family values in the East Asian mores, which fosters reliance on familial strength against individual difficulties and risks and encourages people to sometimes place family honor over personal goals (Hsu, 1981; Shneider and Lee, 1990; Wong and Ahuvia, 1998).

Decomposing the effects at the individual level suggests that the above observations derive from a combination of forces at the intensive *and* the extensive margin, with the latter effect more salient. Program effect at the intensive margin is embodied in variations in income of agents with urban jobs prior to the program, which is in general positive, but not always significant. Results from all methods also suggest that their employment quality is not degraded or even upgraded, indicating that the increment to income is not a trade-off for lower job security. At the extensive margin, participation in the program significantly increases the percentage of the labor force engaged in non-agricultural employment, with the estimated size of the change varying from 3.4 to 51.7 percentage points by different methods.

The remainder of this paper is organized as follows. In section 1.2, I will describe the program and the sample data (a data appendix is included at the end to give more details). Section 1.3 presents a simple model. Section 1.4 examines factors in agents' participation decision using a logit model. Section 1.5 tests the program's impact on agents' income composition and occupation-related properties. Section 1.6 concludes and briefly explores several directions that are potentially promising for future research.

1.2 The Program and Data

1.2.1 *The Housing Exchange Program*

China has a rigid and inflexible land management system. All land is artificially zoned into urban and rural, with use of the latter for urban development purposes strictly banned and tight restrictions imposed on rezoning, nor is market transaction of rural land allowed. As an adjustment mechanism, the central government specifies a maximum area

of farmland that can be converted to construction land each year,[10] which is in turn apportioned down the hierarchy.[11] While the system was functional in the traditional agricultural society, it has increasingly become a binding constraint on the accelerating industrialization and urbanization process, with the allocated zoning quota oftentimes falling short of need, and rural residents unable to urbanize completely by selling their immovable properties. This was exacerbated after the central government determined to conserve farmland by tightening up the quota distribution system in late 1990s. See Appendix A for a detailed account of the current rural land management system in China and its deficiencies.[12]

As a result, local officials "operating under conflicting imperatives of promoting economic growth and managing local government finances while preserving farmland" (Ding and Lichtenberg, 2010) find it increasingly difficult to meet all goals. In their quest for a way out of this dilemma, some have cast eyes on rural construction land - a special type of land that, despite being rural, is still zoned as construction land and hence whose conversion for urban development does not take up the allocated quota and is less heavily regulated by the central government. One big obstacle, however, lies in the fact that most rural construction land is distributed as homestead to households, usually dispersed in a

[10] *Construction land* is a special Chinese land use term, which includes any non-deserted land not used for cultivation.

[11] There are five administrative levels in China, namely central, provincial, municipal, county/district, and township in a top-down order. Villages are one level lower down the hierarchy, yet not included in the civil service system.

[12] While this paper takes the centralized control as exogenous, its merits and faults have been a much debated topic for both the academia and public media, see e.g. Zhang (2000).

broad expanse of farmland.[13] This makes it difficult, if not impossible, to be utilized directly for urban constructions, which require larger tracts of land. To tackle this, some local governments are experimenting with projects that offer smaller urban residence and/or other compensation in exchange for rural households' homestead, which they then reclaim to farmland and use the *net* saving on construction land zoning quota for urban development.[14] The Housing Exchange Program in Jiaxing is among the first of such experiments, as well as one of the largest in scale.

The program offers eligible rural households three mutually exclusive options in exchange for their homestead: a) money, b) newly constructed apartment(s) in the city/town, or c) the right to build a new home within an assigned area in the city/town with some additional monetary compensation. The specific compensation schedule under each option varies by towns and depends on a collection of factors, such as family size, homestead area, and commitment to moving out in time. Besides these three options, households can also choose not to participate and keep their homestead. This is what essentially distinguishes the program from most other urbanization projects, in particular the *chaiqian* programs,[15] which all draw on coercive power to some extent.[16]

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[13] Rural homesteads registered a total of 16.5 million hectares, or 67.3% of all construction land nationwide in 2000 (Ministry of Land and Resources), part of which results from individual appropriation of farmland for construction purposes (Sargeson, 2002). In the case of Jiaxing, the homestead area occupied by an average rural household is nearly three times that of the current prescribed limit. Besides household homesteads, part of rural construction land is kept under the direct oversight and management of the village committee, but the total quantity is negligible.

[14] For historical reasons, the average rural residence by far outsize their urban counterparts. Taking Jiaxing as an example, the floor space of residential buildings per capita in 2008 was $67.45m^2$ in rural areas, compared with merely $29.99m^2$ in the urban district (Statistics Bureau of Jiaxing). The urban-rural disparity in construction area size per capita, which also includes infrastructure, business premises, etc., is somewhat less drastic, yet still huge at $95.71m^2$ vs. $141.23m^2$ (Jiaxing Municipal Bureau of Land and Resources, 1996).

[15] The official definition of *chaiqian* (meaning demolition and resettlement) is "an legal act in

The program is implemented in two phases. Phase I was launched in early 2008, when the program was experimented in nine out of the 71 towns in Jiaxing (hereafter referred to as pilot towns). Phase II started in early 2010 and extended coverage of the program to all towns of Jiaxing. As the main purpose of the first phase was to test the program's applicability to the entire region and prepare for the second phase, the pilot towns were selected on two criteria to ensure their comparability with other towns. First, at least one town has to be picked from each of the seven counties/districts. Second, pilot towns have to be representative of the respective counties/districts in terms of economic development, industry distribution, income level, fiscal capacity, etc.

As is mentioned at the beginning of this section, trading activities on rural homestead - as one type of rural land - is banned. Thus, from the perspective of eligible households, this program essentially establishes a market, though very preliminary, for their property rights to the homestead. To be sure, this is far from setting up a completely free market for rural real properties, but rather resembles a monopsony with the government as the

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accordance with urban planning requirements and land use documents approved by the government, which involves pulling down houses and appendages within the area zoned for construction, relocating the organizations and residents thereon, and compensating them for their loss." It parallels *eminent domain* (or compulsory purchase in the UK) in that both involve an action of the state to seize or expropriate a citizen's private property rights without the owner's consent. However, condemnation via *eminent domain* has a very limited scope of application by requiring that the property be taken for government use or for public purposes, and is practiced on very rare occasions. *Chaiqian*, on the contrary, has furnished three quarters of the land used for urban expansion in China (Ministry of Land and Resources, 2003), and has been exercised in a much more forceful manner in general.

Incidentally, *chaiqian* programs, being largely exogenous and compulsory, arguably better approximate natural experiments. However, such studies cannot answer the question of *who* will choose to participate in the program. Moreover, *chaiqian* programs usually aim to expand the city contiguously and hence typically cover a rather narrow demographic group living on the immediate outskirts of a city or town. This casts doubt on their representativeness of rural households and in turn the external validity of the findings of these studies.

[16] Despite the official claim of free choice, suspicion of its genuineness is inevitable, especially in the context of a major government-led program with well-specified goals. This suspicion is however largely dispelled in my interviews of the cadres/officials and the households, which also confirm that all residents of the same town have equal access to the same policies.

sole buyer, yet it still profoundly enlarges the choice set for rural households, hence represents a first step toward further liberalization of this market. One concern is that the program was launched quite recently, leaving a short time window for its potential impacts to become identifiable, yet the other side of the coin is that confounding factors also have less time to manifest themselves, making empirical findings more reliable.

To determine the program's effect, care has to be taken that measurement is not biased by other contemporaneous governmental programs. Most of the recent rural policies represent minor modifications on existing programs, except for a program titled Exchange Farmland for Welfare, which, analogous to the Housing Exchange Program, establishes a quasi-market for farmland with the local government acting as a liaison.[17] Besides programs specifically directed at rural households, other noteworthy policies include an ongoing project that extends certain local benefits to permanent residents without Jiaxing *hukou* (officially called "new residents"), mainly migrant workers,[18] thereby improve their living and working conditions. Its impact on the local labor market may have some repercussions on the career prospects of native residents.

My identification assumption would be violated if the geographical span of these programs were correlated with the distribution of our pilot towns. Fortunately, all of them were implemented across the board, therefore can largely be taken as exogenous for our

[17] Households can exchange farmland for a steady stream of rental income by signing long-term subleasing contracts with the village farmland circulation center, or for an upgrade in pension payments to selected household members. The official documents also allude to a third option, i.e. securitize one's farmland property as shares in a village land cooperative, which then manages all these lands jointly and distributes dividends to the "shareholders". However, this option has not been made available in practice, probably due to difficulty with implementation.

[18] The term migrant worker, or *nongmingong*, literally means people with a rural *hukou* but work in the urban sector. It can be interpreted broadly or narrowly in an academic context. The broad definition incorporates both groups of people specified in footnote 1, and the narrow definition only refers to the latter group. The narrow definition is used here.

purpose. Some of the programs may have differential regional impacts. For instance, towns with different proportions of population as non-locals may vary in the scale of impact from new resident policies. However, since our sample selection is perfectly random within sample towns, such concerns are addressed by controlling for town fixed effects. Additionally, these programs may have a non-negligible interaction term with the Housing Exchange Program in their gross impact.[19] This does not invalidate our results. Quite the contrary, as these policy instruments are being widely applied throughout the nation, this actually makes the measured treatment effects all the more relevant and generalizable.[20]

1.2.2 *The Data*

Data used in the principal analysis are collected by a questionnaire survey on a sample of 3,408 households, which comprise 13,796 individuals. All samples are from seven towns in Jiaxing, of which four are pilot towns, i.e. towns within the first batch to have launched the Housing Exchange Program: *Yuxin*, *Yaozhuang*, *Xincang*, and *Longxiang*. This group comprises 1,789 households, or 7,279 people. The other three towns form the control group, namely *Honghe*, *Xindai*, and *Fengming*,[21] which together contain 1,619 households, or 6,527 people.

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[19] For example, as we will see later, participating households are more likely to avail themselves of the newly established farmland market, which implies that the impact of improved farmland tradability may depend on eligibility for the Housing Exchange Program.

[20] The recently completed Shanghai-Hangzhou Express Rail project, with Jiaxing being the midway point, may also be of some concern. But as this project was put into use only on 26 October, 2010 - half a year after the survey, its chief impact on the local economy will likely be felt only in years to come, while leading effects can be mostly absorbed by town fixed effects.

[21] I confined the control group to within the jurisdiction of Jiaxing to avoid incompatibility at the municipal level, because towns in different cities, even if similar in natural endowments and

Sample selection within each town is completely random, while selection of the sample towns was carried out as follows. First, out of the nine pilot towns, I selected the four towns that were quicker in stipulating the detailed policies, which ensures a long enough reaction time for households. Once the treated group is determined, to achieve utmost comparability with the control group, I asked the chief executives of the sample pilot towns to suggest a few towns, preferably within the same county/district, that are a good match for the purpose of this study by both "hard" and "soft" criteria,[22] where *hard* refers to quantifiable measures such as GDP per capita, distance to the country/district center, and fiscal capacity, and *soft* refers to qualitative standards such as competence of government officials and village cadres, and social mores.[23] I then compared statistics on the hard criteria to ascertain their comparability, with some summary statistics reported in Table 1.1.

Geographical locations of the seven towns are marked in Figure 1.1. The resulting distribution of sample towns is geographically and administratively dispersed, which contributes to the sample's representativeness of the region. *Yuxin* is matched with *Honghe*, both within the central district, or the City of Jiaxing. *Longxiang* is matched with *Fengming*, which is also located in *Tongxiang* County and adjacent to the county center. *Xincang* and *Yaozhuang* are both matched with *Xindai*, as they are all far away from the center of their respective counties, yet bordering Shanghai. Validity of the

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geographical location, are subject to distinct administrative orders. Besides, towns within Jiaxing are more comparable with each other than with towns outside the city on most non-policy factors relevant to our analysis anyways.

[22] In practice, they are all very specific about *the* most comparable town to their own.

[23] Other criteria include proximity to the pilot towns, leading industries, household disposable income, proportion of migrant workers in the labor force, distance to Shanghai, etc.

Table 1.1 Summary Statistics on Sample Towns

	Treated Group					Control Group			
	Yuxin	*Yaozhuang*	*Xincang*	*Longxiang*	***Total***	*Honghe*	*Xindai*	*Fengming*	***Total***
Permanent rural resident population	38,137	18,890	44,095	23,178	**124,300**	47,960	60,090	23,084	**131,134**
# permanent rural households[a]	11,095	4,590	10,311	6,209	**32,205**	6,536	13,075	5,848	**25,459**
Distance to the closest arterial road or freeway exit < 10*km*	Yes	Yes	Yes	Yes		Yes	Yes	Yes	
County government accessible in 1*hr*	Yes	Yes	Yes	Yes		Yes	Yes	Yes	
Annual income per capita (%)									
< 2,500 yuan	4.8	2.1	4.4	0.1	**3.3**	1.1	3.6	0.8	**2.3**
2,500-3,500 yuan	7.8	3.3	6.8	1.6	**5.5**	2.1	7.7	1.7	**4.8**
3,500-7,000 yuan	31.6	14.7	28.8	12.8	**24.2**	12.8	37.0	15.2	**25.5**
7,000-10,000 yuan	35.7	31.3	40.7	60.3	**41.8**	47.4	39.5	60.4	**46.5**
> 10,000 yuan	20.1	48.5	19.4	25.1	**25.3**	36.7	12.3	21.9	**20.9**
Education level (%)									
Never attended school	13.8	14.9	14.7	18.0	**15.1**	16.4	16.6	15.7	**16.4**
Primary school	40.6	43.1	37.4	41.9	**40.2**	40.4	39.4	42.1	**40.3**
Junior high school	33.8	33.7	38.9	31.1	**34.9**	36.3	36.9	30.2	**35.2**
Senior high school	9.5	6.6	7.0	6.7	**7.7**	5.6	5.5	9.2	**6.4**
College and above	2.4	1.6	2.1	2.4	**2.2**	1.3	1.6	2.9	**1.8**
Employment type (%)									
Unemployed or not in labor force	28.9	27.5	26.0	26.2	**27.1**	27.6	25.0	27.0	**26.1**
Employer	1.4	1.1	1.2	1.5	**1.3**	0.4	1.6	1.4	**1.2**
Domestic assistant[b]	1.1	0.9	0.7	0.8	**0.8**	0.3	1.1	0.6	**0.8**
Self-employed	31.8	33.8	25.7	30.3	**29.7**	51.6	26.2	37.7	**35.5**
Employee	36.5	36.2	46.0	40.8	**40.5**	19.9	45.3	32.6	**35.8**
Public office worker	0.4	0.5	0.5	0.6	**0.5**	0.3	0.8	0.7	**0.6**
Time engaged in agricultural production on own farm (%)									
Nil	49.0	46.4	53.5	55.7	**51.5**	44.0	52.1	47.6	**49.0**
< 3 months	22.0	27.5	25.2	18.6	**23.2**	34.2	27.4	16.7	**26.6**
3-9 months	22.4	17.5	15.9	11.7	**17.3**	20.3	17.2	8.5	**15.9**
> 9 months	6.6	8.6	5.4	14.0	**8.1**	1.5	3.3	27.2	**8.5**

Table 1.1 (Continued)

Time engaged in non-agricultural employment (%)									
Nil	50.5	51.3	45.8	48.0	**48.5**	38.4	45.3	55.7	**46.0**
< 3 months	1.7	2.8	1.1	1.2	**1.5**	2.2	0.8	1.8	**1.4**
3-9 months	10.7	13.9	6.4	6.1	**8.8**	28.6	5.5	6.2	**11.6**
> 9 months	37.1	32.1	46.7	44.7	**41.1**	30.8	48.5	36.3	**41.1**
Number of household-owned real property (%)									
Nil	1.2	0.2	1.5	0.2	**0.9**	0.2	0.1	0.0	**0.1**
1	92.2	91.9	90.2	79.2	**89.0**	86.2	90.6	88.3	**88.9**
2	6.3	7.6	8.0	19.6	**9.5**	13.1	9.0	10.8	**10.5**
≥ 3	0.4	0.4	0.4	1.1	**0.6**	0.6	0.4	0.8	**0.6**
Self-evaluated real property value (%)[c]									
< 50,000 yuan	18.5	29.7	10.6	15.4	**17.1**	11.8	11.0	4.9	**9.8**
50,000-200,000 yuan	66.2	64.4	75.2	72.8	**70.7**	71.8	76.4	71.4	**74.1**
> 200,000 yuan	14.2	5.7	12.7	11.6	**12.1**	16.3	12.5	23.7	**16.1**
Primary cooking energy source (%)[d]									
Firewood	67.2	42.4	84.6	87.4	**73.1**	71.5	62.6	88.2	**70.8**
Coal	0.3	0.0	0.0	0.2	**0.1**	0.0	0.1	0.1	**0.1**
Tanked gas	30.0	56.1	14.3	11.0	**25.1**	24.9	35.2	10.7	**26.9**
Marsh gas	0.9	0.0	0.0	0.0	**0.3**	0.0	0.0	0.7	**0.2**
Electricity	1.6	1.5	1.0	1.4	**1.4**	3.6	2.1	0.3	**2.1**
Has tap water (%)	89.4	99.9	99.3	96.1	**95.4**	95.2	99.1	96.0	**97.4**

Source: 2007 Agricultural Census. The statistics correspond to conditions in 2006.
Notes: The percentage numbers may not sum up to 100% due to rounding errors.

[a] Defined as households that had not moved for over a year at the time of the census.
[b] Domestic assistants are people who assist other family members with productive activities. They are usually not paid separately.
[c] Households without any real property are excluded.
[d] Many, if not most, rural households use both firewood and tanked gas, and oftentimes find it hard to distinguish between the primary and secondary sources unambiguously, and the apparent cross-town disparity is remedied if secondary sources are taken into account. Only statistics on primary energy source are shown here because merging data on primary and secondary sources would run into the problem of double-counting households using only one source.

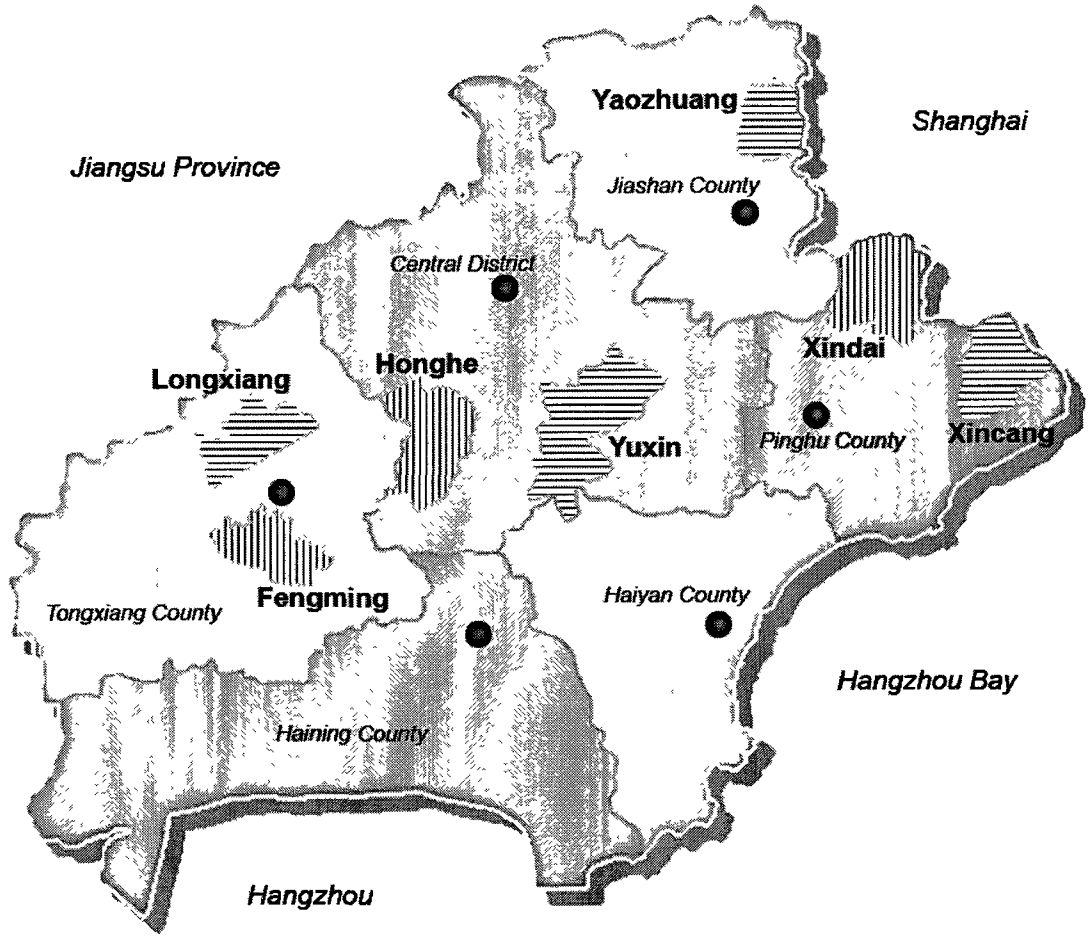

Figure 1.1. Map of Jiaxing

Notes: Jiaxing is a municipality comprising two central districts and five counties. An analogy with Greater Boston would liken the central districts to the City of Boston, and the counties to its satellite cities. I will hereafter use *Greater Jiaxing* or simply *Jiaxing* to denote the entire administrative area of the municipality, and *the City of Jiaxing* to denote the central districts. In this map, the central district and five counties are distinguished by different colors, with their respective centers depicted by solid black circles. Jurisdictions of the seven sample towns are marked out on the map, with pilot towns shaded with horizontal lines and non-experimental towns with vertical lines. We did not sample towns from the two southmost counties because the three pilot towns there all launched the program relatively recently.

choice of matches is strongly supported by summary statistics in Table 1.1. To account for the fact that pilot towns were not selected randomly, the empirical studies below all

control for town fixed effects.[24] Details for the data collection process can be found in Appendix B.

1.3 The Model

The two questions proposed in Section 1.1, i.e. *who* participates in this program and *what* effects it has on participants' occupation and income, actually derive from a two-stage decision-making process. They choose whether or not to participate in the first stage, and then decide on an occupation in the second stage, taking their first-stage choices as given.[25] I will develop a simple model here to study the underlying reasoning process, and test the model predictions for the two stages in Sections 1.4 and 1.5 respectively.

For the first stage, I will draw on the classical approach to the study of human migration following the seminal work of Sjaastad (1962), which views it as an investment whose worthiness depends on a rational evaluation of costs and benefits. The basic dual sector structure bears a resemblance to that in Harris and Todaro (1970).[26] The second stage is what differentiates this model from the rest, which generally place occupation or income into a one-to-one correspondence with location. Instead, agents have to optimize

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[24] Inclusion of village fixed effects would be redundant as sample selection below the township level is perfectly random. Incidentally, given the small number of pilot towns (nine), even a randomized selection at the township level cannot guarantee comparability between the experimental and control groups.

[25] As is laid out in Section 1.2, the program offers participants several options, which I simplify to an either-or choice in this model. In particular, I effectively assume the option of pure monetary compensation away for its negligible take-up rate in practice.

[26] Other classical works along the same line include, but are not restricted to, Todaro (1969), Bhagwati and Srinivasan (1974), Fields (1975), Polachek and Horvath (1977), Mincer (1978), Cole and Sanders (1985), Todaro (1986), Borjas (1987), and Massey (1990a).

with respect to occupational choice and time allocation at this stage, following the framework set up in Gronau (1977) that is itself an extension of Becker (1965).

Formally, the economy consists of two sectors, the agricultural sector and the non-agricultural sector, which are located in the village and the city respectively with distance δ in between. Let there be a single-person household which initially lives in the village and maximizes utility $U(Y, l)$, a function of total income (Y) and leisure (l) with standard signs for all the first- and second- order partial derivatives. For notational ease, I structure this as a one-period problem, where Y is interpreted as the present value of all current and future incomes, and can be further divided into labor income and other incomes: $Y = W + V$. As we will see shortly, V not only encompasses monetary terms, but also the monetized value of other types of costs and benefits.

The household is endowed with a piece of farmland, which it can either rent out for R or cultivate by itself. In the latter case, a fixed amount of time $\bar{t}$ is spent on farm work which produces $f(s)$, where s is the agent's agricultural skill.[27] The agent may also pursue a job in the non-agricultural sector, which is however not guaranteed but rather obtained with probability $p(S, d)$, where S is her non-agricultural skill, and d is the distance from

[27] The assumption of a two-element choice set allows a more parsimonious model while keeping the main results. It also captures reality pretty well. As rural households in Jiaxing are allocated very small plots of farmland (0.06 *ha.* per capita or 0.21 *ha.* per household according to the 2007 Agricultural Census), and a large share of the agricultural production cost is fixed, they normally make an either-or choice between self-farming, which entails a large fixed labor input, and rental, without much micro-division. The possibility of leasing in more farmland is also implicitly assumed away here, since only 109 of all the 3,402 sample households with relevant data reported leasing in farmland in 2007, among which only 24 were over 1 *ha.* It does not violate the market clearing condition, either, since farmland is usually leased to agricultural firms or non-locals, especially after the Exchange Farmland for Welfare Program.

her home to the city.[28] $\partial p/\partial S > 0$, reflecting the higher relative demand for skilled workers; and $\partial p/\partial d < 0$, which corresponds to the greater search efficiency of those living closer to urban areas, following from the well-studied residential location theory (see e.g. Simpson, 1992; Wasmera and Zenou, 2002). Non-agricultural income is 0 if the job search is unsuccessful, and $F(S, T)$ if a job is found, where T is time spent on non-agricultural work.[29] In addition, job in each sector entails a fixed commute time cost: $c(d)$ for the urban sector and $c(\delta - d)$ for the agricultural sector, with $c(0) = 0$ and $c'(\cdot) > 0$.

Rather than specializing in one sector, agents may work in both sectors at the same time, which equips her with a four-element occupational choice set: full-time farmer, non-agricultural worker, double-jobbing, and not working.[30] For simplicity, I assume that all occupational choices are made at the beginning of the period, so that one cannot depend her disposal of the farm on whether she gets an urban job or not. The agent maximizes expected utility by choosing an occupation and then optimally allocating time, which is depicted in Plot A, Figure 1.2.

Prior to the program, the agent is endowed with V_1. If she does not work at all, she gets a total income of $R + V_1$, and ends up at point D. A non-agricultural worker starts from this point, incurs a time cost $c(\delta)$, and picks the point A_1 where her indifference curve touches the production possibility frontier (PPF). A full-time farmer with farming

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[28] S encompasses a full set of determinants of one's desirability for *urban* jobs, including school education, vocational training, physique, talent, etc.

[29] Instead of zero income in the worse state, the Todaro (1969) model of a traditional urban sector and a more modern urban sector may be more realistic, or even a continuum of states with different non-agricultural income levels. These however do not fundamentally change the results.

[30] While double-jobbing in a single person household involves the same person straddling two sectors, in a multi-member household, it can have some members being full-time farmers and others working full-time in the urban sector (and even some members not working at all).

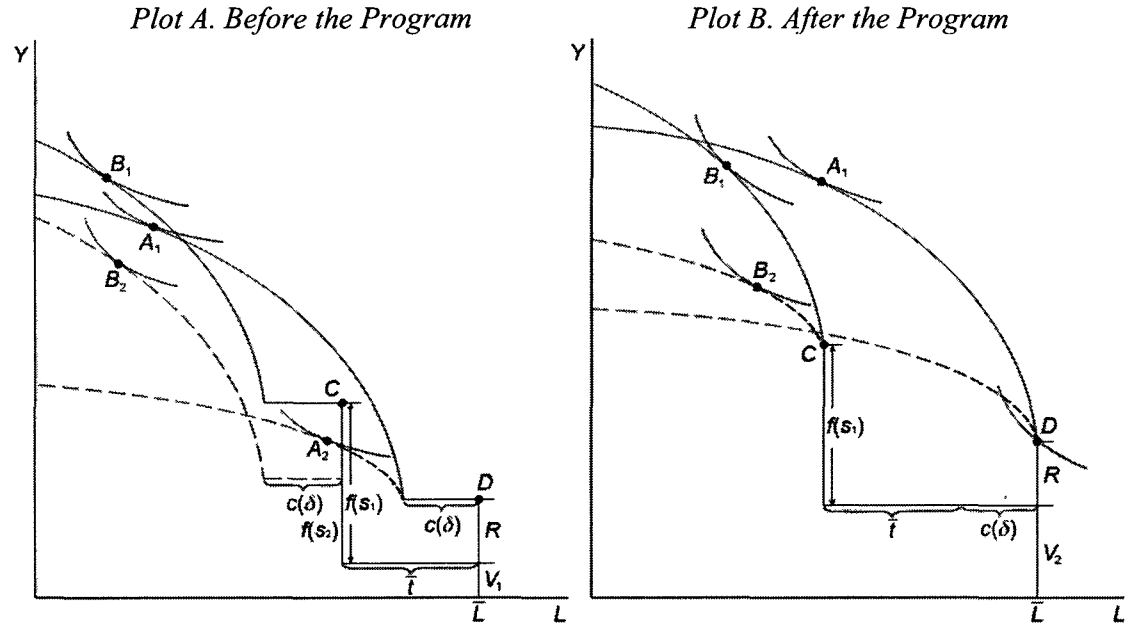

Figure 1.2 Optimization with Respect to Occupational Choice and Time Allocation

Notes: The horizontal axis corresponds to leisure (or the opposite of labor input), and the vertical axis to income. The production possibility frontiers should be treated as having been adjusted for $p(S, d)$.

skill s_1 incurs zero commute time and gets point C, while a double-jobber gets point B_1. In this case, B_1 is preferred to all the other achievable points, hence the agent optimally chooses to be a double-jobber. If instead, her farming skill is lower at $s_2 < s_1$, then the highest utility level she can get as a double-jobber (B_2) is now lower than A_1, and she ends up being a full-time urban worker. Finally, if her non-agricultural skill S is lower so that the optimum as a non-agricultural worker shifts down from A_1 to A_2, then she is best off being a full-time farmer (C), or even not working at all (D).

Mathematically, the agent's maximization problem can then be formulated as

$$\begin{aligned}
&\max_{I_N I_A T} E\big[U(Y, l)\big] \\
&= \max_{I_N I_A T} I_N p(S,d) U\big(F(S,T) + I_A f(s) + (1 - I_A)R + V,\ H - T - c(d) - I_A(\bar{t} + c(\delta - d))\big) \\
&\qquad + \big(1 - I_N p(S,d)\big) U(I_A f(s) + (1 - I_A)R + V,\ H - I_A(\bar{t} + c(\delta - d)))
\end{aligned} \tag{1.1}$$

where I_N and I_A are two indicator variables for occupational choice. $I_N = 1$ if and only if the agent pursues a non-agricultural employment, $I_A = 1$ if and only if she works on the farm. A double jobber ($I_N = I_A = 1$) represents an internal solution, where the real marginal product of non-agricultural production with respect to labor is less than or equal to that of agricultural production, and the technical trade-off between income and leisure in the utility function is also balanced: $(\partial U/\partial Y) \cdot (\partial F/\partial T) = \partial U/\partial l$. If S exceeds a certain level for each s, the agent becomes a non-agricultural worker, and vice versa for full-time farmers.[31] However, if her productivity in both sectors were too low, then she would give up working entirely. This classification of occupational choices contingent on relative skill levels is depicted in Plot A, Figure 1.3.

The program directly affects two parameters, d and V. The value of d drops from δ to 0 as participating households move from the village to urban areas. Impact on V can be divided into a one-time lump sum component and the discounted value of a future net benefit stream. The former is embodied in the compensation package itself (including an appreciation in the locational value of the real property),[32] psychic costs of relocation, and inconveniences in the moving process and the transition period. The stream component consists of variations in the house rental, imputed or real, and better access to urban amenities yet estrangement from pastoral life, with the overall desirability a matter of personal preference. The overall impact on V is represented as a change from V_1 to V_2,

[31] The agricultural sector is analogous to home production in Gronau (1977), except that its valuation is market-based. Actually, part, if not most, of the home-produced agricultural products *are* for own consumption in Jiaxing. More on this in Section 1.5.

[32] In addition to pure locational value, residence in the urban area may also be of greater commercial value, as it is in a better position to attract a sufficient customer flow for lodging and catering businesses, thereby generating a steady income stream.

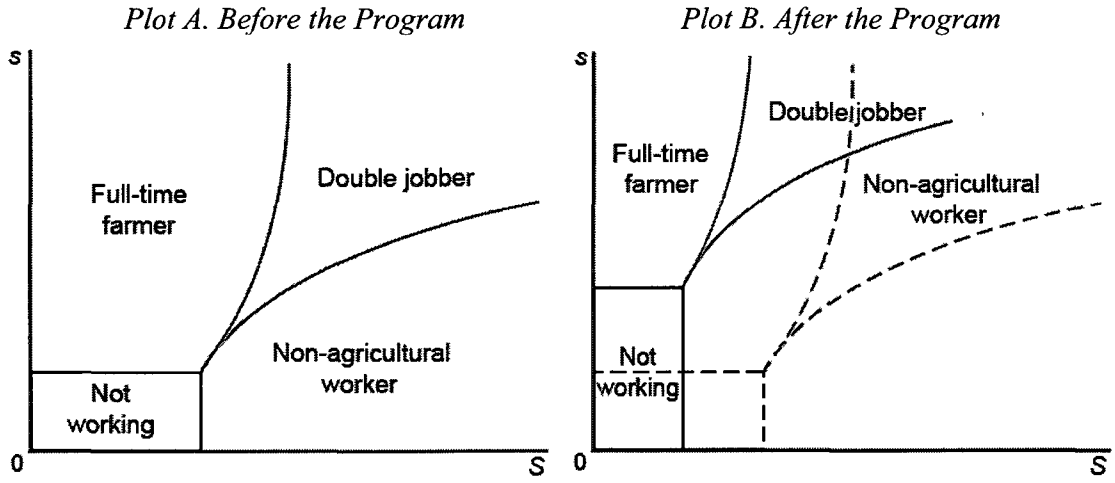

Figure 1.3 Occupational Choice Distribution on Skill Space

Notes: The horizontal axis corresponds to the agent's non-agricultural skill level, and the vertical axis to her agricultural skill level. Both plots take the distance from the agent's home to the city (d) and the present value of other incomes (V) as exogenous.

which I assume to be positive in the main analysis.

The two-stage problem can be solved backwards. At the second stage, the agent optimizes her occupational choice and time allocation contingent on participation in the program, which can be represented by Plot B, Figure 1.2. Utility from not working goes up (D) following an increase in V. Working on the farm now has to incur a commute time of $c(\delta)$, shifting the outcome point for full-time farmers (C) inward. On the contrary, urban jobs can now be done at zero commute time cost, while the success rate of getting one, $p(S, d)$, also goes up from $p(S, \delta)$ to $p(S, 0)$.[33] Both effects shift the PPF for non-agricultural workers outward, so that the new optimum (A_1) is now preferred to the

[33] Though not shown clearly in the graph, an important effect is also in work here. For a double-jobber, the uncertainty with successfully finding an urban job places farm yields in the role of a safety net. This role becomes less important with a higher V and a higher $p(S, d)$ upon program participation. This can be graphically represented as an upward shift of the concave part of non-agricultural worker's PPF.

optimum for a double-jobber (B_1), even if the agent has a high agricultural skill level at s_1. For someone with a low S, point C now lies inside her PPF as a non-agricultural worker (the dashed curve starting from D), causing her to optimally switch from a full-time farmer to a double jobber (point B_2), or even a full-time urban worker. Thus, the overall program effect at the *extensive* margin would be an increase in the take-up rate of non-agricultural jobs, with the increment being former full-time farmers and jobless people with a relatively higher S. The number of full-time farmers drops sharply, leaving only the most skillful still specializing. Plot B in Figure 1.3 depicts the new occupational choice distribution on the skill space. Finally, all these results are more pronounced the greater is the rural-urban distance δ and the associated commute time $c(\delta)$.[34]

Effects at the *intensive* margin, i.e. for those upholding occupational choices, are more ambiguous, largely because the income effect of an increase in V discourages all from working in order to enjoy more leisure.[35] Agricultural income is lower for sure under the income effect and a higher commute time cost. Expected non-agricultural income may increase for a higher $p(S, d)$, or decrease due to the income effect. This also leaves the sign of change in total labor income undecided. All predictions so far are made on the assumption of an increase in V. If the change is negative instead, predictions for the extensive margin will still hold, while those for the intensive margin have to be revised following a reversal of the income effect. More specifically, the sign of change in

[34] Introduction of path dependence in occupations (e.g. through job-switching cost and learning by doing) will affect the threshold values, but not the basic result.

[35] This also has an effect at the extensive margin by making the option of not working more attractive, especially for those with low skills. There are indeed cases of participants idling on the compensation, but very rare, because compensation from this program is insufficient in general to accommodate such indolence. Therefore I do not discuss this in detail here.

agricultural income becomes uncertain, and non-agricultural income unambiguously increases. As we will see shortly, $(V_2 - V_1)$ cannot be too low for households to find the program worthwhile, hence the benchmark predictions more likely in practice.

Having solved the second-stage optimization problem, we can go back to the first stage and find out who is attracted to the program. The answer draws on the very crux of the investment approach to migration: participate *if* and only *if* the benefits exceed the costs, i.e. $U_{\text{participating}} > U_{\text{non-participating}}$. This is more likely the greater is $(V_2 - V_1)$, which goes up with the compensation amount, improvement in housing conditions, the ease with which one adapts to urban life, etc. If the compensation amount falls short of the total expenses of relocation, those with insufficient savings may also be hindered by liquidity constraint, considering the poor access to credit for rural households.

For a given change in V, the program has a greater appeal if S is bigger and/or s is smaller, and if δ is larger. This however says little about the prior distribution of different types of income for participants against non-participants. On the one hand, taking δ as given, participants should on average have a lower agricultural income and a higher non-agricultural income, since the program implies greater time saving and lower opportunity cost for non-agricultural workers and double-jobbers. On the other hand, agents from outlying villages (big δ) and with a reasonably high S may have opted for the agricultural over the non-agricultural sector due to the distance factor, yet they are exactly the people who are the most inclined to participate.

1.4 Households' Participation Decision

1.4.1 *Summary Statistics on Determinants of Households' Choice*

This section addresses the question of who participates in the program by first examining some summary statistics, most of which can be found in Table 1.2.

The first and foremost determinant is the compensation amount, which is at the discretion of the township government. Given the total economic gains from a rural-to-urban repositioning of construction land quota generated by the program and other associated social benefits, the government decides which portion of it goes to participating households and how it is divided among them. Households take the offered amount largely as exogenous, and accept it if an only if it exceeds their reservation level, in the same way as suppliers respond to a price offer, where the "supply curve" can be roughly traced out given the distinct policies of the pilot towns.

To gauge the relative munificence of the four towns' compensation policies, Table 1.3 presents their respective exchange-for-apartments schedules, which are divided into two parts.[36] The first part represents net compensation for the old house, which is calculated as the average difference between the compensation amount and the depreciated construction cost of the old house[37]. Dividing this by the average homestead area gives us the average net compensation per unit area. *Yuxin* and *Yaozhuang* are more generous on this measure than *Xincang* and *Longxiang*. Since the compensation schedules do not account for the historical cost of the old house or take it only as a minor input, such costs represent a net cut into the compensation amount, thus making the

[36] Compensation standards of the three options are highly correlated, hence the exchange-for-apartments policies are representative of the overall compensation scheme. I focus on the compensation schedule for apartments here because in the case of exchange-for-house, land value of the newly assigned homestead makes up a large portion of the compensation, for which market valuation is unobtainable because it is still officially zoned as untradeable rural land despite its urban location. Nor did I use pure monetary compensation due to its extremely low take-up rate.

[37] Locational value of the old house is not considered because first, it is untradeable; second, any such imputed value would be trivial for the calculations here.

Table 1.2 Summary Statistics (Sample Means) of the Pilot Towns

	Entire Sample		*Yuxin*		*Yaozhuang*		*Xincang*		*Longxiang*	
	Participant	Non-participant	Participant	Non-participant	Participant	Non-participant	Participant	Non-participant	Participant	Non-participant
Basic household and individual characteristics:										
N of observations (hh)	379	1,387	199	220	59	361	41	471	80	335
Family size	3.85	4.11	3.63	3.66	3.69	4.01	4.15	4.34	4.30	4.18
Average age	43.98	43.44	45.13	44.21	42.73	45.48	45.36	42.86	40.98	41.58
Average age of hh decision makers	49.92	48.78	51.49	51.01	47.90	49.97	49.55	47.85	46.93	47.28
% hh w/ old members[a]	88.8	89.7	89.4	81.2	77.8	94.2	92.7	93.0	90.0	85.4
% hh w/ single adults[b]	41.6	40.7	42.2	35.3	33.3	41.4	43.9	39.9	42.5	44.5
% hh w/ cadre member[c]	30.1	27.4	24.6	28.0	38.9	27.2	43.9	27.4	32.5	27.2
% hh w/ party member	14.0	16.5	8.0	12.6	27.8	16.7	26.8	20.4	16.3	13.1
2007 distance to town center (*km*)[d]	4.51	3.16	5.67	4.21	3.33	2.79	3.13	3.11	2.86	2.99
2010 distance to town center (*km*)	3.25	3.13	3.85	4.21	3.00	2.78	3.00	3.04	1.82	2.96
Years of schooling[e]	5.94	5.71	5.93	5.81	6.34	5.59	6.30	5.88	5.62	5.53
Old house characteristics (2007):										
Construction year	1990.7	1992.4	1990.9	1992.1	1987.1	1989.0	1992.3	1993.5	1991.3	1994.9
Historical construction cost (*yuan*)	52,565	71,222	53,094	78,133	25,056	49,355	63,763	79,356	58,344	78,909
Homestead area (m^2)	140.7	135.0	134.1	131.8	137.1	130.4	101.8	114.3	178.8	170.8
% w/ business on homestead	2.5	7.6	2.5	11.1	8.3	11.1	0.0	4.9	1.3	5.4
Rental income (*yuan*)[f]	489	1,141	271	1,014	200	1,021	1,110	1,535	851	794
Household income (*yuan*; 2010 figures are in expectation terms):										
2007 ag. income[g]	11,461	10,183	14,115	11,477	13,469	14,293	3,482	5,118	8,143	12,086
2007-10 change	*-5,639*	*1,084*	*-9,730*	*-1,326*	*448*	*5,411*	*-251*	*224*	*-945*	*-909*
2007 non-ag. income[h]	32,443	38,986	26,480	34,202	20,667	30,167	49,488	45,129	43,763	42,589
2007-10 change	*11,367*	*9,664*	*12,946*	*8,961*	*10,777*	*10,151*	*4,627*	*9,732*	*11,182*	*9,641*

Table 1.2 (Continued)

2007 non-ag. income / total labor income	68.4	76.8	60.0	68.9	59.0	68.0	90.6	87.4	82.5	75.7
2010 non-ag. income / total labor income	89.9	80.7	93.3	77.7	74.3	71.7	91.7	89.4	87.4	79.4
2007 total income[l]	44,816	50,632	41,056	47,411	35,763	46,099	55,144	51,894	53,077	55,452
2007-10 change	*8,611*	*11,367*	*7,309*	*9,562*	*13,788*	*15,966*	*4,512*	*10,191*	*11,551*	*9,205*
Individual income (*yuan*; 2010 figures are in expectation terms) and occupation:										
2007 non-ag. income	19,483	20,913	17,771	19,914	17,302	19,997	24,155	21,156	20,716	21,834
2007-10 change	*2,227*	*2,836*	*2,491*	*2,990*	*1,255*	*2,335*	*497*	*2,784*	*3,704*	*3,426*
2007 % w/ non-ag. emp	43.3	45.4	41.0	46.9	32.4	37.6	49.4	49.2	49.1	46.7
2007-10 change	*9.1*	*4.4*	*12.7*	*5.1*	*12.7*	*7.3*	*3.5*	*3.6*	*2.9*	*2.4*
% as full-time farmer in 2010	10.4	17.1	6.1	15.6	18.8	21.8	11.2	13.3	15.7	18.8

Source: Own survey.

[a] Defined as men aged over 55 and women aged over 45. The differential cutoff points correspond to the observation by local residents that men and women above the respective ages find it much harder to get employed. Shifting the cutoffs generates qualitatively the same results.
[b] Adult is defined at age 18. Varying the cutoff age or focusing on one gender type does not change the comparative results.
[c] Defined as having at least one member that is or has been a cadre before.
[d] Distance to the closest town center, which usually is but does not necessarily have to be the household's hometown. For example, if a household's homestead is located within the jurisdiction of Town A, but is closer to the center of Town B, then its distance to the center of Town B is recorded.
[e] Excluding full-time students. The comparative results are unchanged for the entire sample or a sample of household decision makers only.
[f] The average rental income is slightly higher for participating than non-participating households in *Longxiang*, probably due to small sample bias, as lodging business is not very common in this town. Moreover, a significant proportion of participants own rental properties in the county center, which command a much higher rent than lodgings in rural areas.
[g] Defined as sum of monetary income from agriculture-related activities, including planting, animal husbandry, provision of other agricultural services, and dividends from agricultural cooperatives, but *excluding* rental income from leasing of farmland. Value of farm products used for own consumption is not imputed. Later uses of the term *agricultural income* in this paper share the same definition.
[h] Defined as labor income from non-agricultural employment, including wage/salary job, self-employment, and non-agricultural businesses. Rental income, capital gains, and welfare benefits are excluded. Later uses of the term *non-agricultural income* in this paper share the same definition.
[l] This is calculated by adding up all types incomes reported by the respondents, excluding compensation from the program.

Table 1.3 Compensation for Households Opting for Urban Apartments

	Yuxin	*Yaozhuang*	*Xincang*	*Longxiang*
Compensation for the old house (*yuan*)				
Avg. compensation[a]	132,333	142,000	67,333	129,670
Avg. depreciated old house construction cost[b]	28,951	11,567	37,877	56,793
(1) Avg. net compensation	103,382	130,433	29,456	72,877
(2) Avg. homestead area (m^2)	128.7	114.3	90.0	181.4
(1) / (2)	803	1,141	327	402
Subsidized purchase of new apartments				
Floor space allowance (m^2)				
Benchmark[c]	30/person	40/person	40+40/person	homestead area
Maximum	No	benchmark + 40m^2	200	min(homestead area, 260)
Purchase price (*yuan*/m^2)				
≤ *benchmark*	420	1,000	1,400	1,000
> *benchmark*, ≤ *maximum*	660 below homestead area, 1,260 within 20m^2 above, 1,980 afterwards	1,600	1,900	1,000
Avg. land transferring fees (*yuan*/m^2)[d]	2,055	1,136	700	1,517
Avg. compensation (*yuan*)[e]	338,274	131,947	-110,544	166,660
Participation rate (%)	47.5	14.0	8.0	19.3

Source: Own survey; Jiaxing Municipal Bureau of Land and Resources; program policy documents of the pilot towns.
Notes: All statistics correspond to participating households exchanging homestead for apartments, except for participation rate, which includes all participants.

[a] Compensation for the homestead *per se*, excluding other minor compensation items such as rental subsidy during the transition period, moving subsidy, and rewards for moving on time.
[b] Depreciated to 2009 using a conventional depreciation rate of 2%.
[c] The number of household members is adjusted by counting single child as two people.
[d] To calculate this number, I obtained the complete 2008-2009 auction records of land zoned for urban real estate development in the four towns, and divided the sum of land transferring fees by the sum of lot size.
[e] This is the marketized value of total compensation for a representative participating household of the respective towns, that is with the average family size (added by one to adjust for single child), average homestead area, and average net compensation for the old house. I also assume that the new apartments purchased by the household is of the maximum allowable floor space (homestead are + 20m^2 is used for *Yuxin*). The focus should be on the *relative* rather than the *absolute* values, because the unit market price of apartments is proxied by land transferring fees, while the true price should also incorporate the construction cost and a decent markup.

program more appealing for households that constructed houses earlier (by 1.7 years) and at a cheaper cost (by around 20,000 *yuan*) on average (see Table 1.2).[38]

The second part of compensation is embodied in discounted new apartments, subject to limits on floor space. The extent of this discount can be assessed by comparing the land transferring fees, a proxy for the apartments' market value, against the offered price.[39] Clearly, *Yuxin* is by far the most generous, followed by *Longxiang*, and then *Yaozhuang*, while *Xincang* lags way behind. By combining the two parts, we can get the total compensation for a representative household, which has *Yuxin* at the top, *Longxiang* and *Yaozhuang* in the middle, and *Xincang* at the bottom. The resulting participation rates follow the exactly reverse order, which highlights the crucial role of compensation in households' participation choice. Table 1.2 also shows that homestead area, which is the most important basis in calculating both parts of compensation, is larger for participating than for non-participating households.[40]

Besides the one-time compensation, V is negatively affected if the household derives an income stream from the old house which is likely compromised by relocation. For this reason, the take-up rate is lower for households carrying out business activities on

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[38] Some households were planning on rebuilding their houses even absent the program, and were among the quickest to respond to the program. Strictly speaking, the historical cost of the old house should not be deducted in computing their net compensation, which makes the entire compensation amount something like a windfall gain.

[39] Housing prices at the township level are unavailable. Land transferring fee is the auction price paid to the government for the right to use a lot for a certain term - 70 years in the case of residential development, which is generally the largest component and the most crucial determinant of the final real estate price.

[40] Even for towns that base floor space allowance only on family size, homestead area is still highly correlated with the second part of compensation since it was allocated based on family size in the first place. Participating households in *Xincang* have a relatively smaller average homestead area probably due to its lackluster compensation schedule.

homestead, or collecting a greater house rental income (Table 1.2).[41] Likewise, V also goes up with the improvement in living conditions, which adds to the future consumption stream from housing. Since conditions of new homes such as neighborhood quality and infrastructure are largely fixed, this implies that households living in poorer conditions are more drawn to the program. Table 1.4 confirms this by showing that partipants underperformed on most measured dimensions of amenities of life.

Another prediction of the model that the program better appeals to remotely located households (larger δ) is also strongly born out by participating households' much longer average distance to town center in 2007 (Table 1.2).[42] Plot A in Figure 1.4 illustrates the positive relationship between take-up rate and distance to town center by tracing out an upward sloping convex function. A reduction in this distance not only facilitates job search as in the model, but may also bring a more convenient living for participants, who used to be disconnected from urban shopping and medical resources (Table 1.4). Table 1.2 also reports a higher education level for participants in every town, which confirms the program's stronger appeal to people with larger S.

Regarding the model's ambiguous predictions on income, the summary statistics serve as a good preliminary empirical test. Participating households had a lower

[41] Businesses on homestead typically take the form of mom-and-pop stores, home cooking restaurants, guesthouses, and small workshops. Compared with standard commercial and factory buildings, they have the advantages of zero rent and loose regulation. Relocation implies either discontinuation of such operations, or substitute premises have to be found, usually leased for a rent and subject to stricter regulations. House rental income here refers to proceeds from long-term lodging services targeted at migrant workers, which are quite popular for their cheap rental price, especially in a city with a third of its permanent resident population as immigrants.

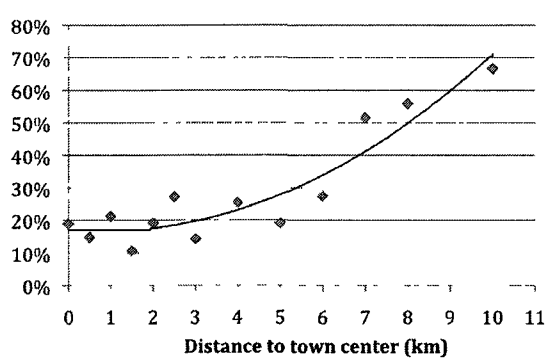
[42] This gap closes up from $1.35km$ to $0.12km$ following the program, which is almost entirely caused by locational change on the part of participating households (Table 1.2). Such a drastic reduction in distance is however an underestimation, since some participants had not moved out of their old residence or were unsure of their exact destination at the time of the survey, hence did not report a change in home location.

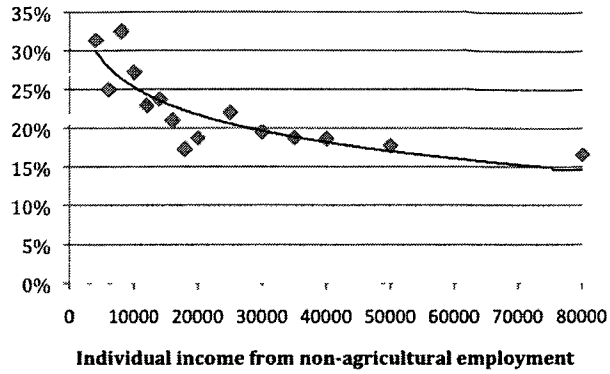
Table 1.4 Percentages of Households Adapted to Modern Facilities

	Entire Sample		*Yuxin*		*Yaozhuang*		*Xincang*		*Longxiang*	
	Participant	Non-participant	Participant	Non-participant	Participant	Non-participant	Participant	Non-participant	Participant	Non-participant
Primary cooking energy is not firewood (2007)	48.3	82.4	27.1	93.2	91.7	90.9	68.3	75.8	71.2	75.8
Buy clothes and big-ticket items mainly from county or city shops	19.4	33.4	7.5	19.3	36.1	38.9	29.3	27.4	36.7	44.5
Go to municipal or county hospital for minor illness	5.6	12.1	2.0	4.8	13.9	16.9	7.3	13.6	10.1	9.3
Do laundry mainly by washing machine	18.6	17.3	18.7	14.5	8.3	11.7	24.4	21.0	20.3	20.1
Access to internet at home	32.9	41.8	35.2	44.4	33.3	39.2	34.1	52.2	26.3	28.4

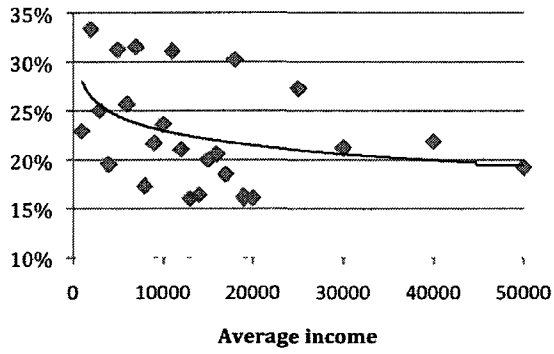
Source: Own survey.

Notes: Except for the first row, which corresponds to the situation in 2007, all other items are situation at the time of the survey (2010). Since some of the participating households had already completed relocation by that time, their living conditions prior to the program, which is what we are actually concerned about, are likely to be worse still. I did not include source of drinking water because all towns have a rural tap water coverage rate of over 95%, which compresses possible between-group difference to a minimal level. A slightly higher tap water coverage is still registered for non-participating households in all towns except *Xincang*.





Plot C: Participation rate against household income averaged by family size



Plot D: Participation rate against family size[a]

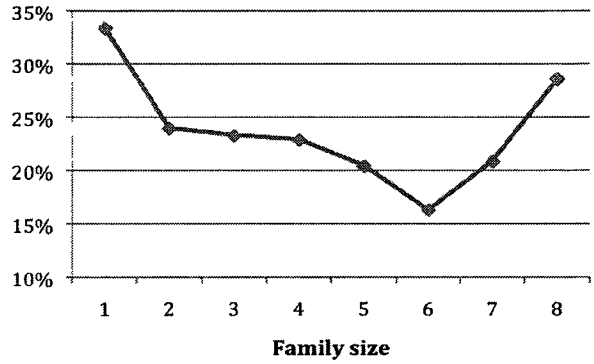


Figure 1.4 Scattor Plots of Participation Rate against Explanatory Factor

Source: Own survey.
Notes: The solid curve in Plot A is a fitted polynomial trend line. Solid curves in Plots B and C are fitted logarithmic trend lines.

[a] Only two families in the entire sample have nine members each, neither of which participated in the program. These two observations are excluded from the plot to avoid small sample bias.

agricultural income than non-participating households in all towns except *Yuxin* - the town where participants' distance to town center were also exceptionally high in 2007. Participants also had a lower non-agricultural income both at the household and the individual levels, with Plot B in Figure 1.4 tracing out quite a smooth downward-sloping curve. Together these result in a lower proportion of non-agricultural income in total

labor income for participating households. Disaggregation by towns however generates a few exceptions.[43]

Total household income is lower on average for participants in all towns except *Xincang*. A closer look in Plot C, Figure 1.4 suggests an overall negative correlation between the take-up rate and household income averaged by family size, yet the dispersion of the points refutes a clear relationship. This probably results from the many factors that simultaneously affect total income and participation choices. Table 1.5 supports this view by showing that households with different income levels have diverse reasons for participation, and Table 1.6 suggests that the take-up rate with respect to households' relative economic standing follows a U-shaped curve: highest for the lower middle group, and lowest for the richest and the poorest groups. The program's greater popularity with poorer households implies it is overall progressive. Moreover, 18.8 percent non-participating households claimed to be deterred by unaffordability, which implies that the participation rate of poorer households would be higher, had they not been financially constrained.[44]

Basic demographic characteristics may also influence the willingness to participate by affecting V through idiosyncratic preferences. Larger families may find it harder to reach a consensus on participation, yet the converse can be true as the multi-generation-under-the-same-roof lifestyle quickly goes out of favor. Other things equal, younger

[43] Table 1.2 reports arithmetic means. In the case of income, a variable with a minimum value but unrestricted at the upper end, it is likely to be right-skewed by outliers.

[44] Even with compensation from the program, households typically have to top it up with own savings, which may go beyond some households' affordability. This is corroborated by the fact that apartment, which generally involves much less out-of-pocket expenditure, is favored by poorer households, while house is preferred by the richer (Table 1.6). Also, since relocation under the program also entails an urban real estate investment, the poorer hence more risk-averse households may choose not to make it even if they can afford it.

Table 1.5 Percentage of Households with Various Reasons for Participation in the Program, by Income Group

Household income in 2007	Reason for Participation							
	Old house is too shabby, want to improve living conditions	Easier access to work	More convenient for life	Child growing up or getting married	Closer to child's school	For rental income or appreciation in property value	Neighbors have moved, so jumped on the bandwagon	Other
≤ 5000	8.3	0.0	0.0	0.0	0.0	0.0	66.7	25.0
> 5000, ≤ 10000	20.0	10.0	0.0	10.0	0.0	0.0	60.0	0.0
> 10000, ≤ 20000	47.1	0.0	0.0	11.8	0.0	0.0	35.3	5.9
> 20000, ≤ 30000	27.5	0.0	2.5	15.0	2.5	2.5	42.5	7.5
> 30000, ≤ 40000	36.8	0.0	5.3	18.4	0.0	0.0	31.6	7.9
> 40000, ≤ 50000	37.8	0.0	6.7	15.6	4.4	2.2	28.9	4.4
> 50000, ≤ 60000	46.9	3.1	9.4	18.8	0.0	0.0	18.8	3.1
> 60000, ≤ 70000	42.3	3.9	15.4	11.5	3.9	0.0	15.4	7.7
> 70000, ≤ 80000	21.4	0.0	7.1	28.6	0.0	0.0	21.4	21.4
> 80000, ≤ 100000	12.5	0.0	25.0	12.5	0.0	0.0	37.5	12.5
> 100000	23.5	5.9	5.9	17.7	11.8	5.9	23.5	5.9
Total	34.6	1.6	6.9	15.5	2.0	0.4	32.5	6.5

Source: Own survey.

Notes: The top three reasons for participation in the program are "old house is too shabby, want to improve living conditions", "neighbors have moved, so jumped on the bandwagon", and "child growing up or getting married". The first reason is the most popular with middle-income households. The bandwagon effect is more salient for poorer households, which corresponds to the firmly upheld behavioral mode in rural areas to "follow the wise men", with the "wise men" generally being the better off. The wealthiest households also appear to be more inclined to follow the trend, probably because they had already moved principal residence to the city/town, hence less acquainted with this rural program. Households choosing "child growing up or getting married "are evenly distributed across various income levels. Statistics in Table 1.2 fail to reveal a significant effect of unmarried household members, perhaps because households have very different cutoff ages for "grown-up". Richer households also seem to value convenience of life more than poorer ones. The percentage numbers may not sum up to 100% due to rounding errors.

Table 1.6 Percentage of Households with Different Choices in the Program, by Self-Evaluated Economic Standing in the Village

	Self-evaluated economic standing in the village					
	Very rich	Above average	Average	Lower middle	Very poor	Total
Non-participating	90.0	82.5	80.3	73.3	87.3	79.8
Apartment	5.0	2.8	5.7	11.8	9.1	6.3
House	0.0	13.8	13.5	13.9	3.6	13.1
Monetary compensation	5.0	1.0	0.5	1.0	0.0	0.7

Source: Own survey. Statistics are calculated based on respondents' answers to a multiple-choice question in the survey, "how do you rank your economic standing in the village?" The choices offered correspond to the five scales identified in the table.
Notes: The total non-participation rate is slightly different from that in Table 1.2 because one household did not answer this question. Statistics at the township level are not calculated, because the sample sizes are too small to generate stable results. While savings may be a good alternative measure of financial capability, such data proved difficult to collect, as participants failed in general to give the counterfactual number (that is, absent the program) with acceptable precision. Also, respondents generally have a vague memory of savings before the program, which is not reliable. The percentage numbers may not sum up to 100% due to rounding errors.

people are more willing to move in general.[45] Households with unmarried youths likely get even more motivated, given the social norm for parents to prepare a new home for the newlyweds.[46] Households with cadre or party members may feel obliged to take the lead and/or enjoy some informational advantage that bestows on them more confidence in the program. Summary statistics in Table 1.2 however do not lend much support to the

[45] The reason is twofold. First, relocation generally imposes a greater psychic cost on the elderly, while younger people aspire more eagerly to urban life. The second reason is economic. Older generations are on the whole less educated and physically weaker (lower S), nor do they have an edge on working experience, since the industrial sector was opened up to rural residents on a massive scale only around mid-1990s. On the other hand, older people have accumulated greater farming skills (s) on average through experience. These, coupled with their diminished learning ability and a shorter remaining working life, dim their urban employment prospects.

[46] This norm is especially espoused in rural areas of China. While the responsibility typically fell on the groom's family in the past, gender matters much less now with both sides being the only child or having at most one sibling, owing to the family planning policies since late 1970s.

theoretical reasoning. Participating and non-participating households differ only marginally in terms of family size, average age, and member composition, and the difference is not always of the expected sign. This suggests that either basic demographic characteristics are not decisive, or something is missing from simple averages. Indeed, the U-shaped curve in Plot D, Figure 1.4 points to a non-linear relationship between family size and willingness to participate.[47]

1.4.2 *Logit Regression*

The previous section uncovers some important factors in households' reaction to the program. In this section, I will estimate a logit model by using the participation choice as the dependent variable. The baseline case groups observations by households. The single-person household in the model can be easily generalized to a multi-person household with aggregate utility function $U = \sum w_i Eu_i$, where i indexes members of the household, w_i is her weight in the household utility function, and Eu_i is her individual expected utility. The basic regression specification is

$$\Pr(\omega_j = 1) = \Lambda(A_j, C_j, L_j, d_j, N_j, \Gamma_j) \tag{1.2}$$

where $\omega_j = 1$ if household j participates in the program, and = 0 otherwise. A_j is a set of basic demographic variables, C_j includes all the compensation-related variables,[48] L_j

[47] Family size is correlated with many other factors such as the allotted homestead area and the construction year of the old house. Larger families also comprise more generations on average, hence are more likely to have elder members and unmarried youths. Thus, the gross impact of family size may indirectly incorporate the effects of these factors.

[48] Ideally the compensation amount can be included as a regressor. This is however empirically infeasible since value of the new houses cannot be estimated for reasons raised before. The case of non-participants is even trickier, as it is impossible to determine *a priori* their favorite option among the three, each of which employs a distinctive compensation schedule.

represents other housing-related gains and losses such as improvement in living conditions, d_j is the household's distance from town center, N_j contains employment- and income- related variables which also serve as proxies for household members' skills levels, and Γ_j encompasses all other relevant factors. The monetary terms are added by one and then taken natural logs, with dummy variables added for the zeros to capture any possible special effect. All regressions include a complete set of town fixed effects.

Results for the baseline regression are reported in column 1, Table 1.7. Regarding basic demographic factors, having at least one family member who is or has been a cadre has a positive impact. The (unreported) impact of family size seems to be non-linear indeed, with opposite signs for the coefficients of the first- and second- order terms. Also unreported are the coefficients on the indicator variables of older members and unmarried adults, which have the expected signs but insignificant.

All compensation-related variables are significant and of the expected signs. Both construction year and construction cost of the old house, which proxy for the old house value to be netted out from the compensation amount, have negative effects. Having controlled for these, the size of homestead area, the most important factor in compensation determination, has a strongly positive effect. Other housing-related variables also come out as important factors. Households that solely used firewood as cooking energy and drank from shallow wells, both indicators of substandard living, are strongly inclined to participate. Households running business on homestead are strongly discouraged from participation. House rental income has a positive effect, which is quite counterintuitive, but the coefficient is statistically and economically very insignificant.

Table 1.7 Logit Regression Estimates: Participation Decision

Dependent variable	Participation choice (1 = participating)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Household		Household (*Yuxin*)		Decision maker		Working decision maker	
Has cadre member[a]	0.343^{+}	0.361^{+}	0.217	0.711	0.412**	0.424*	0.646**	0.545*
	(0.179)	(0.215)	(0.505)	(0.628)	(0.160)	(0.193)	(0.226)	(0.256)
House construction year	-0.022^{+}	-0.042*	-0.070^{+}	-0.144*	-0.032**	-0.057**	-0.031^{+}	-0.051*
	(0.013)	(0.018)	(0.039)	(0.060)	(0.012)	(0.017)	(0.018)	(0.022)
Construction cost	-0.215*	-0.143	-0.012	0.027	-0.206*	-0.129	-0.218^{+}	-0.189
	(0.098)	(0.120)	(0.245)	(0.365)	(0.090)	(0.111)	(0.124)	(0.141)
Homestead area	0.347*	0.303	1.205^{+}	1.256	0.375**	0.368^{+}	0.231	0.431^{+}
	(0.151)	(0.216)	(0.731)	(0.866)	(0.144)	(0.213)	(0.190)	(0.251)
Cooked solely with firewood	0.745**	0.450^{+}	4.314**	4.268**	0.946**	0.680**	0.931**	0.594*
	(0.209)	(0.244)	(0.755)	(0.885)	(0.185)	(0.219)	(0.253)	(0.284)
Drinking shallow well water	1.118*	0.860	1.089	1.002	0.793*	0.480	1.197*	1.211^{+}
	(0.461)	(0.606)	(0.896)	(1.041)	(0.400)	(0.523)	(0.603)	(0.682)
Business operation on homestead	-1.545**	-1.473**	-1.824	-0.947	-1.601**	-1.387**	-1.980**	-1.310^{+}
	(0.442)	(0.511)	(1.033)	(1.126)	(0.372)	(0.431)	(0.603)	(0.632)
House rental income	0.039	0.141	-1.052	0.486	-0.003	0.160	-0.068	0.167
	(0.334)	(0.111)	(1.429)	(0.398)	(0.304)	(0.099)	(0.375)	(0.118)
Rental income squared		-0.012		-0.022		-0.013^{+}		-0.013^{+}
		(0.007)		(0.020)		(0.007)		(0.008)
Distance to town center[b]	0.129**	0.141*	0.152	0.254^{+}	0.116**	0.136**	0.158**	0.151*
	(0.046)	(0.055)	(0.128)	(0.155)	(0.040)	(0.049)	(0.054)	(0.061)
Has full-time farmer	-0.533**	-0.538*	-1.886**	-1.660*	-0.181	-0.245	-0.168	-0.366
	(0.202)	(0.234)	(0.572)	(0.710)	(0.200)	(0.228)	(0.249)	(0.272)
Agricultural income	-0.100	-0.155	-0.213	-0.581^{+}	-0.163*	-0.253**	-0.133	-0.141
	(0.088)	(0.106)	(0.239)	(0.325)	(0.080)	(0.096)	(0.113)	(0.119)
Self-farmed area	0.030*	0.100**	0.110	0.192*	0.033**	0.124**	0.068*	0.091**
	(0.012)	(0.028)	(0.078)	(0.096)	(0.012)	(0.025)	(0.030)	(0.031)
Household non-ag. income	0.056	-0.035	-0.489	-0.520	-0.015	0.047	-0.289	-0.216
	(0.158)	(0.184)	(0.376)	(0.443)	(0.140)	(0.165)	(0.258)	(0.284)

Table 1.7 (Continued)

Age					-0.003	0.002	-0.029^{+}	-0.032^{+}
					(0.010)	(0.012)	(0.016)	(0.017)
Years of schooling					-0.005	-0.007	-0.015	-0.020
					(0.028)	(0.033)	(0.041)	(0.045)
Years in non-ag. employment							0.034**	0.024^{+}
							(0.012)	(0.014)
Employment quality = 4							0.459^{+}	0.698*
							(0.276)	(0.315)
Town fixed effect	Yes	Yes	-	-	Yes	Yes	Yes	Yes
Dummies for zero values of the logged variables	Yes	-	Yes	-	Yes	-	Yes	-
Other household-level control variables	Family size, family size squared, has old member, has single adult, knowledge of policy, self-evaluated financial status in village, location of old principal residence, pre-program construction/purchase plan of property, cooked with some firewood, drinking shallow well water, farm rent							
Other individual-level control variables					Gender, marital status, occupation		Gender, marital status, vocational training & certificate, workplace location, industry	
Pseudo R^2	0.3512	0.3675	0.6035	0.6520	0.3693	0.3994	0.4231	0.4384
N	1,571	1,149	352	264	2,120	1,567	1,365	1,099

Notes: A plus sign ($^{+}$) denotes a significant coefficient at the 10% level, one star (*) denotes significance at the 5% level, two stars (**) at the 1% level. All regressors correspond to conditions in 2007 (the base year). Variables with the same names as those in Table 1.2 also share the same definition. The monetary terms are logged, including: house construction cost, housing rental income, agricultural income, farm rent, household non-agricultural income, and individual non-agricultural income. In columns 1-4, they are first added by one and then taken natural logs, with dummy variables for the zeros includedas regressors (coefficients are in general insignificant and not reported here). In columns 5-8, they are taken natural logs directly, with the zero observations dropped.

[a] For individual-level regression, replacing this with an indicators variable denoting whether the decision maker is or has been a cadre before gives similar but less significant coefficients.

[b] Inclusion of a squared term of this distance does not add much explanatory power, with the coefficient insignificant and economically small, therefore the results shown here have excluded it.

Table 1.7 (Continued)

Definition of selected explanatory variables:

Rental income: columns 1-4 use $\log(HR_j + 1)$, where HR_j is household j's housing rental income in 2007. Columns 5-8 use HR_j *per se* (measured in 1,000 *yuan*), because many households reported zero rental income in 2007 and directly taking logs would exclude all of them, which greatly reduces the number of observations. Another regressor, farm rent, is calculated in the same way.

Cooked solely with firewood, Cooked with some firewood: two indicator variables, with the former meaning the household used firewood as the only cooking energy, which is usually associated with very poor living conditions. and the latter meaning the household used a combination of firewood and tanked gas as cooking energy. The omitted default is "cooked solely with tanked gas".

Drinking artesian well water, Drinking shallow well water: indicator variables for the households' main drinking water source. The omitted default is tap water. Artesian well water is superior to shallow well water in hygienic sense.

Self-farmed area: area of farm cultivated by the household measured in *mu*. This is the sum of farm area allotted to the household and that rented from others, net of farm rented out.

Knowledge of policy: a 1-4 scale variable with larger numbers denoting poorer knowledge of the policy, based on the respondent's answer to a multiple-choice survey question on compensation options. 1 means she picked all the correct choices, 2 means she picked some but not all the correct choices, 3 means she picked some correct and some incorrect choices, 4 means she only picked incorrect choices or admitted nil knowledge of the program.

Employment quality: a 1-4 scale variable with larger numbers denoting poorer employment quality (or job security). 1 means having a formal job in the formal sector, 2 means having a formal job in the informal sector, 3 means having an informal job in the formal sector, 4 means having an informal job in the informal sector. Only coefficients for the lowest quality level is reported in the table.

Distance to town center has a strongly positive effect on participation, reflecting the desire of remotely located households to make up for the geographic gap. When distance is controlled, presence of full-time farmers, which represents greater farming skills (s), has a very significant negative effect. Agricultural income also has a negative effect as predicted, though insignificant. Self-farmed area has a significantly positive effect, probably because having controlled for agricultural income, a larger area size implies a smaller unit output, which corresponds to poorer farming skills. Finally, household non-

agricultural income has a positive effect, which runs against the model prediction, yet the effect is very insignificant.[49]

Column 2 is a reproduction of column 1, but has the monetary terms logged directly with zero observations omitted, which leads to a sizeable drop in the number of observations. The results are qualitatively the same, except that the coefficient on non-agricultural income now has the expected negative sign, though still insignificant.

While inclusion of town fixed effects should absorb most variation at the township level, to address the concern that the explanatory variables have different impacts in different towns, I also ran regressions for each town separately.[50] The results do not vary qualitatively from regressions on the pooled sample, although in general less significant due to much smaller sample sizes. Columns 3 and 4 give the regression results for *Yuxin* using the same set of variables as in columns 1 and 2. Noticeably, the coefficients on homestead area are much larger now, which accords well with the especially munificent compensation policies of this town.

While preceding regressions take households as the unit of analysis, it may be the case that the participation decision is made by a few key household members, who bring individual benefits into play along with collective interest. This can be modeled by assigning $w_i = 1$ to decision makers, and 0 otherwise in the aggregate household utility function $U = \sum w_i E u_i$. Columns 5-8 estimate a logit model similar to those in columns 1-2, but with household decision makers as the unit of analysis:

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[49] Total household income is excluded to avoid multicollinearity. Alternatively, only using this variable with all other income items excluded generates a significantly negative effect, which suggests that the program on the whole has a greater appeal to poorer households.

[50] This is equivalent to interacting all regressors with town dummies.

$$\Pr(\omega_i = 1) = \Lambda(A_j, C_j, L_j, d_j, N_j, \Gamma_j, X_i) \tag{1.3}$$

$\omega_i = 1$ if individual i is a decision maker in a participating household, and = 0 if in a non-participating household. All the explanatory variables from column 1 are kept, with the addition of some individual-level variables, represented by X_i. Column 5 includes a parsimonious set of basic individual characteristics. Column 7, by restricting the sample to decision makers who worked in the non-agricultural sector in 2007, can accommodate a much fuller set of individual-level regressors.[51] Columns 6 and 8 are reproductions of columns 5 and 7 respectively, with the monetary terms treated in the alternative way.

Coefficients on all the household-level regressors are largely comparable to previous regressions both in magnitude and significance. Concerning individual characteristics, age has a negative effect and is significant in some specifications, which confirms that older agents are less willing to relocate. Years of schooling have a negative coefficient, though small and insignificant. This is possibly because the decision makers, being middle and old aged in general (the average age is almost 50, see Table 1.2), went through a period with major disruptions to the education system right at school age, which weakens the link between years at school and skill level. Years in non-agricultural employment, which proxies for experience with the urban sector, seems to be a better measure of S and has a significantly positive effect. Finally, agents with inferior employment quality are more willing to participate, which testifies to the beneficial effect of relocation in easing labor market access.

[51] A regression using only individual-level variables generates very similar coefficients, but with a much smaller R^2, which highlights the importance of collective household interests. As a sensitivity check, I also ran the two regressions on all sample individuals, and on a sample of only the principal respondents, both generating fundamentally the same results.

1.4.3 *Section Summary*

Findings in this section conform very well with the model predictions. Moreover, it seems households that are on the whole worse off (in terms of living conditions, income, job security, etc.) are more likely to participate in the program. This is a desirable outcome because as long as the decisions are made out of own will, poorer households must have found the program to their advantage, which casts it in a redistributive role.

One complication is that despite the government's claim of utterly unforced choice by households, we cannot dispel the concern that coercion or coaxing is still practiced to prompt participation. Members of poorer households, who in general are less capable of challenging the authority and carrying out informed independent analysis, more easily fall subject to such manipulation, resulting in their seemingly higher participation rate. However, household interviews argue for the opposite. Moreover, three of the four sampled pilot towns (except *Xincang*) have all reported a larger number of applicants than expected, which necessitated adjustments in their construction plans to accommodate all applicants. It is unlikely that coercion was still actively exercised under such excess demand. Our analysis in the next section also supports this assumption.

Readers also have to be reminded that instead of establishing a complete market for rural property rights, the program rather lifts the ban on trading of such rights to a limited extent, with the government being the sole player on the demand side. A natural implication of such a monopsony is that the price offered, namely compensation in our case, is different from that in a free market, which brings about different outcomes. It is conceivable that households with superior location and housing quality are more willing to relocate if fair market value is offered. While this may be a good theoretical exercise,

policy experiments on rural property right nationwide have all taken a similar stance in this respect, and a fundamental change is unlikely in the near term. Therefore, examination of quasi-market outcomes is practically more relevant.

1.5 Program Impact on Occupation and Income

Having investigated the first-stage problem of who participates in the program, this section will test the model's predictions on the program's occupational and income implications. We see from Table 1.2 that while the average agricultural income of non-participating households increased by around 10 percent from 2007 to 2010, that of participating households was almost halved.[52] In contrast, participating households experienced a greater increase in non-agricultural income over the same period. Consequently, the sign of the difference between non-agricultural to total labor income ratios of participating versus non-participating households is reversed, while the 10-percentage-point magnitude is kept. The growth in total household income and individual non-agricultural income, however, is smaller for participants than non-participants. Finally, participants experienced a much stronger upsurge in the take-up rate of urban jobs, and the percentage gap as full-time farmers is likely the result of a program-induced fall on the part of participants. While the simple averages serve as a good point of departure, the fact that participants and non-participants were drawn from different parts of the prior distribution means a valid test also has to take the pre-conditions into consideration. To this we now turn.

[52] Such a drastic fall is largely the result of many participating households quitting farming altogether. While a slightly higher percentage of participating households (85.1%) reported positive agricultural income in 2007 than non-participating households (79.3%), the figures become 32.9% vs. 70.9% in 2010.

1.5.1 *OLS Regressions*

I start with household-level regressions, and use three dependent variables: non-agricultural income, agricultural income, and total income, which is the sum of the preceding two income items plus rents and reliefs (same as in Table 1.2). The basic specification is

$$y_{jt} = b_1 + b_2 y_{jt-1} + b_3 P_j + b_4 D_j + \varepsilon_j \tag{1.4}$$

where y_{jt} is the current (time t) value of the dependent variable of household j, and y_{jt-1} is its base year (2007) equivalent.[53] This follows from the literature on income evolution and income persistence (for a good review, see Benjamin, Brandt and Giles, 2002; or Jenkins, 2000), and should have incorporated factors that affect both the outcome variable and the participation choice, thereby ruling out spurious correlations in this regard. P_j is an indicator variable which equals 1 if household j participates and 0 otherwise. D_j is a vector of control variables, and ε_j is an error term.

For each dependent variable, D_j includes the head counts of the corresponding income earners, namely the number of off-farm workers for non-agricultural income, full-time farmers for agricultural income, and both for total household income. In addition, family size is included in all regressions to control for other income sources, such as welfare benefits, as well as to account for the possibility that having members specializing in housework helps others focus on income-earning activities.[54] For

[53] Transformation of the income terms follows the baseline case in Section 2.11.4.2, i.e. $y_{jt} = (Y_{jt} + 1)$. OLS and 2SLS (later) estimates using the alternative of taking logs directly and dropping the zero observations differ in absolute values, but keep the signs and significance levels in general.

[54] Exclusion of disabled individuals from this head count generates almost identical results. Similarly, inclusion of the number of household members receiving pension payments as an additional control variable does not qualitatively change the results, either.

regressions on agricultural income and total income, a full set of agriculture-related control variables is also included as explanatory variables.

Regression results are reported in Panel A, Table 1.8. A baseline case is first estimated for each dependent variable by dropping D_j, which leaves P_j as the only exogenous disturbance.[55] The resulting estimates suggest that participation in the program significantly increases non-agricultural household income by 37.7 percent (column 1) and reduces agricultural income by 91.6 percent (column 4). Overall household income insignificantly decreases by 5.8 percent (column 7). Regressions with control variables (columns 2, 5, and 9) generate similar results, except that the absolute values of coefficients on P_j are somewhat smaller for non-agricultural and agricultural incomes, and slightly larger for total household income. The other explanatory variables also behave as expected.[56]

These results are consistent with the model in demonstrating a shift in participating households' earnings capacity from the agricultural to the non-agricultural sector, yet by summing over all members, this mixes up program impacts at the *intensive* and the *extensive* margins. For example, a rise in household non-agricultural income could stem from greater efforts of full-time workers and redistribution of time toward off-farm activities by double-jobbers (the intensive margin), alternatively it could be brought about by new entrants into the urban sector who used to be full-time farmers or jobless (the

[55] In the baseline cases with non-agricultural income and agricultural income as the dependent variable (columns 1 and 4), the lagged values of both are included as explanatory variables for symmetry. Estimated coefficients using only the lagged value of the respective dependent variable as a regressor are very similar in magnitude and significance.

[56] Family size has a significantly negative effect on agricultural income, probably because own consumption of farm products increases in family size, leaving fewer for sale.

Table 1.8 Program Effects on Income and Occupation: Linear Regressions

Panel A: Household-Level Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable	Non-agricultural income			Agricultural income			Total income		
Type of regression	OLS	OLS	2SLS	OLS	OLS	2SLS	OLS	OLS	2SLS
Participating	0.320**	0.233*	2.063*	-2.477**	-1.088**	-3.247*	-0.060	-0.082	0.804+
	(0.122)	(0.110)	(0.890)	(0.213)	(0.172)	(1.612)	(0.056)	(0.056)	(0.459)
2007 non-agricultural income	0.771**	0.609**	0.629**	-0.014	0.017	0.004			
	(0.029)	(0.036)	(0.036)	(0.021)	(0.022)	(0.025)			
2007 agricultural income	0.019	0.015	0.015	0.857**	0.643**	0.675**			
	(0.011)	(0.010)	(0.012)	(0.017)	(0.023)	(0.038)			
2007 total income							0.830**	0.708**	0.697**
							(0.068)	(0.081)	(0.080)
Family size		0.017	0.028		-0.124*	-0.092		0.003	-0.005
		(0.033)	(0.037)		(0.052)	(0.062)		(0.016)	(0.019)
Number of workers		0.819**	0.761**					0.266**	0.278**
		(0.077)	(0.078)					(0.049)	(0.052)
Number of farmers					0.331**	0.300**		0.070*	0.072*
					(0.071)	(0.080)		(0.028)	(0.031)
Town fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls for column 5, 6, 8, 9	Paddy field area, dry farmland area, other agricultural land area, farmland leased out, farmland leased in, cash crop[a], husbandry[b]								
Adjusted R^2	0.7223	0.7631	0.7178	0.6322	0.7419	0.7092	0.6219	0.6582	0.6004
N	1,675	1,675	1,668	1,674	1,671	1,664	1,643	1,640	1,634

[a] A dummy variable which is one for households growing cash crops (usually oilseed rape, vegetables, fruits, seedling and flowers in the area of Jiaxing) in 2010, and zero for those only growing grain crops (predominantly rice, and possibly wheat and potatoes) or nothing at all.
[b] A dummy variable for those raising animals for sale in 2010. Households raising animals for own consumption only are not counted.

Panel B: Individual-Level Regressions (Table 1.8 Continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Dependent variable	Non-agricultural income			Non-agricultural wage			Employment quality			Participation rate in non-agric. sector	
Type of regression	OLS	OLS	2SLS	OLS	OLS	2SLS	OLS	OLS	2SLS	Probit	2SLS probit
Participating	0.015	0.009	0.557*	0.019	0.018	0.243	-0.024	-0.014	-0.208	0.127*	0.459**
	(0.014)	(0.013)	(0.245)	(0.013)	(0.013)	(0.224)	(0.024)	(0.024)	(0.312)	(0.059)	(0.178)
2007 non-agricultural income	0.835**	0.793**	0.813**	0.791**	0.724**	0.731**					
	(0.017)	(0.021)	(0.024)	(0.021)	(0.027)	(0.028)					
2007 emp. quality							0.924**	0.890**	0.891**		
							(0.007)	(0.010)	(0.011)		
Gender = F		-0.031**	-0.039**		-0.017	-0.021^{+}		-0.010	-0.006	-0.172**	-0.309**
		(0.011)	(0.015)		(0.011)	(0.013)		(0.022)	(0.023)	(0.048)	(0.052)
Age		-0.004**	-0.005**		-0.004**	-0.005**		0.002	0.002^{+}	-0.025**	-0.055**
		(0.001)	(0.001)		(0.001)	(0.001)		(0.001)	(0.001)	(0.002)	(0.003)
Years of schooling		0.003	0.0004		0.005*	0.003		0.00001	0.001	0.164**	0.054**
		(0.002)	(0.003)		(0.002)	(0.003)		(0.004)	(0.004)	(0.006)	(0.009)
Years on current job		0.002*	0.002^{+}		0.003**	0.003**		0.00008	0.00002		
		(0.001)	(0.001)		(0.001)	(0.001)		(0.001)	(0.001)		
2010 distance to town center										-0.026*	-0.034**
										(0.011)	(0.012)
Town fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Non-baseline controls	Marital status, if is decision maker, if is party member, if is or has been cadre, if has been soldier; Years in non-ag. work, number of non-ag. jobs taken, vocational training, vocational certificate, workplace location										
Adjusted R^2	0.6927	0.7800	0.6229	0.7585	0.7789	0.7423	0.8545	0.8674	0.8632	0.3070	-
N	3,204	3,078	3,067	2,421	2,345	2,340	3,219	3,093	3,082	5439	5395

Notes: A plus sign ($^{+}$) denotes a significant coefficient at the 10% level, one star (*) denotes significance at the 5% level, two stars (**) at the 1% level. All income variables are transformed as in the baseline regressions in Table 1.7, i.e., added by one and then taken natural logs. 2SLS estimates use distance to town center (2007) as the instrument variable. LIML regressions generate estimates that are very close to 2SLS.

extensive margin). This is addressed by regressions at the individual level.

To examine program effects at the intensive margin, I employ a similar regression setup as before - first the baseline and then with an extensive set of control variables, but this time on a sample of individuals with paid non-agricultural jobs in both 2007 and 2010.[57] I first run regressions on the entire sample, using non-agricultural income as the dependent variable. As the program offers monetary compensation and collaterable assets, which may help relax entrepreneurs' credit constraint yet largely irrelevant for wage earners, I then restrict the sample to wage earners as opposed to the self-employed and business owners to tease out the program's impact absent this effect. While the theoretical model concludes here, the empirical evaluation of employment does not take income as the only gauge. For instance, some jobs may feature higher expected remunerations, yet involve greater unemployment risks and income volatility, which impairs its desirability. To assess the program's impact on job security, I next use the 1-4 scale employment quality (higher scale denotes lower quality) measure as the dependent variable.

Results are reported in Panel B, Table 1.8. Baseline specification (columns 1, 4 and 7) suggests that participation in the program has a favorable effect on all three measures, though insignificant. This is corroborated by estimates with control variables (columns 2, 5 and 8). Selected coefficients on control variables indicate that women and old people earn a lower non-agricultural income on average, while years of schooling and years served on the current job (proxies for S) exert positive effects. Also, having controlled for base-year employment quality, all other regressors seem to have little explanatory power, probably due to the small number of people who had a significant change on this aspect

[57] Agricultural income is not examined here because it is not easily attributable to individuals.

within a short period.

To evaluate the program's effect at the extensive margin, I create an indicator variable for all agents in the labor force at the time of the survey, which takes value one for people with a strictly positive non-agricultural income, and zero otherwise.[58] A one-period probit regression is then run with this as the dependent variable.[59] In addition to individual characteristics, distance to town center in 2010 is also included as a control variable to make the results more comparable to the 2SLS regression later. While estimates at the intensive margin are small and insignificant, participation in the program significantly increases the take-up rate of urban employment by 12.7 percentage points (column 10), exactly as the model predicts.

1.5.2 *IV Regressions*

The OLS regressions give some benchmark results, which fit the model quite well. However, by including a full set of control variables that may at the same time influence households' participation choice, the treatment effect may be underestimated due to *over-controlling bias*.[60] While this is partially addressed by the baseline regressions, a related yet probably more problematic bias is the *omitted variable bias*. With non-random

[58] Retirees, full-time students, people aged under 18 and without a job, and those who lost work ability are excluded from the sample.

[59] Value of this variable is unavailable for the base year due to the lack of 2007 data on labor force participation. A probit model is used here to make the results more comparable to the 2SLS regression analysis later. Results obtained by logit regression are nevertheless very similar.

[60] If we want to estimate the effect of X on Y, and W is dependent on X, then the inclusion of W as a control variable results in over-controlling bias. Examples of W in our case would be control variables that are also affected by the program, e.g. the number of workers versus farmers, the size of self-operated farmland, etc. Thus, the estimated coefficients on Participating may have failed to account for the program's impact through these indirect channels.

selection into the program, some uncontrolled confounding factors may simultaneously affect participation choice and employment outcomes, causing the observed program effects to embody correlation rather than causation. To address both types of bias, we need an instrumental variable that affects households' participation choice, but not the dependent variables conditional on all the other control variables.

One such candidate is distance from home to town center in 2007, which, as we have seen in Section 1.4.2, is a crucial determinant of households' participation decisions. Now the task remains to verify that this variable does not affect the outcome variables conditional on the controls. This is the case as long as the base year value of the dependent variable is controlled for, which should absorb whatever effects past home location may have on employment conditions. Moreover, over 95 percent of the sample households were living in the countryside by the end of 2007, with an overwhelming majority having had their homestead allotted over ten years ago, making it unlikely for some lagging effects to be missing in 2007 and show up in 2010. In Section 1.2.1, we have also precluded the possibility of major recent distance-contingent events bringing in unwanted correlation between the instrumental variable and current employment conditions not captured by Y_{jt-1}. The case for urban job take-up rate is trickier, since no 2007 value is available. I address this by controlling for distance to town center in 2010. While this may takes away the linear effects of relocation under the program, it should override the instrumental variable with respect to the *current* employment conditions.[61]

[61] For sensitivity check, I also tried controlling for a complete set of village fixed effects in all the 2SLS regression, which should take care of any distance-contingent effects of recent events since households from the same village share similar distance to town. Results are consistent with those in Table 1.8, yet much lower in significance due to the small sample size from each village.

There may be the concern that some participants had advanced vocational responses to before 2007 in anticipation of the program, in the same vein as the Ashenfelter dip (see Ashenfelter and Card, 1985). However, information leakage is unlikely since the motion was first submitted to the municipal council only in late 2007, with no public discussion or hearing held till the launch of the pilot program in early 2008. Nor could households have been heralded by similar programs in other parts of the country, since this one is among the first of its kind nationwide. On-site interviews confirm that households had little prior knowledge of the forthcoming program. Even if forward-looking behavior does occur, it would nevertheless reinforce our results by wrongly attributing some of the program's effects to Y_{jt-1}, which leads to an underestimation of the coefficient on P_j.[62]

Another threat to the validity of this identification strategy is the possibility of trends in outcomes over by distance to town center even in the absence of the program. To verify this is not the case, I take the differences between the 2007 and 2010 values of the outcome variables and run reduced form regressions of them on the 2007 distance to town center and the other baseline covariates, with the sample divided into the pilot towns (treatment) and the non-experimental towns (control). The estimated coefficients

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To eliminate possible biases brought by non-participants that relocated after 2007, I also ran the 2SLS regressions on a selected sample that either participated in the program or kept the same principal residence over the studied period. This generates fundamentally the same results.

A related concern is that if proportionally more non-participants moved home not long before the program and were still in a transition period at the time of the survey, which would temporarily bias their income and occupational outcomes. 2SLS regressions on a sample excluding agents relocated in 2006-2007 generates almost identical results to Table 1.8.

[62] Some households claimed their main reason for non-participation as waiting to be covered in some *chaiqian* program. Behavioral impacts of such expectations potentially bias our results, e.g. if these agents choose to enjoy more leisure currently in anticipation of a large *chaiqian* compensation in the foreseeable future, this may cause participants to appear more committed to work. 2SLS regressions on a sample with these observations excluded however generate very similar results to those in Table 1.8.

on the distance to town center for the treatment towns are strongly significant, while those for the control towns are insignificant and numerically much smaller, which refutes the existence of interim trends for reasons other than the program. Moreover, coefficients for the treatment and control samples are generally of the opposite sign, which indicates that even if such trends are in place, they would work against the program hence corroborate our findings. Regression results are reported in Table 1.9.

Results are reported in Table 1.8. At the household level, 2SLS regressions again indicate that the program exerts a significantly positive effect on non-agricultural income and the opposite for agricultural income, but the coefficients are much bigger than the OLS estimates in absolute value. Other things equal, participation in the program increases household non-agricultural income by almost sevenfold (column 3), and reduces agricultural income by 96 percent (column 6). Such big coefficients are largely the result of participating households that went from little or zero to a decent off-farm income, and those that derived some income from the farm but zero now. Unlike OLS estimates, total household income now increases significantly. While all results so far point to a pronounced program-induced shift of labor from the rural to the urban sector, the overall impact on total income is ambiguous, which, given the income effect of the compensation program, should not be taken as a measure of changes in welfare. Rather, participants may now optimally afford to enjoy more leisure as in Figure 1.2, which keeps their overall household income little changed on average.[63]

[63] Apart from the measured gains in Table 1.3, the urban housing market in Jiaxing is booming these years, which adds to the implicit compensation value and reinforces the income effect. For example, average housing price shot up from 2,500 to 3,600 *yuan* per square meter in *Yuxin* and from 2,000 to 3,000 in *Yaozhuang* over the period of late 2008 to late 2009 (*Dongfang Daily*, December, 2009).

Table 1.9 Validity Test of Distance to Town as Instrumental Variable

Dependent variable (log differences)	Household non-agricultural income		Household agricultural income		Household total income		Individual non-agricultural income	
Sample	Treated	Control	Treated	Control	Treated	Control	Treated	Control
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2007 distance to town center	0.050*	-0.002	-0.146**	0.046^{+}	0.023**	-0.015	0.009**	0.004
	(0.024)	(0.018)	(0.039)	(0.024)	(0.009)	(0.012)	(0.003)	(0.003)
2007 household non-agricultural income	-0.226**	-0.167**	-0.010	-0.008				
	(0.029)	(0.030)	(0.022)	(0.007)				
2007 household agricultural income	0.015	0.002	-0.129**	-0.065**				
	(0.012)	(0.008)	(0.017)	(0.014)				
2007 household total income					-0.170*	-0.182**		
					(0.069)	(0.064)		
2007 individual non-agricultural income							-0.213**	-0.109**
							(0.022)	(0.018)
Town fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.1928	0.1337	0.2981	0.0299	0.0730	0.1383	0.0870	0.0573
N	1,670	1,573	1,669	1,573	1,639	1,514	3,196	3,002

Notes: Results are generated using OLS regressions. The "Treated" sample includes all observations from the four pilot towns, and the "Control" sample includes all observations from the three non-experimental towns. The dependent variables are calculated as $\log(Y_{2010}+1)-\log(Y_{2007}+1)$, where Y_{2007} and Y_{2010} are the 2007 and 2010 values of the corresponding income types. Employment quality is not used as a dependent variable here because differencing a scale variable makes its interpretation more ambiguous, nor is the participation rate in non-agricultural employment used due to the lack of 2007 data. A plus sign ($^{+}$) denotes a significant coefficient at the 10% level, one star (*) denotes significance at the 5% level, two stars (**) at the 1% level.

Individual level regressions suggest that forces at both the intensive and the extensive margins are in effect, while the latter is probably more salient. Participation significantly increases the average non-agricultural income of all people with some urban jobs by 74.5 percent (column 3), and insignificantly for wage earners by 27.5 percent (column 6), which supports the hypothesis of a greater program impact on business owners. Employment quality improves (column 9), though still insignificantly, suggesting that the increment to income is not a trade-off for lower job security. The 2SLS probit regression shows that for an average person in the labor force, the marginal program effect on her probability of taking up an urban job is a significant increase of 51.7 percentage points (column 11) - a sizable effect at the extensive margin.[64]

1.5.3 *Propensity Score Matching*

1.5.3.1 Difference-in-Differences Estimation

While instrumental variable helps correct for over-controlling and omitted variable biases, it is valid only as long as the linear model underlying the regressions is correct. One way to assess the program's impact without imposing any functional form is to use the matching estimator method. As is introduced in Section 1.2.2, I pool observations from the non-experimental towns to form the control group, and program participants in pilot towns for the treated group. While the true average treatment effect on the treated (ATT) is $(Y_1 - Y_0 \mid \omega)$, where Y_1 is the treated and Y_0 the untreated outcome, ω is an indicator variable for treatment status, the matching estimator selects matches for treated

[64] To exclude potential bias from agents who recently joined the labor force from school, I excluded individuals who entered the labor market after 2007 from the sample, and reran the 2SLS probit regression. This time the coefficient on participating is even larger at 0.537, again significant at the 1% level.

observations from the control group based on a set of matching variables, Z, and uses the matched outcomes to proxy for the counterfactual outcome Y_0.[65] The two-stage propensity score matching method is used to counter the curse of dimensionality. In the first stage, all matching variables are integrated into one probability measure $\Pr(\omega = 1 | Z)$, called the *propensity score*, using a logit model. The second stage matches observations in the treated and control groups based on this measure.

For consistency, I use the same explanatory variables for the first stage logit model as in column 1, Table 1.7,[66] supplemented with the same control variables as in column 11, Table 1.8 (excluding distance to town center in 2010) when the outcome variable is at the individual level, because simply matching by household characteristics would result in pairing up individuals from similar families yet with utterly different personal traits. As the inclusion of the dependent variables' 2007 equivalents makes the linear specifications more of a difference-in-differences (DID) analysis, I also set up a DID structure for the propensity score matching estimator for comparability, which involves using $(Y_t - Y_{t-1})$ as the outcome variable when Y is the variable of interest, with t and $t-1$

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[65] The two prerequisites for applying the matching estimator (Rosenbaum and Rubin, 1983) are satisfied: 1) *Ignorability of treatment*: $(Y_0, Y_1) \perp \omega | Z$. This is verified by OLS regressions of the outcome variables (as in Table 1.10) on (Z, ω) and on Z respectively, which suggests that ω has a very insignificant coefficient and almost adds nothing to the R^2 value. 2) *The common support condition*: $0 < \Pr(\omega=1 | Z) < 1$, $\forall Z$ (this is a sufficient but not necessary condition as we are only interested in ATT, see Heckman, Ichimura and Todd, 1998). This is evidenced by Table 1.10 (columns 4 and 5), which confirms that an overwhelming majority of households from both groups are on the common support. The matching method also requires that the distribution of Z not be affected by the treatment, which is not a problem here as all matching variables correspond to conditions in 2007, back when the program was not yet launched.

[66] Instead of taking logs of the income terms, the non-transformed numbers are used for matching purposes. Knowledge of policy is excluded from the list of matching variables because the treated and control groups are incomparable on this dimension.

Table 1.10 Program Effects on Income and Occupation: Propensity Score Matching

	(1) Treated	(2) Controls	(3) Difference	(4) # treated on support[a]	(5) # control on support[a]
Household non-agricultural income					
Treated = Participated in the program	12,087	10,234	1,854	262	1,433
			(1,859)	(329)	(1,484)
Treated = Did not participate in the program	9,757	11,335	-1,579	1,199	1,454
			(1,036)	(1,247)	(1,484)
Differences in differences in differences			**3,432***		
			(1,380)		
Household agricultural income					
Treated = Participated in the program	-5,270	1,248	-6,518**	270	1,435
			(1,033)	(327)	(1,483)
Treated = Did not participate in the program	1,214	532	682	1,221	1,452
			(919)	(1,249)	(1,483)
Differences in differences in differences			**-7,200****		
			(897)		
Household total income					
Treated = Participated in the program	9,290	13,220	-3,930^{+}	255	1,417
			(2,041)	(326)	(1,472)
Treated = Did not participate in the program	11,369	12,185	-816	1,204	1,441
			(1,410)	(1,243)	(1,472)
Differences in differences in differences			**-3,114***		
			(1,563)		
Self-reported household total income					
Treated = Participated in the program	54,310	59,427	-5,116	286	1,441
			(7,346)	(329)	(1,492)
Treated = Did not participate in the program	59,863	67,361	-7,498**	1,219	1,462
			(2,642)	(1,250)	(1,492)
Differences in differences			**2,381***		
			(4,235)		
Individual non-agricultural income					
Treated = Participated in the program	3,739	4,396	-657	444	2,673
			(597)	(544)	(2,819)
Treated = Did not participate in the program	3,347	4,860	-1,513**	2,264	2,762
			(542)	(2,341)	(2,819)
Differences in differences in differences			**845**		
			(592)		

Table 1.10 (Continued)

4-scale employment quality					
Treated = Participated in the program	2.503	3.013	-0.510**	563	2,962
			(0.108)	(660)	(3,096)
Treated = Did not participate in the program	2.401	2.901	-0.501**	2,579	3,018
			(0.041)	(2,608)	(3,096)
Differences in differences			**-0.014**		
			(0.069)		
Labor participation rate in the non-agricultural sector					
Treated = Participated in the program	0.679	0.636	0.044	914	4,606
			(0.039)	(976)	(4,750)
Treated = Did not participate in the program	0.636	0.642	-0.006	4,071	4,677
			(0.015)	(4,097)	(4,750)
Differences in differences			**0.050****		
			(0.019)		

Notes: A plus sign (+) denotes a significant coefficient at the 10% level, one star (*) denotes significance at the 5% level, two stars (**) at the 1% level. Numbers in column 3 may not equal the difference between corresponding numbers in columns 1 and 2 exactly due to rounding errors. The same holds for the DDD results against the corresponding DID results. ATTs for both participants and non-participants are estimated using 5-nearest neighbors matching.

[a] The number in parentheses denotes the total number of observations (both on and off support).

denoting the current and the base year respectively.[67] The measured treatment effect thus should be interpreted as on the *growth* of the concerned variable. For employment quality and rate of non-agricultural employment, I simply use Y_t as the outcome variable. For the former, it is because differencing a scale variable would make its interpretation more ambiguous. As to the rate of non-agricultural employment, this is largely due to the lack of 2007 data.

The results are reported in rows headed "Treated = Participated in the program" in Table 1.10. The increase in household non-agricultural income is 1,854 *yuan* (around

[67] Given the broad collection of 2007 characteristics in the set of matching variables, notably various types of income, the treated and untreated samples should have similar base year conditions as long as they are well matched, therefore replacing the outcome variables with Y_t does not have much effect on the results. Only DID results are reported here for conciseness.

USD 280) higher for treated than untreated households, though insignificant. The relative drop in agricultural income is strongly significant at 6,518 *yuan*, due to a slight increase for an average untreated household, but more importantly to a plunge for treated households. Total household income drops relatively by 3,930 *yuan*, which is marginally significant. Replacing it with households' self-reported 2009 total income gives a comparable but insignificant estimate.[68] At the individual level, increment to non-agricultural income for members of treated households is 657 *yuan* short of those from the control group, but insignificant. Treatment also significantly improves employment quality by half a scale, and brings the rate of non-agricultural employment up by 4.4 percentage points.

1.5.3.2 Difference-in-Differences-in-Differences Estimation

By construction, DID estimations cannot control for town fixed effects as in the linear regressions, hence the measured treatment effects may incorporate differences in town-specific trends that would exist even in the absence of the Housing Exchange Program. Table 1.11 reports the average 2007 and 2010 values of the outcome variables as well as their interim changes for treatment towns (including program participants and non-participants alike) and control towns respectively, which demonstrate that the two groups start out from comparable positions and went through similar trends on most dimensions. The seemingly noncongruent trends can also be explained by the program effects: the change in agricultural income is positive for the control group but negative for the treatment group, probably because many participants in the program quit farming

[68] The outcome variable here is the 2009 income *level* instead of the 2007-2009 *difference*, due to lack of data on self-reported total household income in 2007.

Table 1.11 Comparison between Treatment and Control Towns (Sample Means)

	2007		2010		2007-10 Change	
	Treated	*Control*	*Treated*	*Control*	*Treated*	*Control*
Household non-agricultural income	37,835	42,238	48,024	52,482	10,189	10,244
Household agricultural income	10,312	6,207	9,886	6,564	-426	357
Household total income	49,378	50,929	60,184	61,829	10,806	10,900
Individual non-agricultural income	20,599	22,561	24,043	26,322	3,444	3,761
Employment quality	2.457	3.038	2.443	3.019	-0.014	-0.019
Labor participation rate in the non-agricultural sector	-	-	0.641	0.651	-	-

Source: Own survey.
Notes: The "Treated" sample includes all observations from the four pilot towns (participants and non-participants in the program are both included), and the "Control" sample includes all observations from the three non-experimental towns. To ensure comparability, the household-level statistics are calculated using observations that have non-missing data for all three types of income in both years. Likewise, calculation of individual non-agricultural income uses observations with data for this variable in both years, and similarly for employment quality.

outright; the control group had a greater relative increase in non-agricultural income at the individual than at the household level, which can be attributed to the program's effect on urban employment participation at the extensive margin.

Despite the comparable factual trends between the treatment and control groups, we cannot rule out the possibility of divergence in counterfactual trends. Even program-induced cross-town differences may contribute to this bias. For example, given sufficiently segregated labor markets,[69] if the program indeed stimulates non-agricultural employment, the sudden expansion in labor supply may lead to a temporary dip in the equilibrium wage level of pilot towns, whereas more and better jobs will presumably be

[69] 80.84% of the survey respondents have a stationary non-agricultural job work within their hometown, and 94.18% within the home county.

created as land quota saved under the program becomes available for industrial development.[70] This would bias down the DID estimate of treatment effect on non-agricultural income, which may partially explain its relatively slower growth in treatment towns (Table 1.11). Last but not least, given the preannounced plan to bring all towns in Jiaxing under the program by 2010, households in control towns, having learned from the experience of pilot towns, may have pre-adjusted behavior in anticipation of the program, which again results in underestimation of the treatment effects. To remove the confounding trend effects at the township level, I next turn to a difference-in-differences-in-differences (DDD) analysis.[71]

Mathematically, the outcome variables can be disaggregated into an observation-level and a township-level component: $Y = y + \tau$, where y is what we really want to measure. The DID method estimates the ATT as

$$\begin{aligned} \widehat{ATT} &= \left(Y_t^{Tr} - Y_{t-1}^{Tr}\right) - \left(Y_t^{Un} - Y_{t-1}^{Un}\right) \\ &= \left[\left(y_t^{Tr} + \tau_{t-1}^{Tr}\right) - \left(y_t^{Tr} + \tau_{t-1}^{Tr}\right)\right] - \left[\left(y_t^{Un} + \tau_{t-1}^{Un}\right) - \left(y_t^{Un} + \tau_{t-1}^{Un}\right)\right] \\ &= ATT + \left[\left(\tau_t^{Tr} - \tau_{t-1}^{Tr}\right) - \left(\tau_t^{Un} - \tau_{t-1}^{Un}\right)\right] \end{aligned}$$

where the superscript *Tr* stands for treated, and *Un* for untreated. For notational ease, I assume one-to-one matching here, and let $\left(Y_t^{Un} - Y_{t-1}^{Un}\right)$ denote the time-differenced outcome

[70] A time lag is inevitable as both the approval of zoning plans and the ensuing construction works take time. The pilot towns, in anticipation of a relaxation of the land constraint, may have reoptimized land use plans intertemporally by getting more aggressive on current usage. On the other hand, zoning quota has to be put aside for construction of the new houses and apartments before use of the saved quota can be ratified. Interviews of the town officials indicate that the latter force prevailed at the time of the survey.

[71] DDD estimation is subject to the *externality bias*, i.e. spillover effects on non-participants. For example, non-participants may now get a chance to lease participating households' farm, which adds to their agricultural income. Such effects are however unlikely to overturn the results.

of the match for Y^{Tr}. The true ATT is the observation-level difference between the treated and untreated outcomes, while the bracketed trend difference term represents a noise. As long as $\tau_t^{Tr} - \tau_{t-1}^{Tr} \neq \tau_t^{Un} - \tau_{t-1}^{Un}$, the estimated ATT is biased.

To perform the DDD analysis, I first redo the DID estimation, but this time taking non-participants from pilot towns as the treated group, and denote the result by $Diff^q$ (q = quit). The ATT is then estimated as the difference between $Diff^q$ and the DID-estimated ATT, or $Diff^p$ (p = participating)

$$\begin{aligned}
\widehat{ATT} &= Diff^p - Diff^q \\
&= \left\{\left[\left(y_t^{Tr,p} + \tau_t^{Tr}\right) - \left(y_{t-1}^{Tr,p} + \tau_{t-1}^{Tr}\right)\right] - \left[\left(y_t^{Un,p} + \tau_t^{Un}\right) - \left(y_{t-1}^{Un,p} + \tau_{t-1}^{Un}\right)\right]\right\} \\
&\quad - \left\{\left[\left(y_t^{Tr,q} + \tau_t^{Tr}\right) - \left(y_{t-1}^{Tr,q} + \tau_{t-1}^{Tr}\right)\right] - \left[\left(y_t^{Un,q} + \tau_t^{Un}\right) - \left(y_{t-1}^{Un,q} + \tau_{t-1}^{Un}\right)\right]\right\} \\
&= \left[\left(y_t^{Tr,p} - y_{t-1}^{Tr,p}\right) - \left(y_t^{Un,p} - y_{t-1}^{Un,p}\right)\right] - \left[\left(y_t^{Tr,q} - y_{t-1}^{Tr,q}\right) - \left(y_t^{Un,q} - y_{t-1}^{Un,q}\right)\right] \\
&= ATT + \left[\left(y_t^{Un,q} - y_{t-1}^{Un,q}\right) - \left(y_t^{Un,p} - y_{t-1}^{Un,p}\right)\right]
\end{aligned}$$

The township trend effects cancel out, and assuming that the matched observations for participants and those for non-participants experienced the same average outcome change, then the bracketed term also becomes zero and what is left is the pure treatment effect.[72]

In Table 1.10, column 3 reports $Diff^p$ in rows headed "Treated = Participated in the program", $Diff^q$ in rows headed "Treated = Did not participate in the program". The DDD-estimated ATT is reported below in bold as the difference between the two. Based on the DDD estimates, the growth in non-agricultural income of participating households is significantly higher by 3,432 *yuan* (approximately 520 USD), equivalent to 8.48% of the average annual net income of all Jiaxing rural households in 2008 (Statistics Bureau).

[72] By eliminating the township trend effects, The DDD method has an additional merit of neutralizing the *interviewer bias*, which is brought about by systemic bias on the part of the village-specific surveyors.

Relative agricultural income growth of participating households is again significantly lower by 7,200 *yuan*. In fact, the agricultural income of non-participating households has a slightly larger increment than their matches in non-experimental towns, perhaps because some of them managed to lease farmland from participating households.[73] The relative growth in household total income is 3,114 *yuan* lower for participating households, yet greater by 2,381 *yuan* if self-reported figures are used, both significant at the 5% level. This leaves the overall sign undecided.

At the individual level, the program contributes to the average increment in non-agricultural income by 845 *yuan*, which equals 7.33% of the annual net income of rural residents in Jiaxing in 2008. The effect is, however, insignificant. Employment quality is slightly and very insignificantly improved. The apparently huge improvement by DID estimate seems mostly due to different trends at the township level other than the program. Finally, other things equal, the program raises the labor participation rate in non-agricultural employment by 5.0 percentage points (significant at 1%), which points to a huge sectoral shift of the rural labor force at the extensive margin. Analogous analysis on non-agricultural work hours (unreported) gives a strongly and significantly positive program impact on the total work hours from all household members (58.56 hours by DDD estimate), yet a much smaller and insignificant effect on individuals with an urban job before the program (5.25 hours by DDD estimate).[74] This confirms that the program has stronger effects at the extensive than the intensive margin.

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[73] Indeed, non-participating households have on average been allotted 5.12 *mu* of farmland, but cultivate 6.23 *mu*. Some of the 1.11 *mu* difference is likely subcontracted from other households.

[74] Data on work hours are only collected for 2010, therefore the outcome variable is the number of work hours in 2010 rather than the 2007-10 difference.

Table 1.12 Summary Statistics on Samples on and off the Common Support

	Treated (participants)		Control	
	Off support	On support	Off support	On support
Total household income	48,814	53,509	183,611	60,995
	(46,294)	(50,000)	(74,000)	(55,000)
Household non-agricultural income	35,818	46,436	173,302	51,966
	(32,500)	(45,000)	(60,000)	(46,000)

Source: Own survey.
Notes: The unparenthesized numbers are sample means, and numbers in parentheses are sample medians.

Results reported in this section are generated using 5-nearest neighbors matching, while varying the number of neighbors or using other matching methods does not change the results in any fundamental way. However, it has to be noted that whatever matching method is used, observations for which suitable matches cannot be found are left out. Columns 4 and 5 in Table 1.10 suggest that an overwhelming majority of households *are* on the common support, which verifies the applicability of the matching method, yet this still leaves out some extreme observations. Table 1.12 presents summary statistics on total household income and household non-agricultural income for participating households (the treated) and households from the control towns respectively. The two groups have close mean and median values for observations on the common support, which confirms that the matching method successfully sorts out comparable households. Turning to off-support households, those in the treated group have lower total income and non-agricultural income than households on the common support, and the reverse is true for the control group. This suggests that the economically worst off households have a very high probability for participation, for whom matches cannot be found in the control

group. The treatment effects would have been underestimated if this subset of households experienced greater changes in livelihood.

1.5.4 *Section Summary*

Findings in this section demonstrate that the Housing Exchange Program provokes notable occupational and earnings responses, just as the model predicts. Most saliently, participating households experience a strong increase in non-agricultural income and a drop in agricultural income, suggesting an internal shift of labor from the rural to the urban sector. A closer look at the individual level reveals that these observations have to be more attributed to outright occupational changes rather than adjustments in work time allocation.

Given the usually deliberative planning and searching process before job changes and time needed to adapt oneself afterwards, some, if not most, of this recently instituted program's effects will unfold only in years to come. This is reinforced by the conceivable estrangement of moving participants from established social networks - a crucial source for job seekers in an "acquaintance society" (Fei, 1948), while a new one takes years to build.[75] In addition, some participating households had members actively involved in the construction and fitting-out works, which considerably cut into their time spent on pecuniary jobs, causing a temporary downward bias in our income figures. All these factors, coupled with the time lag before saved zoning quota can be translated into

[75] DDD estimator suggests that the proportion of non-agricultural workers who got their current jobs through an acquaintance's referral is 2.8 percentage points lower for participants than non-participants, though insignificant. Redoing the analysis on a sample of individuals who obtained their current jobs in or after 2007 widens the gap to 11.8 percentage points, significant at the 2% level. These results are consistent with the hypothesis that relocation brings about a (transient) disturbance to agents' social network.

industrial development and job creation, imply that the long-term occupational and income impacts will likely exceed the current estimates.

Not only do program effects take time to evolve, they are also not synchronized. Some are quite instant, e.g. a slump in farming income upon quitting; others take much longer to realize. For instance, households may experience a dip in income from homestead-based lodging services, yet have it recouped or even swelled after settling down in the new home. Moreover, with the aging farmers retiring and their educated offspring unable *and* unwilling to take over, the currently higher agricultural income of non-participating households is unsustainable on average anyways.[76]

So far the analysis has focused on the direct impact of the program on participants. To broaden the vision, I will next introduce two possible extensions of the above-discussed program implications. First, the fall in participating households' agricultural income does not signify a declining agricultural industry. Rather the opposite. The prevailing situation with a myriad of households farming finely gridded plots not only obstructs exploitation of economies of scale,[77] but more importantly, enriched by earnings from the urban sector, many rural households grow little more than just enough

[76] Taking Jiaxing as an example, the average age of the 504.9 thousand full-time farmers was 57.3, with 60.9% over 55 years old and 86.2% over 45 (2007 Agricultural Census).

[77] Empirical tests of scale economies in farming give mixed results. However, such tests are mostly done in countries where the average farm size well exceeds that in Southeast China, and the literature generally agrees that size does matter when production scale is small (see e.g. Kislev and Peterson, 1996). The average area of farmland holdings is 186.95 *ha* in the U.S., 70.21 *ha* in the U.K., 31.46 *ha* in France, 1.55 *ha* in India, 1.20 *ha* in Japan, 1.05 *ha* in South Korea, 2.16 *ha* in the Philippines, and 3.36 *ha* in Thailand (FAO, 1990), while that in Jiaxing is well below the prevailing size distribution at 0.21 *ha* (2007 Agricultural Census), making increasing returns to scale highly plausible.

for own consumption, which is much less than the potential yield.[78] With most of the participating households leasing their farm out to skilled farmers and agricultural companies, often with the help of Exchange Farmland for Welfare, such a jump from self-sustenance to scale production will likely give agricultural output a strong push. Empirical tests will have to wait a few years, as the agricultural industry features high initial outlay and a long investment cycle. Incidentally, it may also represent a solution to the increasingly acute food safety problem, as reputation building of agricultural products involves a high fixed cost that only makes sense for scale operation.[79]

Second, social welfare depends not only on the income *level* but also its *distribution*. As is shown in Section 1.4, participating households cluster around the lower section of the distribution along various economic dimensions *ex ante*. Thus, as long as they expect a net utility increase, the program should be progressive. Gini coefficient calculated using sample households' total income in 2007 is 0.345 for the four pilot towns, and 0.380 for the three control towns. The figure drops to 0.334 for the former and increases slightly to 0.382 for the latter in 2010. While this simple comparison confirms progressivity of the program, more carefully designed analysis is required to draw a sound conclusion. In particular, the income effect of compensation payments has to be purged before earnings

[78] Jiaxing, with its soil and climate conditions especially suitable for rice production, has the tradition of a double, and sometimes triple, cropping system. However, out of the 3,117 sampled rice-growing households, merely 70, or 2.24%, reported growing two or more crops annually in 2007. Actually, geographical overlap of the traditional farm belt and the fastest industrializing region has long been a conundrum for the central government, causing different ministries to sometimes issue conflicting rules (September 2010, personal intercourse with officials of the Ministry of Agriculture).

[79] Besides economies of *scale*, there also exist prospects for economies of *scope*. With de-urbanization concomitant with urbanization in some bigger cities, large enough plantations can be developed into upscale residential areas, rehabilitation centers, pastoral resorts, etc. to cater to the high-end market. Jiaxing, being located in the economically most advanced region in China - the Yangtze Delta and neighboring several big cities, has a potential for such business.

can be truly taken as welfare measures.

1.6 Conclusion and Future Extensions

The Housing Exchange Program in Jiaxing, by extending limited property trading rights to rural households, facilitates their acquisition of a decent accommodation in nearby secondary cities/towns. Given the compensation level, this program has a greater appeal to households that are economically worse off, live in shabbier conditions, and are farther removed from urban areas. The program's relative popularity with disadvantaged households evidences its progressivity and conduciveness to promoting equality, notably embodied in its greater effectiveness in easing access to the urban labor market and amenities for such households. Comparison of pilot towns also points to compensation from the program as a crucial determinant of participation choices, which calls for a compensation hike in most towns if the take-up rate is to be lifted from its currently modest level while still keeping participation on a voluntary basis. This could either be in cash, or in more implicit forms such as a greater discount in the purchase price of new residence - indeed, the continuing housing market boom in Jiaxing has already added to the attraction of the existing compensation packages and invoked some new interest.

This paper also looks at the program's impact on participants' employment and income. At the intensive margin, it appears to have a positive effect on the earnings of people engaged in off-farm work, without compromising their employment quality. More notably, it works at the extensive margin by inducing a sizable shift of labor from the agricultural and domestic sectors to the non-agricultural sector. At the household level, these individual occupational changes add up to a sizable non-agricultural income growth and a plunge in agricultural income.

These findings stand in stark contrast to the predominant form of urbanization - or rather semi-urbanization - around large cities, where a household typically straddles rural and urban sectors both in terms of inhabitation and income source. As the program was launched only in 2008, only the short-term effects are observable, which makes the size and scope of its longer-term impacts an interesting question for future research. Besides that, I will also discuss below four other potentially promising research directions.

First, while this paper mainly assesses the program's effect on employment and income, it cannot be emphasized too strongly that utility also depends on many other factors. We have already shown that a reduction in income could well be the planned result of reoptimization between consumption of goods and leisure, and a drop in farm product sales may be in exchange for a lower yet much steadier stream of rental income. To evaluate the program's welfare impact, one has to look at consumption - the direct determinant of utility. Using the propensity score estimator, I find that an average participating household spends 737 *yuan* more on consumption annually, and the treatment effect jumps to a significant 3,155 *yuan* increase once expenditure on durable goods is excluded.[80] The results are reported in Table 1.13. While this may represent an enhancement in utility caused by compensation-induced wealth effect, it could partially be the result of higher living costs associated with urban life. For example, around a third of the relative increase in consumption, or 1,031 *yuan*, goes to food (Table 1.13), which is likely the combined result of a market evaluation of what used to be home-produced and an improvement in living standards. As the participating households are undergoing a

[80] This adjustment is to correct for the behavioral bias that participants may deviate from their regular durable goods purchasing schedule, with those moving in overspending on furniture and appliances, and those whose new residence is not ready holding back.

Table 1.13 Program Effects on Consumption: Propensity Score Matching

	(1) Treated	(2) Controls	(3) Difference	(4) # treated on support[a]	(5) # control on support[a]
Household consumption					
Treated = Participated in the program	39,390	36,437	2,953	286	1,440
			(4,372)	(329)	(1,493)
Treated = Did not participate in the program	40,663	38,446	2,216	1,222	1,463
			(1,978)	(1,252)	(1,493)
Differences in differences			**737**		
			(2,433)		
Household normal consumption					
Treated = Participated in the program	35,884	31,987	3,898	286	1,440
			(3,754)	(329)	(1,493)
Treated = Did not participate in the program	35,825	35,083	743	1,222	1,463
			(1,074)	(1,252)	(1,493)
Differences in differences			**3,155**$^{+}$		
			(1,763)		
Household expenditure on food					
Treated = Participated in the program	7,332	7,256	76	286	1,440
			(914)	(329)	(1,493)
Treated = Did not participate in the program	7,089	8,043	-955**	1,223	1,463
			(234)	(1,253)	(1,493)
Differences in differences			**1,031****		
			(365)		

Notes: A plus sign ($^{+}$) denotes a significant coefficient at the 10% level, one star (*) denotes significance at the 5% level, two stars (**) at the 1% level. Numbers in column 3 may not equal the difference between corresponding numbers in columns 1 and 2 exactly due to rounding errors. ATTs for both participants and non-participants are estimated using 5-nearest neighbors matching. The outcome variable corresponds to the current situations rather than the difference between the current and the base year, due to the lack of 2007 data. "Household consumption" is the annual aggregate household expenditure on all consumption items. "Household normal consumption" excludes expenditure on furniture and appliances and other major durable goods.

[a] The number in parentheses denotes the total number of observations (both on and off support).

complete transformation of consumption pattern, more conclusive results will have to wait until a new steady state is reached, which poses an interesting question for future studies.[81]

Another direction is to study the program's feasibility for and potential impact on rural migrant workers, or "new residents". Unlike local rural residents, their homestead is so far from the industrial region that they have to migrate annually, leaving less skilled members of the family behind in their more indigent hometown (UNRISD, 1994). The Statistics Bureau of Jiaxing has been conducting an annual survey on new residents since 2006. Comparison of their 2009 round with my survey reveals that the migrant workers are on a par with or even surpass local rural residents in terms of human capital, yet receive considerably lower earnings (see Table 1.14). Part of this can be attributed to migrant workers' lack of social capital, which has been discussed a lot in the literature (see, e.g., Massey, 1990b; Calvó-Armengol and Jackson, 2007). Another factor, which has recently come to the attention of public media but not academia yet, is the diminishing work effort of migrant workers, notably the younger ones. While many worked hard for the prospect of settling down in the city someday, soaring housing prices in recent years reduces it to a remote dream, causing some to lose the drive and work in an irregular and intermittent pattern.[82] This can be individually rational, yet socially inefficient and inequitable.

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[81] For instance, they may overspend on everyday goods as economization is usually harder in transitional period, or they may cut down on expenditure because the upfront housing costs tighten up their credit constraint in the short term.

[82] The observation was first brought up in my personal contact with Sen Gong, Deputy Director of the Department of Social Development, Development Research Center of the State Council. Besides an instant fall in income, in the long term, such an unstable work pattern is detrimental to skill accumulation, which in turn reinforces work instability - a vicious circle.

Table 1.14 Comparative Statistics on Local Rural Residents and New Residents

	Local rural residents		New residents
	Entire sample[a]	Age < 50[b]	
Gender (% male)	53.8	49.9	57.9
Age distribution (%)			
16-19	1.3	1.7	9.2
20-29	17.9	23.2	39.3
30-39	21.6	28.1	32.7
40-49	36.1	47.0	18.3
≥ 50	23.1	-	2.9
Education level (%)			
Illiterate or almost illiterate	5.2	1.0	2.2
Elementary school	30.1	21.3	17.3
Junior high school	47.7	56.6	63.3
Senior high school (incl. vocational high school)	9.8	11.8	9.9
Technical secondary school	1.7	3.1	3.4
Junior college or above	4.8	6.1	3.9
Average years of schooling	7.65	8.57	8.93
% with some vocational certificate[c]	9.3	10.3	8.9
Average income from non-ag. emp.	28,367	31,475	21,696
Source of residence in Jiaxing (%)[d]			
Self-owned			1.2
Rented			82.6
Provided by workplace			15.6
Others			0.5
Dwelling floor space per person in Jiaxing (m^2)[e]	73.7		10.6

Source: Own survey (numbers are from 2010). Survey on the New Residents of Jiaxing (2009).
Notes: The local sample is restricted to rural residents with positive non-agricultural income in 2010 for categories from *Gender* to *Average income with non-ag. emp.* This is for comparability with the sample of new residents, who are disproportionately working in the non-agricultural sector. The percentage numbers may not sum up to 100% due to rounding errors.

[a] Individuals under age 16 are excluded for compatibility with the sample of new residents.
[b] Individuals over age 49 are excluded to make the age distribution more comparable to the sample of new residents. The remaining sample consists mostly of people still in the labor force.
[c] Including elementary vocational qualification certificate, intermediate vocational qualification certificate, advanced vocational qualification certificate, and Technician or senior technician.
[d] Only four of my sampled households (0.12%) do not own any real property, and all their members are above age 60. It indicates that the overwhelming majority of local rural residents, especially the working population, live in self-owned residence.
[e] The figure for local rural residents is from 2007, because currently many program participants are living in temporary residence, making it difficult to obtain accurate numbers.

Extension of the program to migrant workers, by facilitating access to permanent local residence, potentially addresses both factors. Besides that, the drastic reduction in the effective value of *d* will likely induce an augmented employment impact, most conceivably involving a sizable expansion of labor force participation in the industrial sector. Considering the large population size of this demographic group, appropriate policy measures that mitigate their plight will significantly alleviate the two predominant types of social inequality in China, regional and urban-rural inequality.[83]

A third extension is to compare this program with the *chaiqian* projects. From the viewpoint of affected households, while both types of program offer compensation for their real property, they differ fundamentally in the role of the government. In *chaiqian* projects, the government draws on authoritative power to enforce compliance, leaving households with little bargaining power. Therefore, it represents deprivation of rights rather than empowerment. The Housing Exchange Program, on the other hand, essentially establishes a market relationship by placing the participation choice at the discretion of households.[84] Comparative study of the two types of program may enable us to distinguish the impact of freedom to trade property rights, or "capabilities" in Sen's terminology (Sen, 1999).

Finally, whether judging from the government's primary goal to promote urban development, or from the locational and occupational impact on participating households,

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[83] According to the National Bureau of Statistics, a total of 229.78 million rural residents were doing non-agricultural work in 2009, out of which 145.33 million were working non-locally. To get the total population size of the immediate families of this demographic group, these numbers have to be at least timed by two.

[84] Incidentally, the contrast in the use of coercive power is probably better represented by points widely apart on a spectrum rather than a black/white dichotomy.

the Housing Exchange Program represents an important stimulus to the ongoing urbanization process. While the word *urbanization* is apparently well-defined in macro terms, its micro-definition is more ambiguous and may be more appropriately proxied by a stratified measure that integrates multiple dimensions of life rather than a stark dichotomy. The subject matter of this paper, along with the three extensions, are each associated with some aspect of urbanization at the micro level, including sectoral labor shift, changes in consumption pattern, and geographic relocation. To measure urbanization more accurately, it is perhaps time to design a comprehensive urbanization index that captures the various constituents.[85]

[85] According to the 2010 Issue of the Blue Book of Cities in China (CASS, 2010), the urbanization rate is somewhat overestimated due to inclusion of incompletely urbanized population. The urban population statistic actually contains a large agricultural population, including migrant workers, which leads to an overestimation of the urbanization rate by at least 10 percentage points. The authors of the Blue Book also noted a serious mismatch between the "quantity and quality" of urbanization in China, which "lacks capacity, synergy, and congruity." Such discord is mainly reflected in: excessive expansion of large cities, inadequate development of smaller cities and towns, substandard urban life, and a multitude of residual problems from incomplete urbanization such as "urban villages", "garbage besieged city", etc.

1.7 Appendix A: Rural Land Policies in China and Their Socioeconomic Implications

All land in China is publicly owned, with urban land owned by the state and rural land owned by village collectives. While usage of the former is largely at the discretion of local governments, the latter is strictly reserved for rural purposes, with most residential, commercial, and industrial developments expressly prohibited. Under the current land management system, rural land can be legitimately converted for urban development only after its ownership is transferred from the village collective to the state.

Rural land is further zoned into farmland and land for construction. Rural households are eligible for a piece of each within their village of domicile - farmland for agricultural production and construction land, also called homestead (*zhaijidi*), for home building. However, households only hold *usufructuary* rights to the land, which entitles them to possess, use and trade proceeds from it (such as crops), yet transaction of the land itself is forbidden. In the case of homestead, the inseparability of the house and the land in effect makes the entire real property untradeable.[86] Panel A in Table 1.15 lists the relevant laws and directives that form the basis of this inflexible system.

Initially, the system was instituted for the ease of management in a static agricultural society, yet with the dynamics of industrialization reaching an ever-larger rural population, it is increasingly becoming a restraint, as the conventional practice of moving

[86] More accurately, transfer of land rights, whether or not in exchange for a payment, is only allowed between members of the same village.

Table 1.15 Laws and Directives Underpinning the Rural Land Management System

Panel A: Laws and Directives Constraining Rural Land Use and Transactions

	Year in Effect	*Relevant Terms*	*Implications*
Constitution	1982[a]	Article 10:Land in the cities is owned by the state. Land in the rural and suburban areas is owned by collectives except for those portions which belong to the state in accordance with the law; house sites and privately farmed plots of cropland and hilly land are also owned by collectives.	Stipulates that all rural land is owned by collectives - an entity large enough to suppress individual rights, yet not quite as large (as the state) to render the ownership status nominal.
Law on Administration of Urban Real Estate	1995 (revised edition)	Article 2: Obtaining the land-use right for development of real estate, engaging in development of real estate and transaction of real estate, and exercising administration of real estate in the State-owned land within a planned urban district in the People's Republic of China shall comply with this Law.	Restricts applicability of the law to real estate in *state-owned land*, with no respective laws addressing the administration of rural properties, leaving transactions thereon legally unrecognized and unprotected.
		Article 8: The land-use right for the collective-owned land within a planned urban district may be granted with payment only after it is requisitioned in accordance with the law and turned into state-owned land.	Forbids usage of collective-owned land, i.e. rural land, for urban development.
Reply Regarding the Application of "Transfer of Land by Other Unlawful Means"	1990	It is one of the "other unlawful means of land transfer" for the original user of homestead (*zhaijidi*) to let someone else financially contribute to renovation of the house in exchange for the right to use, and to profit from or obtain property rights of it without legal approval.	First time for the State Bureau of Land Administration to expressly ban a particular form of trade in *usus* rights to homestead, setting a precedent for other transactional attempts.[b]

Table 1.15 (Continued)

Land Administration Law	1999 (revised edition)	Article 43: Except for the establishment of township and village enterprises, villagers' residences, and village (township) public facilities and non-profit undertakings, which can use collective-owned land, any unit or individual that needs to use land for construction must apply for the use of state-owned land in accordance with law. Article 63: The land use right of farmer collectives shall not be leased, transferred or rented for non-agricultural construction.	Forbids any form of usage of collective-owned land, i.e. rural land, for any constructional purposes, with a few very limited exceptions.
Circular of the General Office of the State Council on Strengthening Management of Transfer of Land and Strictly Banning Speculative Land Dealing	1999	Rural residence may not be sold to urban residents, nor may urban residents be permitted to build residence on rural collective-owned land. Departments concerned shall not issue certificate for land use and property ownership certificate for unlawfully constructed or purchased residence.	An outright ban on sales of rural real properties to urban residents.
Regulations on the Protection of Basic Farmland	1999	Article 9: The basic farmland delimited by the provinces, autonomous regions and municipalities directly under the Central Government should account for more than 80 per cent of the total area of cultivated land within their respective administrative areas. Specific quantum targets shall be broken down and transmitted level by level in accordance with the overall national planning for land utilization. Article 14: Local people's governments at all levels should take measures to ensure that there is no reduction in the quantum of basic farmland within their respective administrative areas as determined by the overall planning for land utilization.	Sets a high minimum farmland size for each level of the government to maintain, which severely constrains the change of use of farmland.

Table 1.15 (Continued)

Panel B: Directives that Prepare the Ground for Changes to the Administration of Rural Construction Land

	Year in Effect	*Relevant Terms*
Decision of the State Council on the Furtherance of Reform and Tightening Land Administration	2004	*Usus* right of collectively-owned construction land in villages and towns can be transferred in accordance with law, based on the premise that such undertaking is inline with planning.
Notice on Managing Land Use Economically in Accordance with Laws and Regulations to Support the Construction of a New Socialist Countryside (Ministry of Land Resources)	2006	To meet the new requirements of rural development, the Ministry has approved the steady promotion of pilot projects on linking up the increase in urban construction land quota and reduction in rural construction land quota, and on the transfer of *usus* right to collective construction land. The projects shall be evaluated and revised in a continuous and timely manner.
Decision of the Central Committee of the Communist Party on Some Important Issues Concerning the Promotion of Rural Reform and Development[c]	2008	[We should] gradually establish a market that treats urban and rural construction land equally, i.e. for legally acquired rural collective land for construction, its *usus* right must be transferred publicly and formally through a unified and tangible land market. It should enjoy equal rights with state-owned land as long as it complies with land use planning. Close attention should be paid to update the relevant laws, regulations and supporting policies, and the reform on rural land management system should progress in an orderly fashion.

Notes: This table only lists the most relevant laws and directives at the central level, which form the basis of the more specific and practical regulations and orders at lower levels of the government. The items are listed in chronological order.

[a] Revised in 1988, 1993, 1999, and 2004. Articles quoted here were never revised.
[b] The qualification of "without legal approval" is in name only, as no procedures for approval are in place.
[c] Passed at the Seventeenth Session of the Third Plenary Meeting of the Chinese Communist Party Central Committee.

closer to work by selling the old home fails here.[87] For those who can find an urban job within commuting distance, this is mainly a matter of transport costs; however the majority have to rent somewhere in the city, leaving their homestead, oftentimes a double- or triple- storey house with a decent-sized courtyard, unoccupied for most time of the year, sometimes even resulting in "vacant villages". The latter group in Jiaxing alone encompasses 1.8 million migrant workers, plus locals from outlying village.

Not only are rights to rural land untradeable, but they are also contingent on retaining one's rural *hukou* status. Thus, although urban livelihood without an urban *hukou* entails considerable discriminatory treatment and social marginalization,[88] the fear for being disentitled of such rights still keeps many from a *hukou* conversion. This is especially pronounced in the economically advanced regions, where the rapid expansion of cities substantiates the prospect of a rural household being bought out someday, while escalating urban land prices bespeak a hefty compensation.[89] Consequently, instead of greasing the wheels of rural-urban transition, rural land rights have rather added to its implicit cost and thereby become a white elephant.

At the same time, the accelerating economic development across China has turned conversion of rural land for urban use from a possibility to a necessity. Previously this

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[87] Despite the ban on sale of rural land to people who are not members of the collective, private deals on rural homestead still exist, yet on a relatively small scale. Such deals are also unrecognized by law, which usually leaves the buyer's rights little protected in case of dispute, see e.g. *Xianhua Han* vs. *Jingren Zhang and Jinglin Bu*.

[88] Such discrimination pertains to one's well-being in various ways (see footnote 2). Rural *hukou* holders are also liable to discriminatory treatments in their job search, which often took an institutionalized form in the past but are more implicit nowadays.

[89] Indeed, a number of people who managed to obtain an urban *hukou* years ago (usually at a high cost) are now requesting to resume their rural *hukou*, in anticipation of such compensation. This is most commonly observed in faster developing regions.

has been accomplished through government-led requisition projects, or the so-called *chaiqian* programs, proceeds from which have also become a major source of local fiscal revenue.[90] However, reinforced checks on the use of coercive power, awakening public awareness of civic rights, and most importantly, stronger determination of the higher government bodies to preserve farmland, are increasingly obstructing land conversion in this conventional way (NSD, 2010). Following the revision of the Land Administration Law in 1998, the land use planning procedure switched from a bottom-up to a top-down approach, with the central government first imposing annual and overall construction land quota at the national level, and then breaking it up and transmitting it level by level down the hierarchy. The allocated quota, however, usually falls short of that needed to keep up with the rapid pace of urban development, especially in the more industrialized regions (Au and Henderson, 2006a, 2006b).

Of the two types of rural land, restrictions on farmland are especially stringent. Indeed, the central government claims "the world's most stringent farmland protection measures" by targeting at preserving no less than 120 million hectares of farmland by the end of 2010 (The Eleventh Five-Year Plan, 2006-2010). At lower administrative levels, at least 80 per cent of all cultivated land has to be delimited as *basic farmland*, whose uses can only be changed upon approval of the State Council for such major construction projects as state energy, communications, water conservancy and military installations.[91]

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[90] The Real Right Law (2007) permits the construction land of township and village enterprises, which is by definition collectively owned, to be used as mortgage collateral, thereby implicitly validates its change of ownership status in the case of a foreclosure. While this represents a *de jure* alternative for rural land ownership transfer, its actual practice is an extreme rarity.

[91] With a high percentage (81.6%) of farmland that is four times the provincial average (2007 Agricultural Census), the basic farmland protection requirement is particularly binding for Jiaxing.

By comparison, the change of use of rural construction land is subject to relatively looser regulations, and may be ratified by authorities as low as the county/district level. Besides, the central government has also somewhat lifted legal constraints on the conveyance of title to rural construction land in recent years (see Panel B in Table 1.15 for a list of directives). Although such changes are largely embodied in vague provisions rather than practical terms, it is enough for local officials, who have long been plagued by zoning quota deficiency, to conduct policy experiments.

1.8 Appendix B: Survey Method and Data Collection

1.8.1 *Sample Selection*

Data used in the principal analysis are collected by a questionnaire survey on a sample of 3,408 households from seven towns in Jiaxing, which comprise 13,796 individuals. Four of them are pilot towns for the Housing Exchange Program: *Yuxin*, *Yaozhuang*, *Xincang*, and *Longxiang*. This group comprises 1,789 households, or 7,279 people. The other three towns form the control group, namely *Honghe*, *Xindai*, and *Fengming*,[92] which together contain 1,619 households, or 6,527 people.

The fact that pilot towns were not selected randomly potentially threats the external validity of our results. However, as the program was implemented across the entire jurisdiction of Greater Jiaxing in early 2010, only two years after the experimental phase, and pilot towns were selected on their representativeness of all towns in the first place, pilot towns and other towns cannot differ materially in applicability for this program, which ensures that our results are generalizable at least within Jiaxing.

Sample selection within each town is perfectly random. For each of the seven sample towns, I first obtained the complete registry of households whose members have a local rural *hukou*, and then fed it into the SPSS random sample generator for a sample

[92] I confined the control group to within the jurisdiction of Jiaxing to avoid incompatibility at the municipal level, because towns in different cities, even if similar in natural endowments and geographical location, are subject to distinct administrative orders. Since this study is essentially an assessment of some impacts of a government-led program, it is advisable to ensure homogeneity of the policy environment between the treated and control groups. Besides, compared with other cities, towns within Jiaxing are quite balanced on most indicators relevant to our analysis, such as economic development, population density, industry distribution, farmland fertility, etc., which obviates the need to look for control towns outside the city.

comprising roughly 5% of the population.[93] To make sure that the sample is indeed random and representative, I compared the sample distribution against population distribution graphically on several basic demographic characteristics. The curves track each other very well at both the township and the village level.

1.8.2 *Design of Questionnaire*

The survey questions cover both personal and household information, and correspond to the *current* status, that is at the time of the survey (2010) or the year before (2009), whichever is appropriate. Some of the questions also address conditions in 2007 - the year right before the program was launched - as the base year, thereby assembles a two-period panel data set. As data collected in this way are retrospective in nature, in order to ensure data reliability, only questions about things of which memory does not lapse too quickly were asked.

1.8.3 *Survey Administration and Data Entry*

Village cadres were recruited and trained as surveyors. This choice was mainly because first, the cadres, being acquainted with their fellow villagers, helped dispel doubts about the survey and prevent intentional misreporting, especially as some questions concern rather sensitive topics such as income and consumption. Second, the rural residents in Jiaxing mostly speak dialects, with the accent distinctively different

[93] Alternatively we could have used stratified sampling, which groups township population by villages (the stratum) and applies random sampling independently within each stratum, with the sample size proportional to the village population. It is however unnecessary, as stratification is most useful when sub-populations vary considerably, while villages in the same town are fairly comparable in our case.

even between towns. The village cadre surveyors adapted the survey questions into the local tongue, which facilitates communication.

The survey was carried out from door to door on family members over 18 years. In the case that more than one qualified member is available, the major family economic decision maker was taken as the principal respondent, while the other members were also asked to provide supplementary information. If no qualified member was at home, or a sampled household refused to participate (this happened very rarely, which is another merit of having village cadres as surveyors), the surveyors were instructed to substitute it with its immediate left or right neighbor on a coin toss, and so forth. The sample replacement rate was kept under 5% for each town.

A separate team was set up to inspect on the completeness and consistency of the filled in questionnaires. In particular, follow up calls were made on 10% of the sample households to verify data authenticity. Double data entry (a.k.a. two pass verification) was used to ensure accurate data input from source material to the digital database.

1.9 Appendix C: Robustness Checks Using the Propensity Score Matching Estimator

Instead of basing participation choices on *absolute* values of the factors, households may attend more to their *relative* standing in the community. For the same household with a 50,000 *yuan* annual income, it may choose to stay out if its hometown has an average household income of 10,000 *yuan*, but participate if the number is instead 100,000 *yuan*.[94] Table 1.16 juxtaposes summary statistics on different income types for all households in town and for participating households only, and we see that the two roughly go in tandem by both mean and median measures. Such ranking effects are absorbed in the town fixed effects in linear regressions, but not in the propensity score matching estimators.

To account for this, I normalize the matching variables, wherever appropriate, by subtracting the sample township averages (see notes of Table 1.17 for a full list). For consistency, the outcome variables are also demeaned, again with the exception of employment quality and non-agricultural employment rate. Results are reported in Table 1.17 for DDD analysis on this set of modified variables, which are similar but greater in magnitude and significance at the household level than those in Table 1.10. For example,

[94] Rural households in China are known for deriving utility (or disutility) from their comparative standing in the community. For instance, a key motivation behind "the rural house-building craze," even in the face of outright bans on the use of farmland for non-agricultural purposes, is said to lie in the "social and demographic aspirations of families," where ownership of a large, new house symbolizes wealth and "face". The underlying logic is not only psychological but also economically rational, as such apparently ostentatious flaunting of wealth also serves to attract marriage and business partners (Sargeson, 1999, 2002; McKinley, 1996).

Table 1.16 Relative Economic Standing of Participating Households in 2007 (*yuan*)

	Yuxin	*Yaozhuang*	*Xincang*	*Longxiang*
Non-agricultural income				
All households	30,924	29,238	45,300	42,817
	(25,000)	(24,000)	(40,000)	(41,000)
Participating households	26,480	20,667	49,488	43,763
	(22,000)	(18,250)	(45,000)	(44,000)
Agricultural income				
All households	12,477	14,529	4,916	11,326
	(6,000)	(2,650)	(2,000)	(7,000)
Participating households	14,115	13,469	3,482	8,143
	(8,000)	(2,650)	(2,000)	(5,500)
Total income				
All households	44,363	45,397	51,853	54,995
	(35,900)	(38,000)	(46,300)	(51,000)
Participating households	41,056	35,763	55,144	53,077
	(33,750)	(30,075)	(48,500)	(51,000)

Source: Own survey.
Notes: The unparenthesized numbers are sample means, and numbers in parentheses are sample medians.

the estimated non-agricultural income is 9,736 *yuan*, significant at the 1% level, compared with 3,432 *yuan* (significant at the 5% level) using unnormalized variables. Also, the estimated program effect on total household income is now positive using both aggregated and self-reported figures, though insignificant.[95]

Individual-level estimates are somewhat different from Table 1.10. The DID estimate on non-agricultural income is positive, yet the DDD estimate is slightly and insignificantly negative at -133 *yuan*. Employment quality still improves significantly by DID estimate, but degrades significantly by DDD estimate. Finally, labor participation

[95] Normalization at the *township* level makes the results more comparable to those in Table 1.8. Alternatively, we may normalize at the *village* level if we believe this is the most relevant social group for households' self-assessment of relative standings, which generates qualitatively similar results to those in Table 1.17, and greater in scale and significance. Normalization by the *median* instead of the *mean* also produces similar results.

Table 1.17 Program Effects on Income and Occupation: Propensity Score Matching Using Demeaned Variables[a]

	(1) Treated	(2) Controls	(3) Difference	(4) # treated on support[b]	(5) # control on support[b]
Household non-agricultural income					
Treated = Participated in the program	908	-14,178	15,086**	274	1,450
			(4,188)	(329)	(1,484)
Treated = Did not participate in the program	-884	-6,234	5,350**	1,222	1,466
			(1,735)	(1,247)	(1,484)
Differences in differences in differences			**9,736****		
			(1,665)		
Household agricultural income					
Treated = Participated in the program	-3,863	139	-4,002**	269	1,456
			(1,163)	(327)	(1,483)
Treated = Did not participate in the program	995	-55	1,050+	1,222	1,465
			(539)	(1,249)	(1,483)
Differences in differences in differences			**-5,051****		
			(1,313)		
Household total income					
Treated = Participated in the program	-2,315	-3,835	1,520	265	1,443
			(3,028)	(326)	(1,472)
Treated = Did not participate in the program	-281	-319	38	1,216	1,454
			(1,933)	(1,243)	(1,472)
Differences in differences in differences			**1,482**		
			(1,893)		
Self-reported household total income					
Treated = Participated in the program	-10,899	-37,560	26,661**	286	1,441
			(8,592)	(329)	(1,492)
Treated = Did not participate in the program	-4,731	-24,221	19,490**	1,219	1,462
			(3,132)	(1,250)	(1,492)
Differences in differences			**7,171**		
			(4,497)		
Individual non-agricultural income					
Treated = Participated in the program	858	202	656	426	2,812
			(1,745)	(544)	(2,819)
Treated = Did not participate in the program	498	-177	418	2,278	2,815
			(675)	(2,341)	(2,819)
Differences in differences in differences			**-133**		
			(1,513)		

Table 1.17 (Continued)

4-scale employment quality					
Treated = Participated in the program	2.559	2.961	-0.403^{+}	623	3,048
			(0.210)	(660)	(3,096)
Treated = Did not participate in the program	2.390	2.944	-0.554**	2,548	3,092
			(0.068)	(2,608)	(3,096)
Differences in differences			**0.149***		
			(0.068)		
Labor participation rate in the non-agricultural sector					
Treated = Participated in the program	0.678	0.657	0.021	925	4,706
			(0.049)	(976)	(4,750)
Treated = Did not participate in the program	0.635	0.647	-0.012	4,079	4,745
			(0.016)	(4,097)	(4,750)
Differences in differences			**0.034^{+}**		
			(0.019)		

Notes: A plus sign ($^{+}$) denotes a significant coefficient at the 10% level, one star (*) denotes significance at the 5% level, two stars (**) at the 1% level. Numbers in column 3 may not equal the difference between corresponding numbers in columns 1 and 2 exactly due to rounding errors. The same holds for the DDD results against the corresponding DID results. ATTs for both participants and non-participants are estimated using 5-nearest neighbors matching.

rate in the non-agricultural sector still rises by both the DID and DDD measures, and significant by the DDD estimate. These results confirm that the program has a greater effect at the extensive than at the intensive margin.[96]

To test if the program has varied impacts across demographic groups, I have also run linear regressions and propensity score matching analysis on different gender and age groups. The relative impact of the program on these groups is *a priori* ambiguous. On the one hand, forces preventing women and elder people from non-agricultural employment

[96] To control for town-specific trend effects and ranking effect, I also tried pairing up the most comparable pilot and non-experimental towns (e.g. *Yuxin* and *Honghe*, *Longxiang* and *Fengming*), and applying the DDD analysis to the pairs. The results are in general similar to those of the pooled sample, but less significant, due to larger standard errors with small sample sizes. It has to be noted that propensity score matching generally requires a large sample size within the common support to ensure that good matches can be found, which may not be satisfied by a one-to-one pairing of towns.

before the program may still be in effect. On the other hand, young and mid-aged male agents may have had stretched their non-agricultural working potential prior to the program, whereas the scope is larger for previously disadvantaged groups, especially at the extensive margin. Econometric results indicate that the two forces largely cancel out empirically, with the program having comparable effects across all demographic groups. If anything, it seems to have a slightly greater impact on the younger and the male, but the result is far from conclusive.

Chapter 2

Sectoral Shocks and Aggregate Unemployment: Explaining the Duration of Unemployment*

2.1 Introduction

The unemployment rate rose sharply over the last recession, rising from a low of 4.4 percent in May 2007 to a high of 10.1 percent in October 2009. Among the most noticeable aspects of this increase was the increase in the duration of unemployment. As shown in Figure 2.1 and Figure 2.2, the average duration of unemployment rose sharply in the recession and continued to increase well after the peak in the unemployment rate. Recent readings in excess of 30 weeks are more than 10 weeks above the previous highest duration seen in data going back to the early 1960s. The severity and persistence of the recent slowdown have clearly played a role in pushing up duration, but some other characteristics of the recession are noteworthy as well. In particular, the last expansion was marked by a sustained run up in sectors such as construction and finance, and these sectors were among the hardest hit in the ensuing recession. These developments make one wonder what role sectoral (as opposed to aggregate shocks) may have played in the last recession, and whether they may have something to do with the sharp increase in unemployment duration we have seen this time around.

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* This paper was written jointly with Prakash Kannan (IMF), Prakash Loungani (IMF), and Bharat Trehan (Federal Reserve Bank of San Francisco).

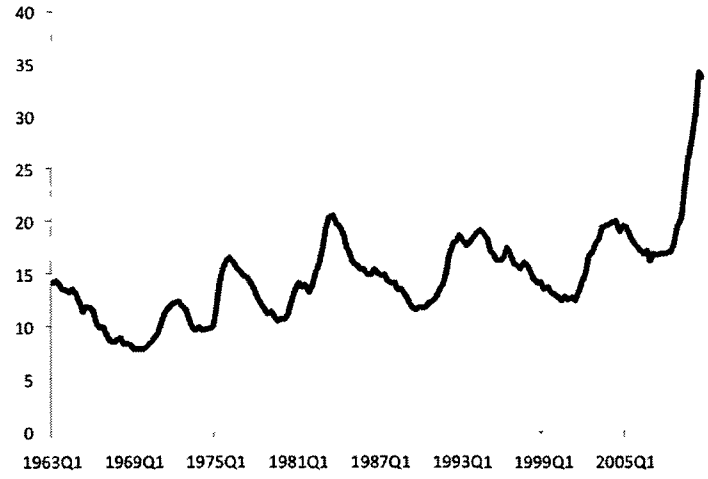

Figure 2.1 Average Duration of Unemployment (Weeks)

Source: U.S. Bureau of Labor Statistics.

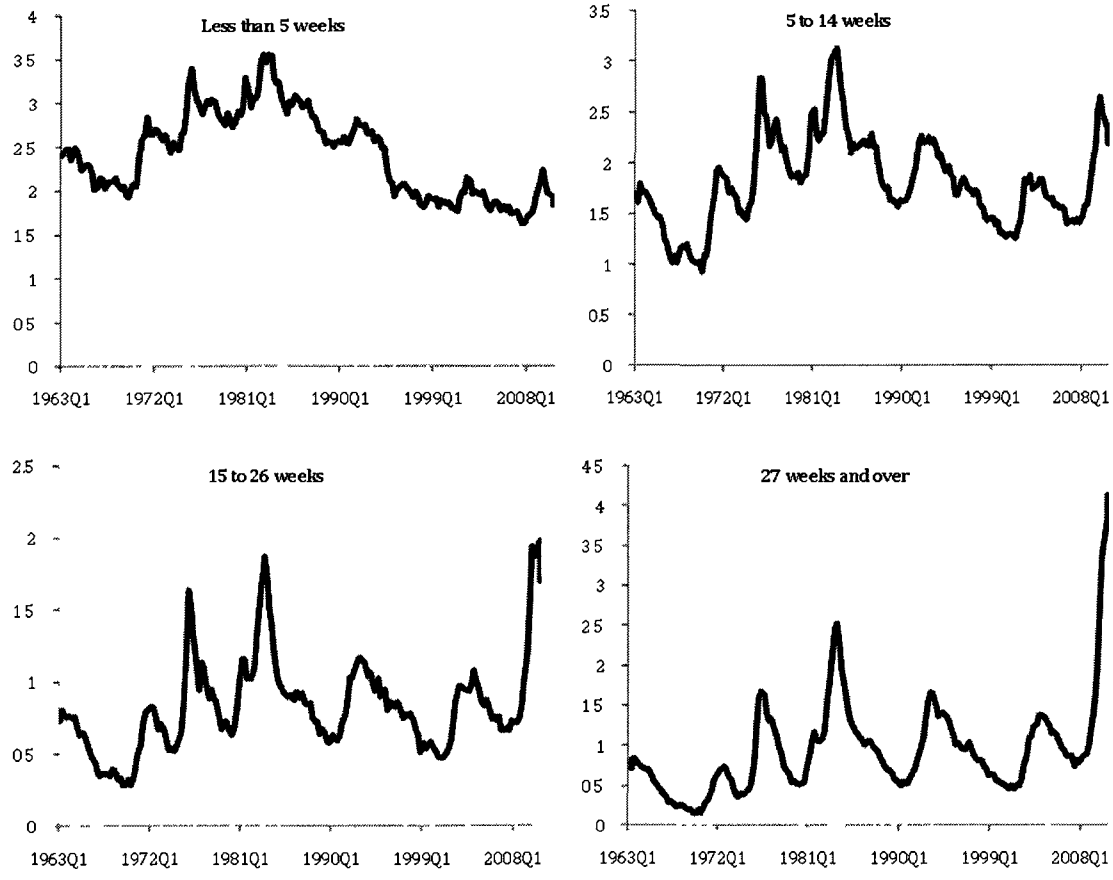

Figure 2.2 Duration of Unemployment (Percent of Labor Force)

Source: U.S. Bureau of Labor Statistics.

In a controversial paper, Lilien (1982) suggested that frictions associated with the reallocation of labor across sectors of the economy accounted for as much as half of all fluctuations in unemployment. Though Lilien's paper inspired a significant amount of follow-up work, the debate over the relative importance of sectoral shifts and aggregate shocks in unemployment fluctuations remains unresolved - see Gallipoli and Pelloni (2008) for a comprehensive critical review of the literature. We revisit Lilien's hypothesis in this paper using a suggestion by Black (1987), who conjectured that periods of greater dispersion in stock returns should be followed by increases in unemployment. The reason is that the stock market dispersion measure gives an "early signal of shocks that affect sectors differently, and puts more weight on shocks that investors expect to be permanent" (Black 1995). This latter point is important because it is presumably permanent shocks that motivate reallocation of labor across industries.

Loungani, Rush, and Tave (1990) implemented Black's proposal by constructing a measure based on the dispersion of stock market returns across sectors, and showed that increases in stock market dispersion were followed by increases in the unemployment rate. Brainard and Cutler (1993) proposed a related measure. Loungani and Trehan (1997) showed that the dispersion measure was more important for explaining long-duration unemployment than for explaining short duration unemployment.

This paper extends earlier work in a number of ways. The expansion of the sample to include the "Great Recession" is a key motivation; as indicated above, we would like to see how much of the recent rise in long duration unemployment can be explained by sectoral shocks. We also undertake two exercises that provide evidence on the robustness of our results, though in different ways. First, we compare our index to the index

proposed by Bloom (2009), who argues that stock market volatility (in the time dimension) provides a measure of uncertainty in the economy. Consistent with his argument that an increase in uncertainty has a *temporary* negative effect on the economy, we find that an increase in the volatility index raises short duration unemployment but has almost no effect on long duration unemployment. Our dispersion index, by contrast, has a larger effect on aggregate unemployment, and its effect increases significantly as we move from short to long duration unemployment. Second, we repeat our analysis using a new data set: a sample of 12 developed economies (excluding the U.S.). Consistent with the results for the U.S., we find that the unemployment rate increases significantly following an increase in stock market dispersion, even after we control for macro variables such as output growth, interest rates, etc.

2.2 Measuring Sectoral Shifts

The amount of labor reallocation that an economy has to carry out can change significantly over time. Some periods may be marked by relatively homogeneous growth in labor demand across sectors, whereas others may be characterized by shifts in the composition of labor demand. While beneficial in the long run, the reallocation of labor in response to sectoral shifts imposes short-run costs in the form of increases in unemployment. The greater the divergence in the fortunes of different industries, the more resources must be moved, and the larger will be the resulting increase in unemployment.

While these ideas are fairly intuitive, constructing a satisfactory measure of sectoral shifts poses an empirical challenge for a couple of reasons. First, as stated by Barro (1986), shocks to the expected profitability of an industry can arrive from "many - mostly

unobservable - disturbances to technology and preferences [that] motivate reallocations of resources across sectors." Second, Davis (1985) points out that "allocative disturbances *from any particular source* are likely to occur rather infrequently over available sample sizes," [italics ours] which makes it difficult to incorporate variables explicitly that capture the effects of sectoral shifts into an aggregate unemployment equation.

These considerations motivated Lilien's construction of a cross-industry employment dispersion index to proxy for the intersectoral flow of labor in response to allocative shocks. Many researchers, most notably Abraham and Katz (1986), however, have questioned Lilien's use of employment dispersion as a measure of labor reallocation. Their basic point is that movements in employment dispersion may simply be reflecting the well-known fact that the business cycle has non-neutral effects across industries. The increase in the dispersion of employment growth rates could reflect not increased labor reallocation, but simply the uneven impact of aggregate demand shocks on temporary layoffs in different industries. Under certain conditions - for instance, if cyclically responsive industries have low trend growth rates of employment - aggregate demand shocks also can lead to a positive correlation between the dispersion index and aggregate unemployment. Hence there is an observational equivalence between the predictions of the sectoral shifts hypothesis and the more traditional "aggregate demand hypothesis."

Loungani, Rush, and Tave (1990) and Brainard and Cutler (1993) attempt to circumvent these problems by constructing an index based on stock prices. Assuming that stock markets are efficient, so that shocks to the expected profitability of an industry are reflected in its stock market return, and assuming that these shocks are followed by

changes in that industry's use of inputs such as labor, their hypothesis is that the dispersion of stock returns across industries can be used as a proxy for shocks to the desired allocation of labor, i.e., as a measure of sectoral shifts. For instance, the arrival of positive news regarding the relative profitability of a particular industry is likely to be followed by an increase in stock price dispersion. In the long run, this news is likely to shift the economy's output mix towards the affected industry. This will necessitate a reallocation of resources, and the unemployment rate will rise as part of this process of reallocation of labor across sectors. Thus, an increase in stock price dispersion will be followed by an increase in the unemployment rate.

For this paper, we update the stock market index used in these earlier studies. The basic data consist of Standard and Poor's indexes of industry stock prices. There are over 50 industries, and they provide comprehensive coverage of manufacturing as well as nonmanufacturing sectors of the economy. The sectoral shifts index is defined as

$$Dispersion_t = \left[\sum_{i=1}^{n} W_i (R_{it} - R_t)^2 \right]^{1/2} \tag{2.1}$$

where R_{it} is the growth rate of industry i's stock price index, R_t is the growth rate of the S&P500 (a composite index), and W_i is a weight based on the industry's share in total employment. Hence, the sectoral shifts index can be interpreted as the weighted standard deviation of industry stock returns.

An advantage of the stock price dispersion measure relative to Lilien's measure is that unlike employment changes, stock prices respond more strongly to disturbances that are perceived to be permanent rather than temporary. The industry stock price represents the present value of expected profits over time. If the shocks are purely temporary, the

innovations will have little impact on the present value of expected profits and, hence, will have little impact on industries' stock prices. But persistent shocks will have a significant impact on expected future profits and will lead to large changes in industries' stock prices. Thus, a dispersion index constructed from industries' stock prices automatically assigns greater weight to permanent structural changes than to temporary cyclical shocks, and so will be less likely to reflect aggregate demand disturbances than a measure based on employment. Furthermore, it is these persistent shocks that are likely to cause productive resources, such as capital and labor, to move across industries.

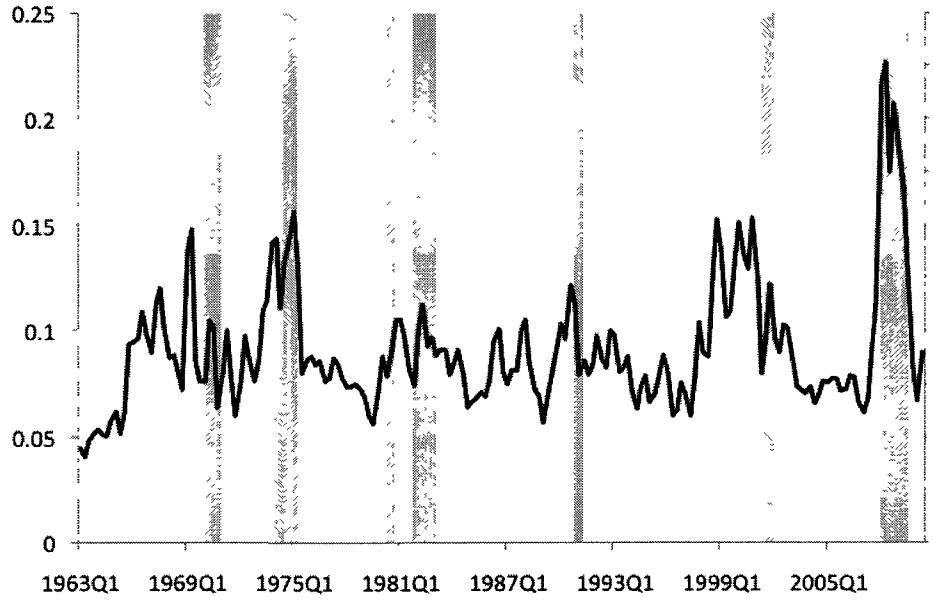

Figure 2.3 Stock Market Returns Dispersion Index (6-Month Moving Average)
Source: S&P 500 and own calculation.

Figure 2.3 shows the behavior of the constructed stock price dispersion index for the U.S. over the period 1963 to 2010. The index displays some cyclical behavior, with recession periods typically associated with an increase in the dispersion index. However, the correlation with the business cycle is far from perfect. For instance, the magnitude of

the increase in the index during a particular recession is not necessarily reflective of the depth of the recession. Increases during the 1974-75 recession and the 1990 recession, for example, are much higher than that experienced during the 1981 recession, even though the latter recession was more severe in terms of output loss. This evidence suggests that changes in the relative profitability of industries during a contraction are closely correlated with the size of the contraction, which is consistent with the interpretation that different recessions are marked by different mixes of sectoral and aggregate shocks. Similarly, the dispersion index has also recorded increases during expansionary periods. The dot-com boom experienced in the late 1990s provides a clear example, as stock prices in the information, technology, and communication sectors experienced much more rapid gains than the market average.

2.3 Candidate Explanations for Changes in Duration

The behavior of the unemployment rate can potentially be influenced by a variety of factors, both cyclical as well as structural, not all of which can be simultaneously included in a moderately sized VAR. Accordingly, in this section we compare the effects of the dispersion index on unemployment rates (of different durations) with the effects of other key macroeconomic variables, using a single-equation framework similar to Romer and Romer (2003) and Cerra and Saxena (2008). Specifically, we regress changes in the unemployment rate (ΔU) on its own lags as well as lagged values of the various "shocks". The lagged values allow for delays in the impact of the shocks on unemployment.[97] The estimated equation is:

[97] A lag length of 4 quarters was found to be optimal.

$$\Delta U_t = \alpha + \sum_{i=1}^{4} \Delta U_{t-i} + \sum_{i=1}^{4} Shock_{t-i} + \varepsilon_t \tag{2.2}$$

Four candidate shocks are evaluated using this framework. The first two are monetary and fiscal (tax rate) policy shocks, as identified by Romer and Romer (2003, 2010). Both these shocks are constructed so as to be exogenous to changes in output through the use of narrative records of the Federal Reserve Open Market Committee meetings, presidential speeches and Congressional reports. The third shock examined is related to oil prices, and is simply measured as the percentage change in the real price of oil. Finally, we look at the effect of changes in the stock price dispersion index. For each of these shocks, standard errors for the impulse response functions are estimated using a bootstrap method.[98]

The impact of a one standard-deviation change in the various shocks on the level of the unemployment rate is shown in Figure 2.4. The unemployment rate increases following each shock, though the magnitude and significance of the responses vary across shocks. The response of unemployment to a monetary policy tightening is particularly large and persistent. A one standard deviation shock to monetary policy results in an increase in the unemployment rate of about 0.6 percent after two years. Shocks to fiscal policy, on the other hand, are small and insignificant. In contrast, increases in the real price of oil are associated with increases in the unemployment rate, with the peak impact occurring after two years. Finally, increases in the dispersion index are associated with a positive and significant change in the unemployment rate. A one

[98] Specifically, 500 pseudo-coefficients are drawn from a multivariate normal distribution based on the estimates of the mean and variance-covariance matrix of the regression coefficient vector.

standard deviation increase in the index results in an increase in the unemployment rate of about 0.2 percent after a year and a half.

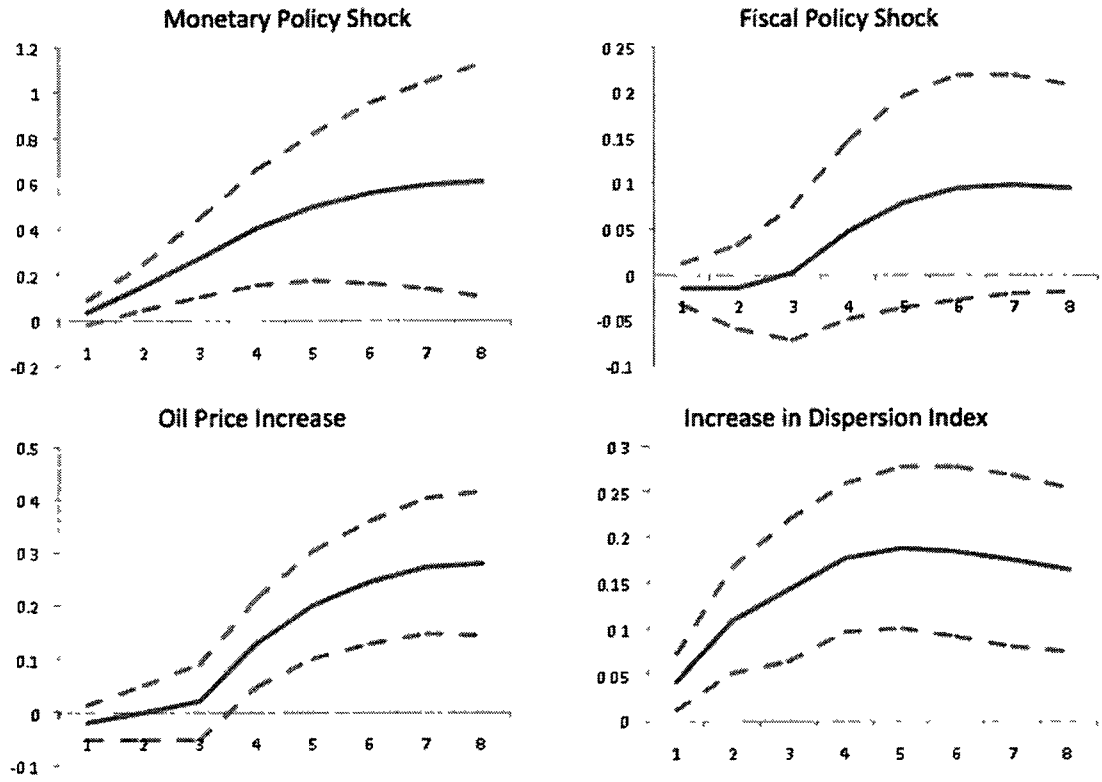

Figure 2.4 Unemployment: Impulse Responses When Shocks Assumed Exogenous

Notes: The graphs show the impact of a one standard-deviation change in the various shocks on the level of the unemployment rate in two years (eight quarters). The dashed curves mark out the 90% confidence region.

The long-term unemployment rate (where duration exceeds 26 weeks) responds very differently to these shocks; see Figure 2.5. The typical response of the long-term unemployment rate to either a monetary policy shock, a fiscal policy shock or an increase in the real price of oil is small in magnitude and of the *opposite* sign to the response of the unemployment rate observed in Figure 2.4. These responses are counter intuitive, but generally not significant at the 90 percent confidence interval. The response of the long-

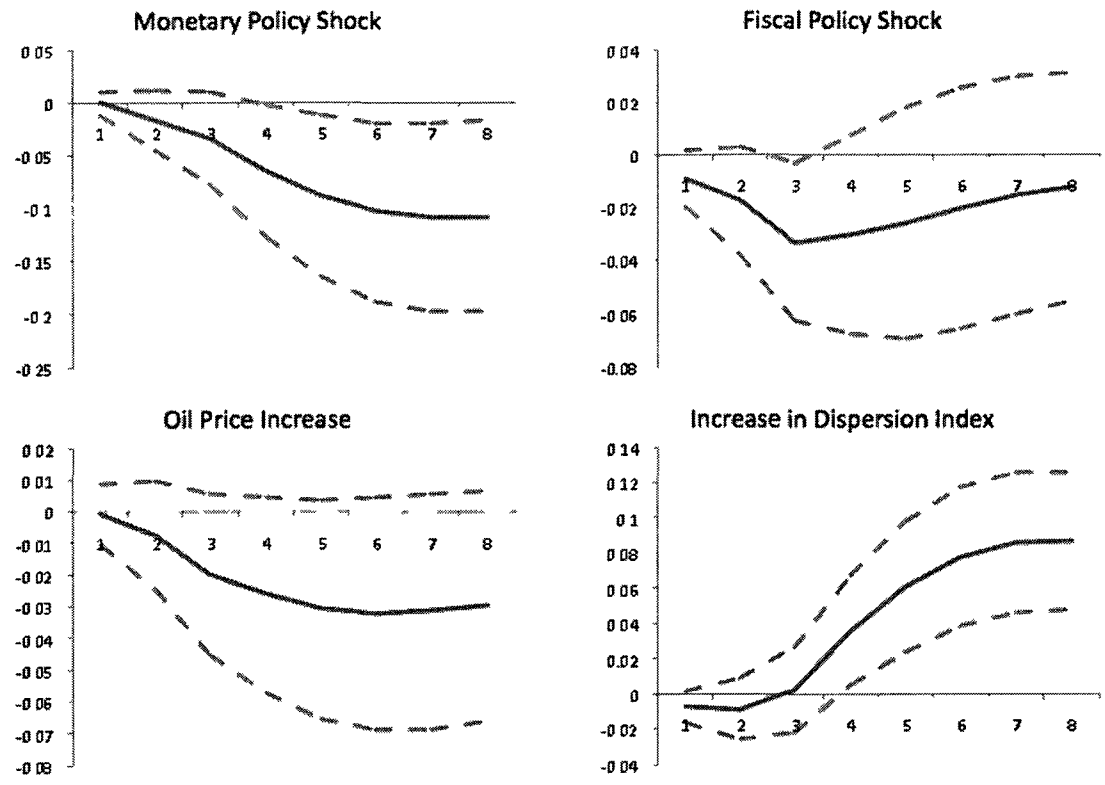

Figure 2.5 Long-Term Unemployment: Impulse Responses When Shocks Assumed Exogenous

Notes: The graphs show the impact of a one standard-deviation change in the various shocks on the level of the long-term unemployment rate (where duration exceeds 26 weeks) in two years (eight quarters). The dashed curves mark out the 90% confidence region.

term unemployment rate to an increase in the dispersion index, however, is positive (as it is in Figure 2.4) and statistically significant. A one standard-deviation increase in the index results in a gradual increase in the long-term unemployment rate, peaking at around 0.1 percentage points after two years. Given that the average long-term unemployment rate in the U.S. for the last 40 years is about 0.9 percent, the impact of changes in the dispersion index on long-term unemployment is relatively substantial.

The findings above highlight the importance of the dispersion index, relative to other standard macroeconomic factors, in explaining variations in unemployment - particularly,

long-term unemployment. Given our results here, we will not carry over the fiscal policy and oil price variables into the next section. Before going further, it is worth pointing out that a regression of the dispersion index on lagged values of the monetary and fiscal policy shocks revealed that the index is not systematically correlated with these macro shocks.

2.4 VAR Estimated on U.S. Data

In this section, we present the results from a VAR estimated on quarterly data from 1963:Q1 to 2010:Q2. The baseline model contains six variables, including the stock market dispersion index and the unemployment rate. The other variables are: real GDP growth, inflation, the federal funds rate, and the total return on the S&P500 index. The inclusion of real GDP controls for the stage of the business cycle; it also means that our model allows for a version of "Okun's Law." The variable measuring returns on the S&P500 index is included to rule out the possibility that the dispersion index explains unemployment because it is mimicking the behavior of the aggregate stock market. Finally, following Bernanke and Blinder (1992), the fed funds rate is included as a measure of monetary policy. The system is identified following the standard recursive ordering procedure. To avoid exaggerating the role of the dispersion index, we place it last in the estimation ordering. The other variables in the system are ordered as follows: GDP growth is placed first, followed by returns on the S&P 500, the unemployment rate, inflation and the fed funds rate. A lag length of 6 quarters was found to be optimal based on the Bayesian Information Criterion.

2.4.1 *The Effects of Sectoral Shocks*

Figure 2.6 shows how unemployment responds to different shocks to the system, along with the associated 90 percent confidence intervals.[99] The unemployment rate declines following a shock to output growth, with a one standard deviation increase in the output growth rate leading to a 0.3 percentage point decrease in the unemployment rate after one year. Innovations to the fed funds rate, meanwhile, result in higher unemployment. Focusing on the response of unemployment to innovations in the dispersion index, we see that the unemployment rate gradually increases, with the response reaching a peak of about 0.3 percentage points two years after the shock. The impact of these identified shocks to the dispersion index - purged of the aggregate influence of GDP, total market return, inflation and monetary policy - on unemployment is much higher than what we obtained from the regressions shown in the last section.

The long-term unemployment rate (Figure 2.7) shows a hump shaped response to innovations in the dispersion index, just as the overall unemployment rate (Figure 2.6). However, long-term unemployment reacts more gradually, reaching its peak only three years after the shock. The magnitude of the peak impact is again higher than what was found in the previous section. Long-term unemployment declines in response to output growth innovations, though just as with regards to dispersion shocks, the response is more delayed relative to the response of the overall unemployment rate. Long-term unemployment eventually increases following a shock to monetary policy, although the magnitude of the response is insignificant at the 90-percent confidence level.

A decomposition of the variance of unemployment forecast errors provides further evidence as to the importance of the dispersion index in explaining unemployment

[99] Standard errors are computed using the statistics based on the asymptotic distribution.

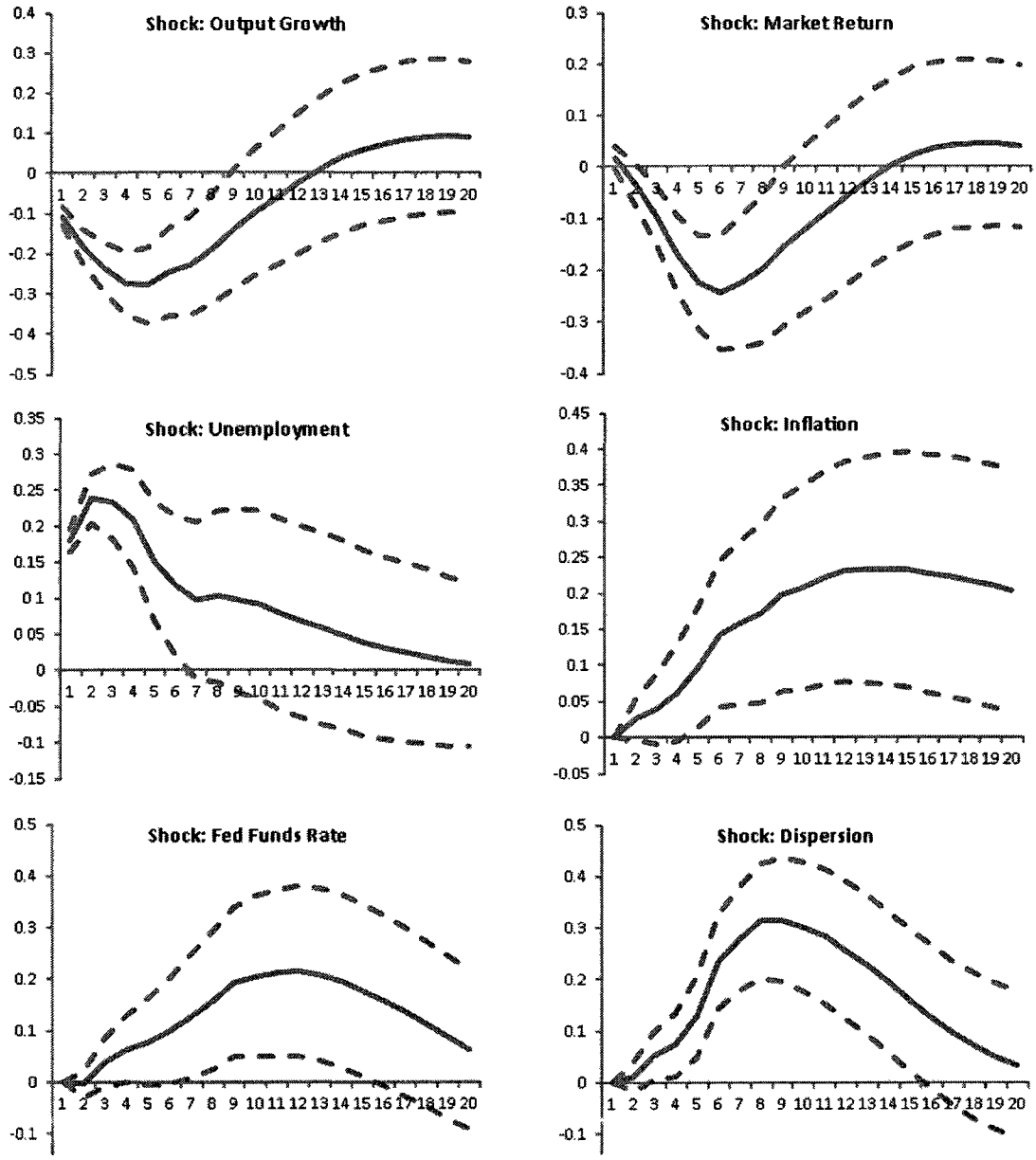

Figure 2.6 Unemployment: Impulse Responses from a VAR

Notes: These graphs are produced using a VAR model estimated on quarterly U.S. data from 1963:Q1 to 2010:Q2, which contains six variables with a lag length of 6 quarters in the following order: GDP growth, returns on the S&P 500, the unemployment rate, inflation, the fed funds rate, and the stock market dispersion index. Each graph shows how a one standard-deviation change in each of the six variables would affect the unemployment rate at a five-year horizon (20 quarters), based on the VAR model. The dashed curves mark out the 90% confidence region.

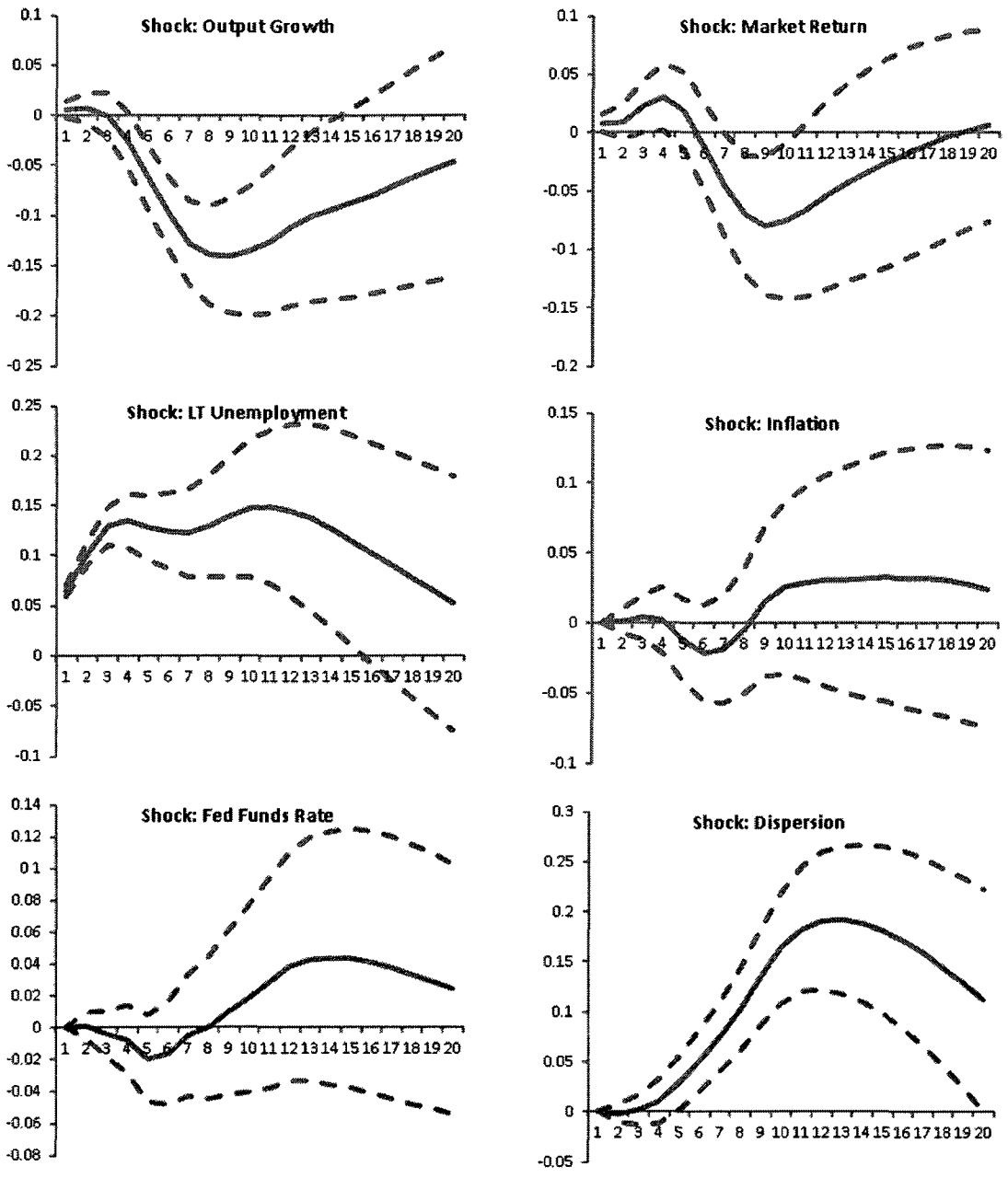

Figure 2.7 Long-Term Unemployment: Impulse Responses From A VAR

Notes: These graphs are produced using a similar VAR model to that in Figure 2.6, except that the variable *unemployment rate* is replaced by *long-term unemployment rate* (duration exceeds 26 weeks). Each graph shows how a one standard-deviation change in each of the six variables would affect the long-term unemployment rate at a five-year horizon (20 quarters), based on the VAR model. The dashed curves mark out the 90% confidence region.

fluctuations. Tables 2.1 and 2.2 show the proportion of the forecast-error variance of overall unemployment and long-term unemployment, respectively, that is explained by the various shocks, given our identification scheme. At the five-year horizon, about 25 percent of the variance of unemployment forecast errors is explained by innovations to the dispersion index. The proportion is much higher when we consider variations in long-term unemployment. Looking once again at the five-year horizon, innovations to the dispersion index account for up to 40 percent of the overall variance, making it more important than any other variable in the VAR, including the unemployment rate itself.

Table 2.1 Forecast-Error Variance Decomposition for the Unemployment Rate

Horizon (Quarters)	Growth	Market Return	Unemployment Rate	Inflation	Fed Funds Rate	Dispersion
5	41.9%	14.8%	34.7%	2.6%	1.9%	4.1%
10	24.9%	16.0%	15.1%	10.0%	8.2%	25.8%
20	16.6%	10.3%	9.8%	23.3%	14.4%	25.6%

Table 2.2 Forecast-Error Variance Decomposition for Long-Term Unemployment Rate

Horizon (Quarters)	Growth	Market Return	Long-Term Unemployment Rate	Inflation	Fed Funds Rate	Dispersion
5	6.4%	2.6%	88.8%	0.2%	0.6%	1.3%
10	26.6%	6.4%	46.5%	0.6%	0.3%	19.5%
20	19.3%	3.9%	32.9%	1.3%	1.7%	40.9%

Figure 2.8 shows how the role played by dispersion shocks changes as we look at different durations of unemployment. For each duration of unemployment, the figure shows the proportion of the forecast error variance of the unemployment rate explained by innovations to the dispersion index at a five-year horizon. In each case, the dispersion index is placed last in the ordering. The figure displays a striking pattern, where the

proportion of the variation in unemployment explained by shocks to the dispersion index increases monotonically with the duration of unemployment. For short-term unemployment (less than 5 weeks), shocks to the dispersion index account for less than 5 percent of the overall variation in the unemployment rate. At this duration, shocks to inflation and the unemployment rate itself account for the bulk of the variation. But at the other end, where duration exceeds 26 weeks, dispersion shocks account for about 40 percent of the variance of the forecast error.

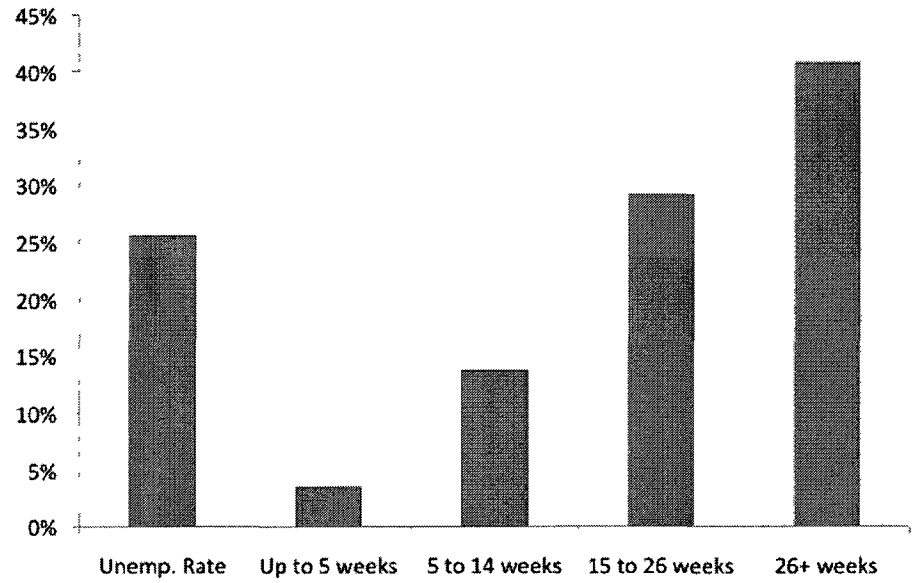

Figure 2.8 Variance of Unemployment Explained by Dispersion (at Horizon 20)

Notes: This figure shows the proportion of the forecast error variance of the unemployment rate explained by innovations to the dispersion index at a five-year horizon for each duration of unemployment.

2.4.2 *Sectoral Shocks and Long-Term Unemployment during the Great Recession*

We now use the VAR estimated above to examine long-term unemployment during the Great Recession. Long-term unemployment (defined as those who were unemployed for more than 26 weeks) constituted 16 percent of total unemployment in the fourth quarter of 2007 and 40 percent in the second quarter of 2010. Notably, it has remained high despite a resumption of growth in output.

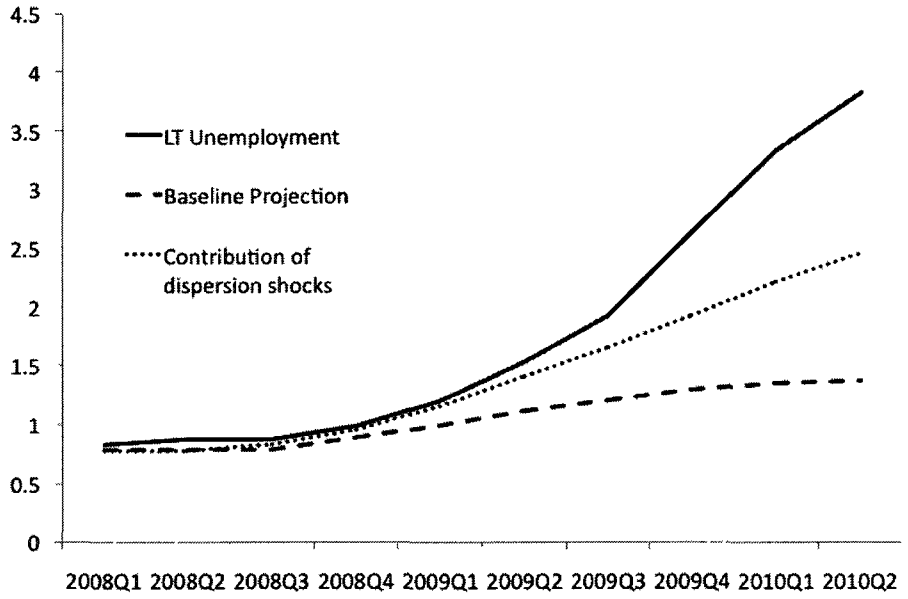

Figure 2.9 Decomposition of Long-Term Unemployment Rate During the Great Recession

Figure 2.9 plots the long-term unemployment rate since the beginning of 2008, together with two forecasts. The base period for both forecasts is chosen to be the fourth quarter of 2007, the start of the recession as declared by the National Bureau of Economic Research. The forecast horizon extends to the second quarter of 2010, or 10 quarters. The line labeled "baseline projection" plots the conditional expectation of the long-term unemployment rate over these 10 quarters as of 2007Q4. In other words, it is

the VAR's forecast of the long-term unemployment rate as of 2007Q4. For the first year of the forecast horizon, long-term unemployment remained close to the baseline projection, deviating at most by around 0.06 percentage points. Subsequently, however, long-term unemployment increased dramatically. During the second quarter of 2010, the long-term unemployment rate was more than 2 ½ percentage points higher than the baseline value.

The third line in the chart shows what the VAR's forecast of the long-term unemployment rate would have been if the orthogonalized dispersion shocks over the 2008Q1-2010Q2 period had been known at the end of 2007.[100] Dispersion shocks turn out to be extremely important in explaining the departure of the realized unemployment rate from the baseline forecast. From the beginning of 2009 up until the second quarter of 2010, shocks arising from the dispersion index accounted for more than half the difference between the actual long-term unemployment rate and its baseline projection. The contribution of shocks to GDP growth, on the other hand, averaged about 15 percent during the same period.

2.4.3 *Structural vs. Cyclical Movements in Unemployment*

Our framework can be used to estimate a measure of structural unemployment over the whole sample period. Each panel of Figure 2.10 plots the unemployment rate of a specific duration together with the corresponding estimate of structural unemployment, which is denoted with an asterisk. We define the structural unemployment rate to be the sum of the baseline unemployment rate and the contribution of dispersion shocks. The

[100] See Fackler and McMillin (1998) or Lutkepohl (2005) for details.

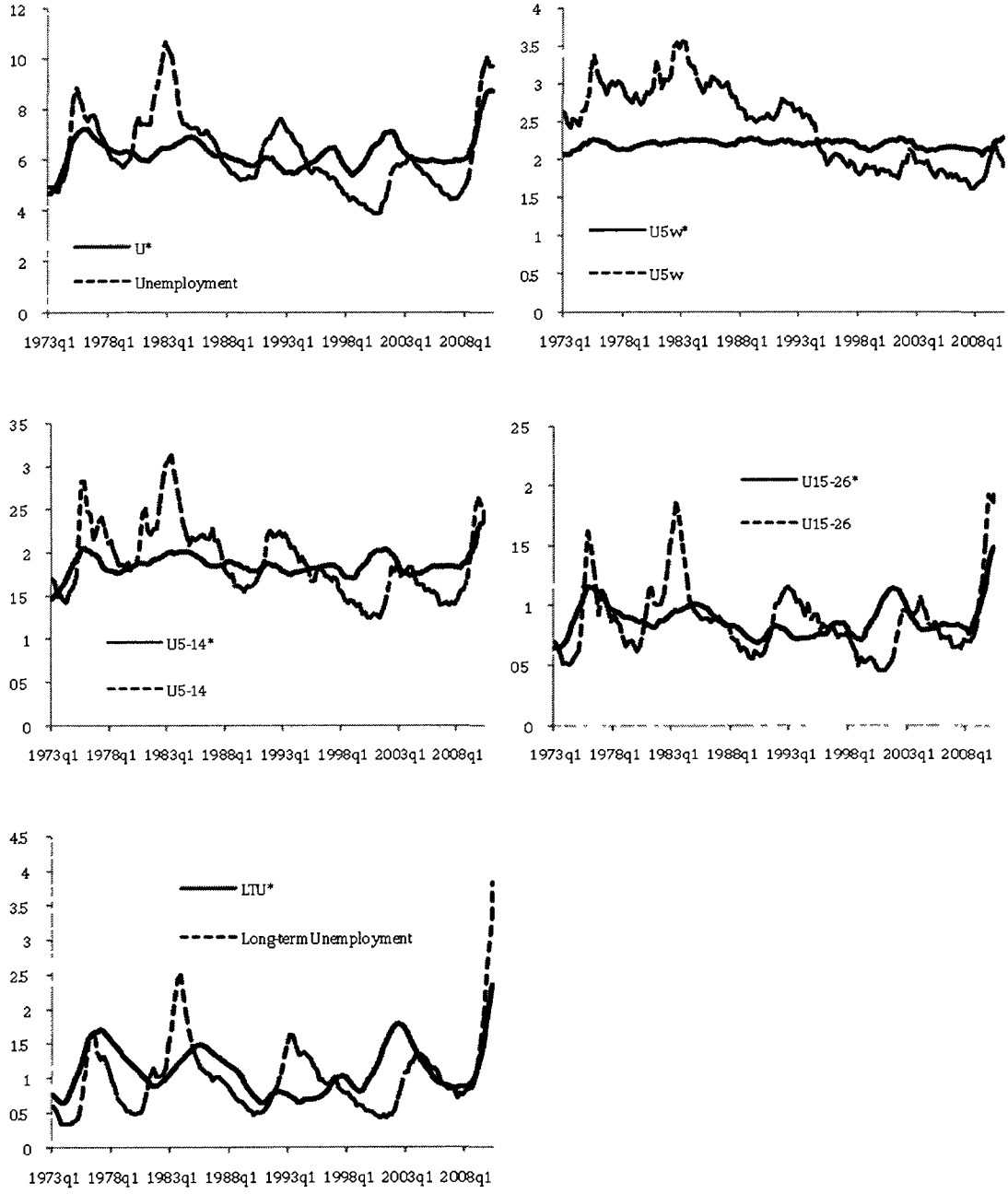

Figure 2.10 Estimates of Structural Unemployment

Notes: Each panel in this figure plots the unemployment rate of a specific duration together with the corresponding estimate of structural unemployment which is denoted with an asterisk for the period of 1973Q1-2010Q2. Structural unemployment rate is defined as the sum of the baseline unemployment rate and the contribution of dispersion shocks.

forecast begins at the beginning of our estimation sample, so the baseline rate (not shown) converges relatively quickly to the long-run average of the series. Thus, almost all the observed variation in the structural unemployment rate reflects dispersion shocks.

The gap between the estimated structural unemployment rate and the actual rate provides a measure of the effects of "cyclical" factors, which in our specification arise from (orthogonalized) innovations to the growth rate of output, total market return, inflation and the fed funds rate, as well as the innovations to the series itself. Cyclical factors appear to be more relevant for short-duration unemployment rates. For unemployment spells that are less than 5 weeks, for example, the estimated structural unemployment rate remains fairly constant through the whole sample compared to the actual rate, which is strongly cyclical (see the second panel of Figure 2.10). At the other extreme, when unemployment duration exceeds 26 weeks, the structural unemployment rate moves much more closely with actual unemployment. The behavior of the structural unemployment rate is also different across different recessions. Looking at the behavior of total unemployment (the first panel), the cyclical component appears to be relatively more important during the recessions of the early 80s and the early 90s and relatively less important during the mid-70s and the recent Great Recession.

2.5 Sectoral Shocks versus Uncertainty

We have shown that our measure of stock market volatility can help explain variations in the unemployment rate, and have argued that this measure represents the effects of sectoral shocks. But it is possible to place other interpretations on measures of stock market volatility. In particular, Bloom (2009) has advocated the use of one such measure as a proxy for uncertainty. He argues that an increase in uncertainty can have

significant negative effects on the economy, as firms optimally adopt a "wait-and-see" approach to capital expenditure and hiring decisions. As stressed by Bloom, these effects are temporary. His VAR estimates show that an uncertainty shock causes employment to fall sharply over the first 6 months, but the rebound is equally rapid. One year after the shock, employment is higher than it was prior to the shock.

Bloom's index differs from ours, in that it is a measure of time series volatility. More specifically, from 1962 to 1985, the series is based on the actual monthly standard deviation of the daily S&P 500 index. From 1986 onwards, the series is the VXO index of implied volatility constructed by the Chicago Board of Options Exchange. By contrast, our index is meant to capture cross section volatility; it measures how individual stock returns differ from the aggregate index at a point in time.[101]

Figure 2.11 plots Bloom's uncertainty index alongside the stock market dispersion index. The two measures tend to move together, particularly towards the latter part of the sample. The close correlation of the two series makes one wonder about how well the uncertainty index might explain unemployment, and, in particular, whether one would find a difference in how long-term unemployment responds to the dispersion index and how it responds to the uncertainty index.

To investigate this issue, we re-estimate the VARs from the previous section - which differ only in the duration of unemployment variable - with the uncertainty index replacing the dispersion series. There is no change to any of the other variables in the

[101] Bloom (2009) points out that his index is correlated with measures of firm level idiosyncratic shocks such as the distribution of profits across firms, a cross sectional TFP measure for manufacturing as well as a cross sectional monthly stock measure, and shows that regressing his volatility index on these measures leads to R^2s between 0.24 and 0.38, where the equations also contain controls such as industry and time dummies, etc.

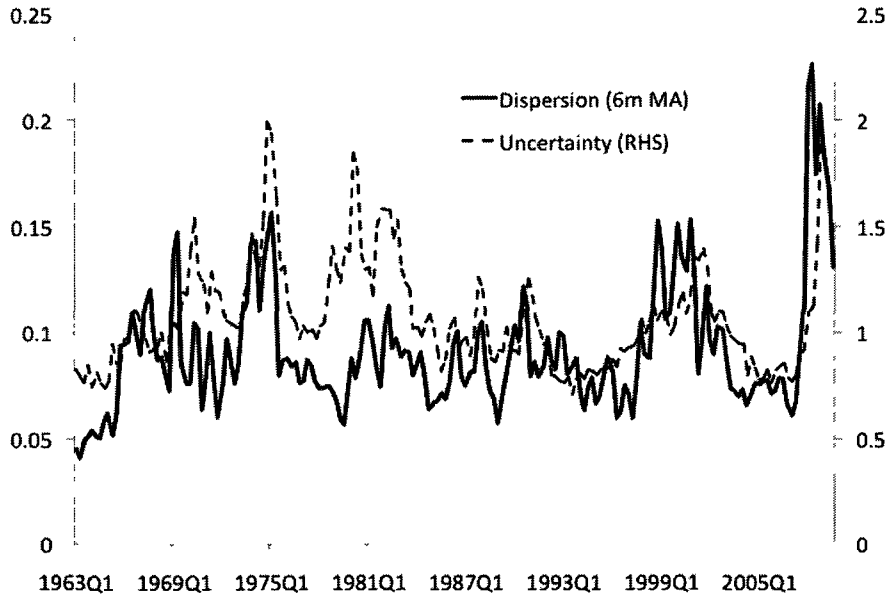

Figure 2.11 Alternative Measures of Stock Market Volatility

Notes: This figure plots Bloom's uncertainty index alongside the stock market dispersion index, which demonstrates a strong correlation between them, especially in the latter years.

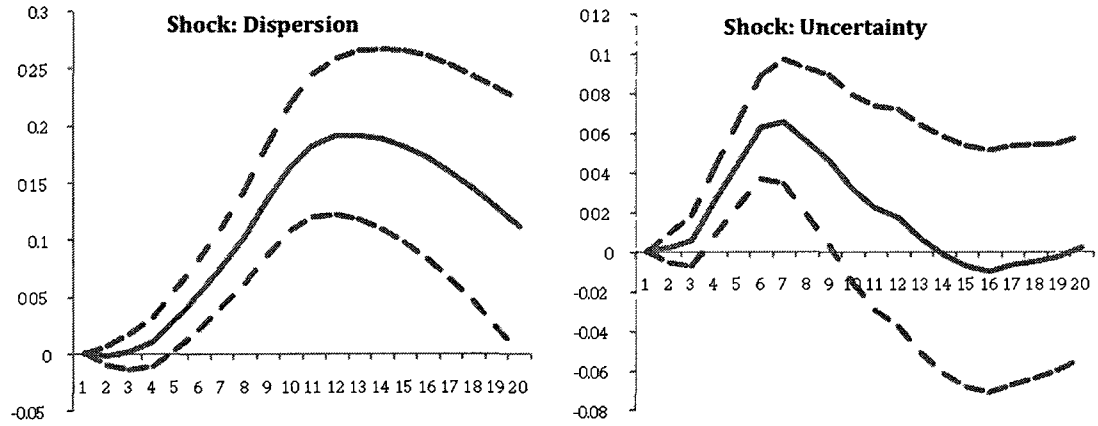

Figure 2.12 Comparing Long-Term Unemployment Responses to Dispersion and Uncertainty Shocks

Notes: The left panel is reproduced from the last graph in Figure 2.7. The right panel is produced using the same VAR model, except that the stock market dispersion index is replaced by the uncertainty index, also placed last in the ordering. Each graph shows how a one standard-deviation change in the corresponding variable would affect the long-term unemployment rate at a five-year horizon (20 quarters), based on quarterly data from 1963:Q1 to 2010:Q2. The dashed curves mark out the 90% confidence region.

system. Just as with the dispersion index, the uncertainty index is placed last in the ordering. Figure 2.12 plots the response of the long-term unemployment rate to uncertainty shocks from this system; for easier comparison, we also reproduce the corresponding graph from the original VAR. There is a noticeable difference in how the long-term unemployment rate responds to the two shocks. For one thing, long-term unemployment tends to peak earlier in response to an uncertainty-index shock (about 6 to 7 quarters) than to a dispersion-index shock (about 12 to 13 quarters). And the effect of the former is statistically indistinguishable from zero after about two years, while the effect of the latter can be distinguished from zero for about five years after the shock. The magnitude of the responses is also markedly different, with the peak response to a one-standard-deviation shock to the dispersion series about three times greater than the peak response to a one-standard-deviation shock to the uncertainty index.

Comparing the results across different unemployment durations reinforces the difference between the indexes. As the variance decompositions in Figure 2.13 reveal, the uncertainty index does best in explaining short duration unemployment. For unemployment duration of 5 weeks or less, it explains more than a fifth of the variance of unemployment (at a horizon of 20 quarters). For unemployment durations of 15 weeks or longer, the uncertainty index explains noticeably less than one tenth of the total variance. This is reminiscent of the findings in Bloom (2009), where uncertainty shocks appear to have a greater impact on activity at the shorter horizon. By contrast, the importance of the dispersion measure actually increases as the unemployment duration goes up (Figure 2.8), and it explains roughly 40 percent of the variance of the longest duration unemployment. Notice also that the dispersion index explains about 25 percent of the variance of the

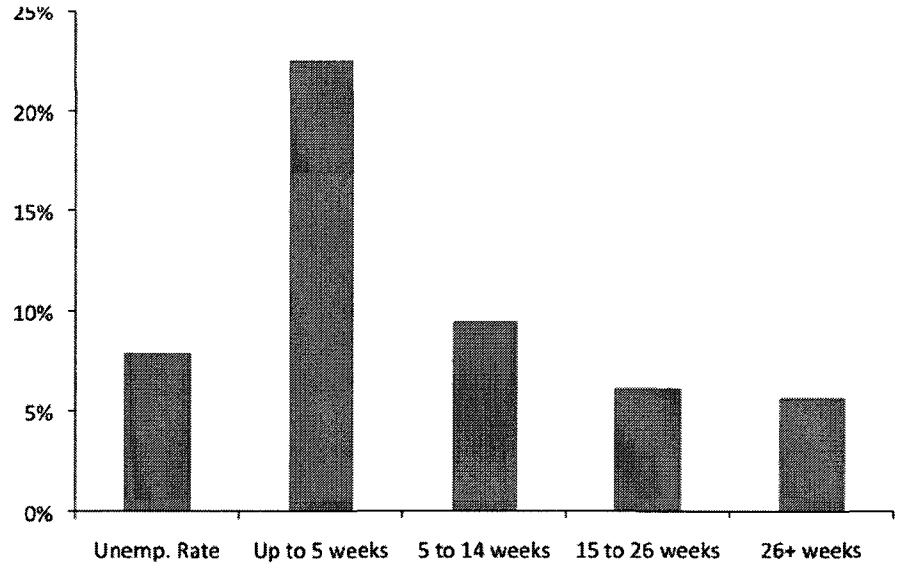

Figure 2.13 Variance of Unemployment Explained by Uncertainty (at Horizon 20)

Notes: This figure shows the proportion of the forecast error variance of the long-term unemployment rate explained by innovations to Bloom's uncertainty index at a five-year horizon for each duration of unemployment.

Table 2.3 Forecast-Error Variance Decomposition for the Long-Term Unemployment Rate - Augmented System

Horizon (Quarters)	Growth	Market Return	Long-Term Unemployment Rate	Inflation	Fed Funds Rate	Uncertainty	Dispersion
5	11.2%	0.2%	76.5%	2.5%	3.6%	5.5%	0.4%
10	30.6%	8.5%	37.3%	1.2%	2.6%	6.0%	13.8%
20	22.0%	6.8%	31.3%	4.1%	4.8%	3.7%	27.2%

aggregate unemployment rate, while the uncertainty index explains less than 10 percent.

As a final check, we included both the uncertainty measure and the dispersion index in the VAR, with the dispersion index being ordered last and the uncertainty measure just above it. Both the response function of long-term unemployment (not shown), as well as the variance decomposition (Table 2.3), show that the dispersion index continues to play

a significant role. For instance, at the 20-quarter horizon, the dispersion index explains about 27 percent of the forecast error variance of the long duration unemployment rate, while the uncertainty index explains less than 4 percent.

2.6 A VAR Estimated on International data

In this section, we present cross-country evidence on the importance of the stock market dispersion index in explaining unemployment fluctuations. The suitability of this index as a proxy for sectoral shocks will differ across countries depending on the depth of their stock markets and whether or not the set of listed firms is representative of the whole economy. The dispersion of stock market returns in relatively thin markets, for example, could be very volatile due to the influence of a few large firms, or of foreign capital flows. In order to minimize such distortions, we limit our analysis to a sample of advanced economies that have relatively deep and broad stock markets. The countries in our sample are Australia, Austria, Denmark, Finland, France, Germany, Italy, Netherlands, Norway, Portugal, Sweden, and the United Kingdom.[102]

We did not add more countries to our sample because we were unable to get stock market data of sufficient length. The move to an international setting forced us to confront several other data issues as well. While data on stock returns at the industry level are obtainable across countries, comparable disaggregate data on employment by industry are not consistently available. A breakdown of sectoral employment at the broad national accounts classification is available, but this is too coarse a breakdown to be applied to the more disaggregated industry stock return data. As a result of this data

[102] Adding the U.S. to this sample does not make a material difference to the results below.

limitation, we weight the stock returns by the industry's share of market capitalization. To minimize large fluctuations in these shares, we use a rolling ten-year average. A further data complication is the lack of cross-country measures of monetary policy. Changes in nominal short-term interest rates, unlike the fed funds rate, contain both policy-induced changes as well as other endogenous responses to disturbances unrelated to policy shifts. Still, for this set of advanced economies, monetary policy is best represented by changes in interest rates. Therefore, instead of the nominal rate, we combine the measures of inflation and the nominal interest rate to construct an *ex post* real interest rate and include it in the VAR. Finally, long-term unemployment rate data are not consistently available for these countries, i.e., we are unable to get data series of sufficient length. Consequently, the analysis is limited to the overall unemployment rate.

The setup of the VAR is similar to the previous section, but now the data have both a cross-sectional and time-series dimension. As such, we estimate a panel VAR (see Love and Zicchino, 2006, for example) where the coefficients on the VAR are restricted to be the same across all cross-sectional units. Country-specific fixed effects, however, are included. The ordering is the same as it was in the previous section: GDP growth is ordered first, followed by total market return, unemployment, real interest rate and, lastly, the dispersion of stock market returns. GMM methods are used to estimate the system over the period 1965:Q2 to 2008:Q3 (see Arellano and Bond, 1991, 1995).[103] The lag length is set at 12 quarters.

The impulse-response functions for unemployment are shown in Figure 2.14. Each panel shows the response of the unemployment rate to a one-standard-deviation shock,

[103] For most countries, however, the stock market data only starts in 1973.

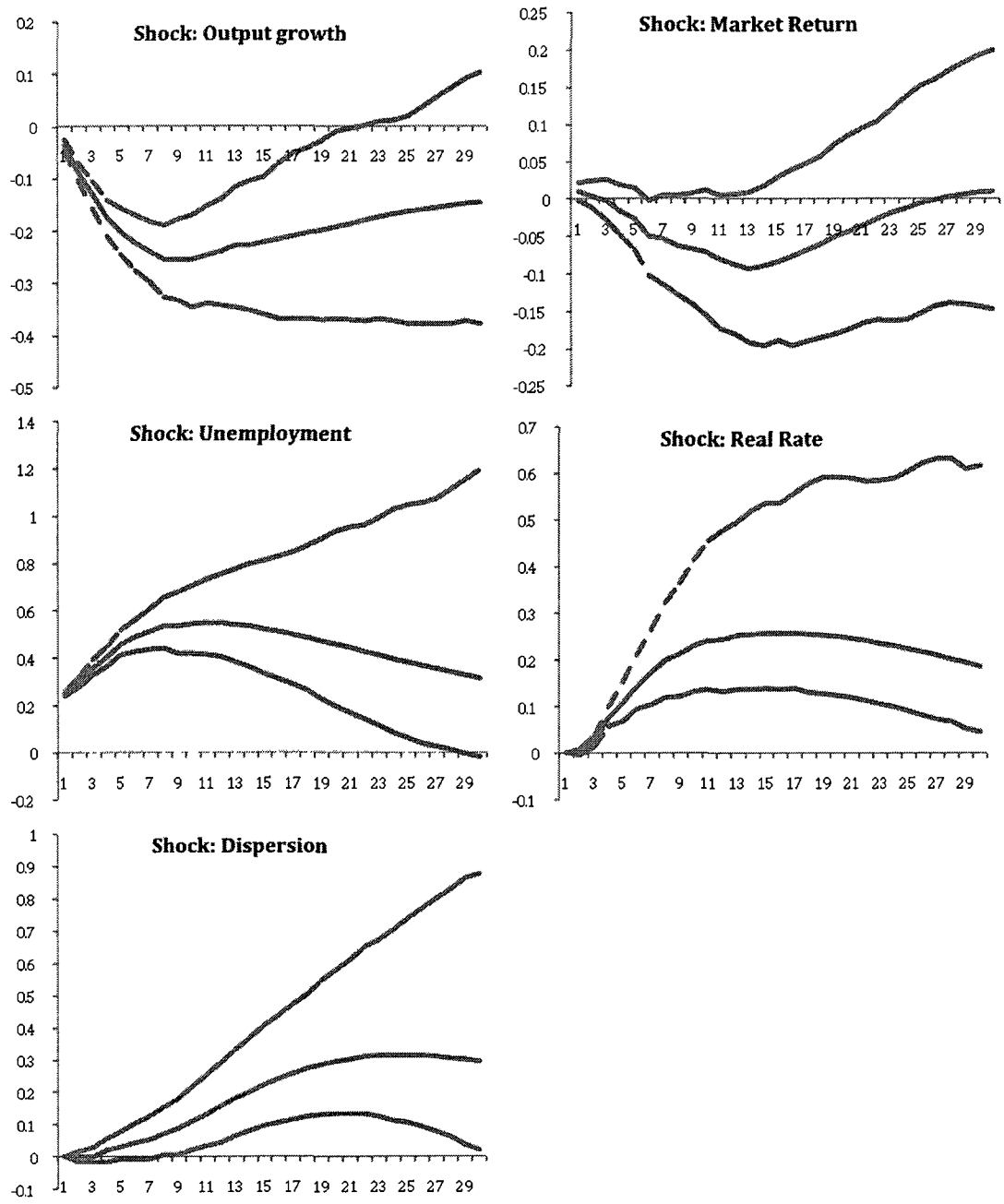

Figure 2.14 Unemployment: Impulse Responses From International Panel

Notes: These graphs are produced using a VAR model estimated on quarterly data of 14 countries pooled together from 1965:Q2 to 2008:Q3, which contains six variables with a lag length of 12 quarters in the following order: GDP growth, returns on the S&P 500, the unemployment rate, inflation, the fed funds rate, and the stock market dispersion index. Each graph shows how a one standard-deviation change in each of the six variables would affect the unemployment rate at a five-year horizon (20 quarters), based on the VAR model. The lower and upper curves mark out the 90% confidence region.

with the residuals orthogonalized according to the ordering above. The impulse-response graphs look similar to those for the U.S. For instance, unemployment increases after an increase in stock market dispersion and decreases after a positive growth shock. Interestingly, the magnitude of the responses is also of the same order. A one-standard-deviation shock to the dispersion index results in a maximum increase of about 0.3 percentage points in the unemployment rate here, just as it did for U.S. data. However, the response of unemployment to the dispersion shock is noticeably delayed; it also persists for much longer. In the cross-country data, the peak impact on unemployment is reached only after 6 years, while the peak in U.S. data is reached after two years.

The other impulse responses also show more persistence in the international panel than they do for the U.S. data. For instance, the response of the unemployment rate to an unemployment rate shock is statistically indistinguishable from zero less than two years after the shock in the U.S. data set (Figure 2.6), but can still be distinguished from zero more than six years after the shock in the international data set (Figure 2.14). Many observers have noted the tendency of the unemployment rates in European countries[104] to stay high for long periods of time following adverse shocks. This has led to discussions of hysteresis in the unemployment rate; see Blanchard and Summers (1986) for an early example, or Blanchard (2006) for a more recent discussion.

The dispersion index also continues to account for a significant proportion of the forecast error variance for unemployment, and - as in the U.S. data - its importance grows over time. Table 2.4 shows that at a forecast horizon of 40 quarters, dispersion shocks account for about 18 percent of the variation in the unemployment rate. This is about

[104] With the exception of Australia, all the countries in our sample are in Europe.

Table 2.4 Forecast-Error Variance Decomposition for the Unemployment Rate - International Panel

Horizon (Quarters)	Growth	Market Return	Unemployment Rate	Real Interest Rate	Dispersion
10	15.2%	0.8%	75.3%	7.6%	1.1%
20	12.8%	1.1%	66.1%	12.0%	8.0%
40	11.4%	0.7%	57.0%	13.0%	17.9%

three-fourths of what we get for the U.S. data at the same horizon. And just as in the U.S. data, apart from shocks to the unemployment rate itself, dispersion shocks are the most important in explaining the variance of the unemployment rate at long horizons.

2.7 Conclusion

We have shown that sectoral shocks (as measured by an index of the cross section variance of stock prices) have a substantial impact on the unemployment rate in a sample that includes the Great Recession of 2007-2009. Further, these shocks become more important as the duration of unemployment increases, a finding that accords with the intuition that such shocks should be associated with longer spells of search, as they cause workers to move across sectors.

An examination of the Great Recession shows that sectoral shocks account for about half of the increase in the long duration unemployment rate that has taken place over this period. Once again, this accords with informal evidence about employment conditions in the construction sector and, to a lesser extent, in finance. In this, the Great Recession is similar to the recession of 1973-75, as sectoral shocks appear to have played a large role at that time as well.

We have also shown that our measure of cross section volatility is quite different from the measure of time series volatility proposed by Bloom (2009). In particular, the time series measure does better at explaining short duration unemployment, but badly at explaining long duration unemployment. By contrast, our measure does better the longer the duration of unemployment under consideration. We interpret these findings to mean that both measures are well suited to the purpose for which they were designed. The time series measure is meant to capture uncertainty, and Bloom emphasizes that uncertainty has a short run effect. By contrast, our measure is meant to capture shocks that cause reallocation across sectors, and such reallocation is going to take time.

Finally, we have shown that the dispersion index continues to matter in a sample of a dozen advanced economies. While we have not been able to find data on long duration unemployment for these countries, we have shown that an increase in stock market dispersion leads to a substantial and significant increase in the unemployment rate, even after we control for output, inflation and the value of the stock market.

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[a] Demeaned matching variables include: homestead area, construction year of the house, historical construction cost, housing rental income, distance to town center, self-farmed area, farm rental income, household agricultural income, household non-agricultural income. The individual-level variables are not demeaned, because they are mostly categorical variables, or variables without much cross-town variation such as age and years of schooling.
[b] The number in parentheses denotes the total number of observations (both on and off support).

Chapter 3

Financial Development, Fixed Costs and International Trade*

3.1 Introduction

Recent research has emphasized that financial development may play a role in determining economic development (see e.g. King and Levine, 1993; Jayaratne and Strahan, 1996; Aghion, Howitt and Mayer-Foulkes, 2005). The literature has highlighted that financial development may be more important in promoting certain industries that require large amounts of external finance (see e.g. Rajan and Zingales, 1998; Wynne, 2005). We extend this argument to the area of international trade. Based on standard corporate finance theory, we predict that financial development will help finance trade-related investments that are likely to be particularly difficult to finance.

Corporate finance theory suggests that financing problems may be particularly acute for investments that are intangible or difficult to observe, or that are firm- or person-specific (see e.g. Myers and Majluf, 1984; Harris and Raviv, 1990). We investigate one

* This paper was written jointly with Bo Becker (Harvard Business School) and David Greenberg (Blackrock Capital). We wish to thank Xiang Ao for research assistance, and John Romalis, Luigi Zingales, Atif Mian, Per Strömberg, Raghuram Rajan, Jens Josephson, René Stulz, Ulf Axelson, Thorsten Beck, Anil Kashyap, Marianne Bertrand, Pol Antràs as well as seminar participants at the CEPR/Banca D'Italia conference on Financial Structure, Product Market Structure and Economic Performance, Chicago, Rochester, Purdue, Cornell, Ohio State, Harvard Business School, Amsterdam, HEC Montréal and Illinois for comments and suggestions. We especially want to thank Davin Chor for sharing data and advice on SMM.

particular type of investment which fits this description - the up-front investments (fixed costs) required to enter export markets. There is considerable evidence that exporting firms face large fixed costs (see e.g. Roberts and Tybout, 1997; Bernard and Wagner, 2001; Bernard and Jensen, 2004). Fixed costs likely include identifying potential target markets, developing distribution networks and adapting products to match foreign regulations and tastes. Such fixed costs may be intangible and difficult to observe, have long gestation periods and be firm- or even person-specific. Their return may be difficult to pledge. These are precisely the kind of investment that would be difficult to finance externally if there was a low financial development. We add to the growing understanding of financial development by pointing to the likely importance of financial development for *firms wishing to export.*

We consider three hypotheses based on the interaction of fixed costs of trade and financial development. First, financial development should have a positive impact on the overall level of exports because more firms can find outside financing. Second, we expect finance to be particularly important where up-front costs are higher. Third, since the fixed costs incurred prior to starting exports are likely to be recipient-specific, switching export across countries is likely to involve further fixed costs, whether new or experienced exporters are involved. Therefore, we predict that exports will be more responsive to changes in the exchange rates in countries with more developed financial systems. This is the third prediction we aim to test.

The first hypothesis, that trade and financial development should be positively correlated has been examined previously (see, e.g. Beck, 2003). Beyond standard trade determinants such as size, location and income, financial development helps explain the

trade volumes in cross-sectional regressions. However, this type of test is not well identified, in that the potential for omitted variable bias is large (some variable which is both correlated with financial development and important for trade may be unobserved, unmeasured, or for other reasons left out of the econometric specification).

In this paper, we examine the second and third prediction of the fixed cost financing argument.[105] We base our tests on the idea that exports vary in the amount of fixed costs they entail. Exporting highly differentiated products that are intensive in R&D and advertising, or that must be tailored to a distant foreign market, likely requires more up-front investment than exporting a commodity good to a nearby country. We construct proxies for fixed costs reflecting differences in product and country-pair characteristics. In estimations of gravity equations with financial development included, we find that exports are more sensitive to financial development when fixed costs are high, confirming the second prediction. When using industry-level data, we include a wide set of controls for comparative advantage based on factor abundance and institutional development. We estimate aggregate regressions with OLS and generalized method of moments (GMM), and industry-level trade regressions with both OLS and simulated method of moments (SMM). As the bilateral trade flows contain a large number of zero trade observations, taking logs entails either dropping them out or performing some transformation, therefore the GMM and SMM methodologies, which match moments in the data to model predictions, are used here mainly to address this issue. Statistical findings are robust to all methodologies.

[105] Regarding the first hypothesis, we have estimated the standard gravity equation using our basic data set, with financial development as one explanatory variable. Our results confirm that higher levels of financial development increase exports. The results are not reported here for succinctness.

Finally, we investigate the third prediction, that financial development should affect how exports react to movements in exchange rates. In panel data, we test whether the exchange rate elasticity of aggregate exports is higher in countries with well-developed finance. We find strong evidence for this. To address endogeneity problems with exchange rates (i.e. macroeconomic shocks will affect both exports and exchange rates), we study *relative* export growth across different importing countries, but holding the exporter fixed (by including exporter-year fixed effects). We find that the relative growth of exports (across different importers) is more sensitive to exchange rate movements in countries with higher financial development. For example, if the Japanese Yen rises relative to the British Pound, exporters will see a higher relative growth of exports to Japan than to Britain on average, but this effect is stronger in countries with better financial development. The pattern is particularly strong for differentiated products. This is consistent with the hypothesized mechanism of exporters in financially developed countries having better access to financing for large upfront costs involved with starting to export.

Our contribution to the literature on finance and growth consists in showing that finance is particularly important for the export performance of an economy, and hence its ability to participate in the international exchange of goods and ideas. In terms of international trade, our contribution is to point out how fixed costs will interact with financial development and that industry heterogeneity in such costs will generate predictable cross-industry patterns in international trade. Our results regarding exchange rate elasticities emphasize the potential importance of domestic financial frictions for international capital flows.

The rest of the paper is organized as follows: Section 3.1 provides some background on two key related literatures, finance and growth, and sunk costs of trade. Section 3.2 discusses fixed costs and how to measure them. Section 3.3 describes the data and Section 3.4 the empirical results from the cross-sectional and time series specifications. Section 3.5 presents robustness tests: regressions with comparative advantage controls. Section 3.6 presents the empirical methodology and results for exchange rate elasticity tests. Section 3.7 concludes.

3.2 Background and Literature

3.2.1 *Financial Development and Economic Performance*

Our work is related to a large empirical literature that establishes the link between finance and growth (see e.g. Goldsmith, 1969; King and Levine, 1993; Jayaratne and Strahan, 1996; Wurgler, 2000; Rajan and Zingales, 1998). The recent wave of research on the growth implications of finance was initiated with cross-country findings that growth rates are higher in countries with more developed financial sectors (King and Levine, 1993). Since it is difficult to control for all conceivable determinants of growth, the cross-country regressions used to establish these findings potentially suffer from omitted variable bias. Some recent work has circumvented this problem by focusing on within-country, between-industry variation (e.g. Demirgüç-Kunt and Maksimovic, 1998; Rajan and Zingales, 1998; Claessens and Laeven, 2003; Carlin and Mayer, 2003; Braun, 2003). Rajan and Zingales (1998) find that industries which are more in need of outside

financing grow more quickly when the financial sector is more developed.[106] Our paper relates to this branch of the growth and finance literature methodologically, since we also exploit within-country variation (we use variation in fixed costs both across industries and across importers for a given exporter).

Our paper obviously differs from the finance and growth literature in that we study the effects of finance on trade rather than income. However, the positive effect of financial development on export performance is a potential mechanism through which financial development may affect overall income and growth rates. In that sense, our results can be seen as advancing the understanding of how financial development affects real outcomes.

3.2.2 *Institutions and Trade*

The impact of institutions on international trade has received growing attention recently. This developing literature emphasizes how institutional quality can affect comparative advantage and international specialization. Grossman and Helpman (2003, 2005) and Antràs (2003) study how institutional problems limit the amount of outsourcing to low-wage countries. Levchenko (2007) models how institutions generate comparative advantage in certain industries and provides evidence that the composition of international trade is affected by property rights protection and the rule of law. Nunn (2007) empirically establishes that countries with stronger contract enforcement ability

[106] Carlin and Mayer (2003) show that the growth rate of industries that are dependent on bank financing (as opposed to outside finance in general or equity finance) or skilled labor are sensitive to financial development. Braun finds that industries which make large investments in intangible assets grow relatively faster in countries with a well developed financial system.

will have a comparative cost advantage in the production of goods requiring relationship-specific investments, which explains more of the pattern of trade than physical capital and skilled labor combined.[107]

Institutions and finance are empirically hard to tell apart for at least two reasons.[108] First of all, institutions and financial development are poorly measured. This means that comparing which matters more for real outcomes is empirically challenging. Second, the distinction between institutions and financial development is subtle because it is possibly not a simple matter of either or. The quality of contract enforcement could be important for trade, but work mainly through its effect on financial contracting. We propose no firm conclusions about this issue but include a rich set of institutional controls following Chor (2010). We also show that exports are more responsive to exchange rates when finance is better developed (prediction three). As far as we are aware, these two predictions do not follow directly from a contractibility theory.

3.2.3 *Trade during the Financial Crisis*

The arguments about fixed costs of trade and financial development are consistent with a negative effect of the 2008-2009 financial crisis on international trade, as has been documented by Wang and Whalley (2009). The theory would predict specific patterns in the trade from countries where the financial system ceased to function well at this time. First, existing exporters, who have already incurred fixed costs, should suffer less than

[107] Our results on overall export levels are similar in nature to Rose's (2005) study of the effects of policies and institutions on trade levels.

[108] For a recent analysis of the distinction, see Acemoglu and Johnson (2005).

new exporters (this is related to predictions in Melitz, 2003). Second, the fall should be particularly steep in industries where intangible, fixed, up-front costs are large. Chor and Manova (2010) find something similar: exports in industries with high financial dependence in the U.S. (based on Rajan and Zingales, 1998) appear to respond more to financial problems during 2009.

3.2.4 *Other Related Literature*

Our results on the industry composition of trade (high fixed cost vs. low fixed cost industries) are related to recent papers that relate financial development to comparative advantage, e.g. Svaleryd and Vlachos (2005) and Beck (2003). These studies show that financial development is correlated with net exports in industries that depend on external finance (Svaleryd and Vlachos, 2005), and in manufacturing (Beck, 2003). In an important respect, this study follows a similar path. It differs in that we study the implication of a particular reason why finance may matter for exports (fixed costs are hard to finance externally). We sort industries based on fixed costs involved in trading, rather than their dependence on external finance. This is an advantage when attempting to identify the effects of financial development on trade, because we are less likely to find an effect that reflects general equilibrium re-allocation effects (as in Rajan and Zingales, 1998), and more likely to find trade-specific effects of financial development. Also, since our arguments apply less to imports than to exports, it is cleaner to test using exports as the left-hand side variable (not trade balances). Finally, we examine exchange rate elasticities as well as export levels, confirming a set of predictions that follow from fixed costs but which are not implied by general financing frictions.

The paper closest to our study is Manova (2006). Manova models the impact of financial development on international trade when firms are credit constrained and industries differ in their financial vulnerability. Manova, just like us, confirms that financial development helps predict the industry composition of trade. There are three differences. First, her predictions about where finance matters (e.g., which industries) differ. Her industry ranking follows Rajan and Zingales, which is based on predicted needs for external finance in general, whereas we construct proxies for the fixed costs that are known to be important for international trade specifically. Second, Manova's regressions are based on time series variation in private credit, whereas we use cross-sectional variation in private credit as well as accounting standards. The advantage of using a pure cross-country measure is that there is less concern about business cycle contamination of the identification. Third, Manova does not consider how the exchange rate elasticity of exports varies with financial development.

Our findings are related to the firm-level study of Desai, Foley and Forbes (2008), which compares the response of U.S. multinational subsidiaries and local firms in developing countries to currency crises. Desai et al. find that both local firms and multinationals expand output and investment in the wake of currency crises (which amount to large depreciations of the local currency) but that multinationals expand much faster. This is consistent with the idea that low financial development inhibits expansion by local firms in response to exchange rate movements, and that U.S. multinationals with access to large internal capital markets (and indirectly to U.S. financial markets), are better able to capture opportunities arising after a depreciation.[109] This interpretation of

[109] Desai et al. (2008) study crises in e.g. Brazil, Russia, Peru, South Korea and Turkey. These countries tend to have low financial development according to the measures we use.

the findings of Desai et al. ties in with our results at the aggregate level that the elasticity of exports with respect to exchange rates is highly sensitive to financial development.

The model established by Chaney (2005) also connects to our paper in that it highlights the restraint on international trade imposed by liquidity constraint in the face of foreign market entry cost, thereby predicts that a deepening or a widening of the financial markets increases total exports. It also notes that appreciation of the exchange rate, by relaxing the liquidity constraints of some firms, may stimulate exports at the extensive margin and dampen the negative competitiveness impact, which is somewhat related to our third prediction. While both papers examine the relationship between financial development, exchange rate, and international trade, we differ in that our paper investigate the *differential* impacts on trade across countries and industries, depending on financial development and product differentiation, while Chaney models all exports into one differentiated sector (plus a homogeneous good as numeraire) and focuses on *aggregate* exports. Nor did he test his predictions out empirically. Finally, our exchange rate elasticity findings are related to the effect of exchange rates on investment flows in the presence of financial frictions identified by Froot and Stein (1991).

3.3 Theory and Predictions

3.3.1 *Fixed Costs of Trade*

The existence of fixed costs for entering export markets has long been recognized in the literature on international trade. Firm-level evidence of fixed costs is reported by Roberts and Tybout (1997), who find that a firm's current exporting status is largely determined by its previous export experience. Prior exporting experience increases the

probability of exports by up to 60% in their sample of Colombian firms. They infer that sunk costs must play an important role in a firm's decision to export. Similar findings have been reported for other countries (see e.g. Bernard and Wagner (2001) for Germany and Bernard and Jensen (2004) for the United States).[110]

So far, firm-level evidence on the nature of fixed costs is scarce, but several components seem likely. Before exporting can start, a firm must incur the search costs to identify a suitable market and determine if exports are potentially profitable. Once a market is selected, products must be adapted to foreign regulations, conventions (e.g. the voltage of electrical currents) and tastes. This type of fixed cost is addressed by the World Bank Standards and Trade Database (see Otsuki and Wilson, 2004). This is a survey of 689 exporting firms in 17 developing countries across a wide range of industries that investigates the costs imposed on exporting firms by different impediments to trade. The survey indicates that many exporting firms perceive limited access to credit to be a major impediment to business, and that product standards and other regulations also pose barriers to exporting. Most importantly, exporting firms commonly make additional investment, both in compliance costs and in new plant and equipment. These results suggest that regulation-driven fixed costs may be important. Finally, setting up marketing and distribution networks is probably more difficult and expensive than for domestic sales, since cross-border information flows are slower and less reliable and cross-border contracting involves intricacies above domestic contracting.

[110] Several theoretical papers on trade are based on fixed costs. Baldwin (1988), Baldwin and Krugman (1989) and Dixit (1989) suggest that the limited response of import penetration to exchange rates may be due to fixed costs. Melitz (2003) addresses the effect of fixed costs on firm composition in exporting industries.

These examples suggest that fixed up-front costs have long gestation periods, that they are intangible and difficult to observe and that they are firm- or even employee-specific. Hence, these fixed costs are a natural candidate for investments that are difficult to finance externally when finance is underdeveloped. If so, export performance will depend on financial development, especially in industries where exports involve large fixed costs.

It is only reasonable to point out that the relation between financial development and trade has not been much analyzed in the literature. Our theory is perhaps not the only one that might predict a positive relationship with the same direction of causality. Wynne (2005) predicts that wealthier countries will gain an advantage in those sectors where financial imperfections are severe. In his model, high individual wealth relaxes financial constraints for entrepreneurs. Those sectors where credit constraints are most limiting are those where wealth matters most. He assumes that entrepreneurs are richer in countries with high income per capita, and predicts that such countries will export in industries with high credit constraints. In that sense, our cross-country predictions are similar to Wynne's except that where we assume that financial development reduces financing problems for the inception of export business, Wynne assumes personal wealth does so.[111] Wynne does not consider the exchange rate dynamics we study.

3.3.2 *Empirical Proxies for Fixed Costs*

[111] Wynne (2005) also does not consider the role of fixed costs in exacerbating financial constraints. He therefore sorts industries quite differently.

Since we wish to test whether finance is especially important for exports when fixed costs are high, we need proxies for fixed costs. The first proxy, the log of distance between countries, is based on the idea that countries far apart are generally less similar. We assume that fixed costs are higher for trade between countries far apart. The second proxy, a dummy for whether a major language is shared between exporter and importer, captures one particular aspect of similarities between countries, but one that is perhaps important. We assume that fixed costs are lower between countries that share a language. The third proxy is a measure of the official cost of entry for start-up firms to legally start a business in a country, from a data set introduced by Djankov et al. (2002). While Djankov et al. do not specify the geographical scope of operation for such costs to be applicable, Helpman, Melitz and Rubinstein (2008) confirm empirically their relevance for exporting firms, and that "these costs are magnified when both exporting and importing countries impose high regulatory hurdles." We therefore construct the third proxy as the sum of the regulation costs of firm entry - direct cost plus the monetized value of the entrepreneur's time as a fraction of GDP per capita in 1999 - of the importing and exporting countries.[112] We assume that fixed costs are higher between countries with heavier regulations of entry.

We also use two industry-based measures of fixed costs. The first is a variable based on Rauch's (1999) notion of differentiation. Rauch assigns each SITC industry to one of three categories, namely differentiated products (e.g. footwear), goods with reference

[112] Helpman, Melitz and Rubinstein (2008) used indicator variables for high fixed-cost country pairs, defined as those with both the importing and exporting countries having entry costs above the cross-country median. While this satisfies their need for an instrumental variable to meet the exclusion restriction, we stick to the use of a continuous variable here to accommodate more variability.

prices (e.g. Polymerization and Copolymerization Products, SITC 583) and exchange-traded goods (e.g. lead). For the first category, he argues that the "uninformativeness of prices prevents 'globally scanning' traders from substituting for organized exchanges in matching international buyers and sellers". In industries with output of many different varieties and no established prices, it will be more costly to identify and develop export opportunities. The data section describes the details of how a broad industry measure (BEA industries) is calculated from Rauch's narrow industry data (SITC categories).

The final cost proxy represents an attempt to measure the extent to which products have to be modified, altered and tailored for customers. The proxy is constructed as the average fraction of sales devoted to R&D and advertising by U.S. firms in each BEA industry.[113] The proxy is then applied across countries, assuming that the product characteristics indicated by high United States advertising and R&D expenditure are universal, so that the output of these industries requires more adjustment and adaptation no matter which country it come from. We do not assume that measured R&D and advertising activity is similar across all countries.

3.4 Data

In this section, we describe the data used, divided into trade and exchange rate data, industry data, and county data.

[113] The availability of data on advertising and R&D is limited internationally, so the measure is based on U.S. firms in the Compustat database. We checked that the results hold for OECD (2003) data on R&D spending (across member countries) and found that the OECD numbers are highly correlated with the United States-based R&D measure and yield very similar regression results.

3.4.1 *Trade Data and Exchange Rates*

Data on international trade in merchandise is available at the industry level under the SITC-classification system. We use bilateral trade data from Feenstra et al. (2005), which provides data on annual bilateral trade flows from 1963 to 2000 at the industry level. There are more than 170 countries, but we only have financial variables for at most a hundred countries, and for many regressions we have fewer exporters than importers. Part of the analysis in this paper uses total bilateral exports (across all industries), as in standard gravity equations. Other analysis uses data on industry-level bilateral exports. For cross-sectional regressions, we use 1995 as our base year, but have re-run regressions for other years with similar results.[114] For panel regressions, the whole data set is used, but because we calculate export growth rates, that panel data stretches from 1964 to 2000. Trade growth is aggregated across various industries, either all differentiated SITC codes (using the Rauch (1999) measure), or all undifferentiated SITC codes (for comparison) or using BEA industries with above-median advertising plus R&D. We exclude observations with trade value lower than $1,000 in order to avoid identifying off tiny export volumes. We also exclude observations with trade growth (the dependent variable) or lagged trade growth rate or exchange rate changes outside the [-1,1] range in order to avoid identifying off outliers.

Bilateral exchange rates are taken from International Financial Statistics (IMF, 2009). We use controls commonly used in estimation of trade regressions. For each country-pair, we use a dummy variable equal to one if they share a land border and a dummy equal to

[114] The annual cross-sections are very similar to each other, so a panel specification would not add much to the power of our cross-sectional tests.

one if they share an official language. We also use distance in kilometers, as measured between largest cities, reported in Fitzpatrick and Modlin (1986). From Rose (2005) we get dummy variables for membership of a regional trade agreement (these are the EU/EEC, United States-Israel FTA, NAFTA, CARICOM, PATCRA, ANZCERTA, CACM, Mercosur, ASEAN and SPRTC), and data on colonial history: we use a dummy equal to one if exporter and importer were either involved in a colonial relationship or had the same colonizer.

3.4.2 *Industry Data*

Our measure of differentiation is based on Rauch's (1999) classification of products. Rauch categorizes SITC industries based on whether the output is exchange-traded, reference-priced, or differentiated. Of Rauch's three categories, only firms that produce differentiated products are likely to face significant up-front costs in adapting their product for export. We define a dummy variable equal to one if a product in an SITC code is differentiated and zero otherwise. We then calculate average differentiation by BEA industry.

Our advertising and R&D variable is based on firms in the Compustat database, and represents average advertising and R&D expenditure by BEA industry (all firms are from the United States).

Table 3.1 reports summary statistics for our proxies of up-front investment costs for the whole sample, including both differentiation and advertising-R&D, as well as the measures based on exporter-importer pair. To provide some intuition of differentiation, Appendix Table 3.6 presents the fraction of world trade in each of the 34 BEA industry

Table 3.1 Summary Statistics

	Min	10th percentile	Median	Mean	90th percentile	Max	Std dev
Distance (log)	6.19	7.16	8.96	8.76	9.57	9.89	0.78
Common Language	0	0	0	0.11	1	1	0.31
Regulation costs	0.0342	0.4012	0.9072	1.3732	3.3628	8.4189	1.2353
Advertising and R&D to Sales	0.002	0.008	0.027	0.040	0.071	0.17	0.037
Differentiation	0	0.10	0.61	0.57	0.96	1	0.29
Exchange rate (importer's appreciation)	-0.9991	-0.2328	-0.0021	-0.0652	0.0542	0.4225	0.1632
Export growth	-0.9998	-0.4068	0.0804	0.0674	0.5182	0.9998	0.3620
Export growth: differentiated trade	-0.9999	-0.4047	0.0947	0.0781	0.5311	0.9998	0.3674
Export growth: undifferentiated trade	-0.9998	-0.4829	0.0576	0.0447	0.5453	1.000	0.3918
Export growth: high advertising and R&D industries	-1.0000	-0.4038	0.0930	0.0789	0.5412	1.0000	0.3701

Notes: Summary statistics of key variables. All statistics are calculated over the range of data most relevant: Export volume is log of one plus 1995 bilateral export volume, across all exporter-importer pairs; Distance, Common Language, and Regulation costs are also across country pairs; Accounting standards is across exporting countries. These numbers refer to the sub-sample of exporting countries with accounting data. Advertising and R&D is across industries, based on U.S. firms in Compustat. Differentiation is the average share of bilateral exports that are differentiated, based on Rauch (1999), across country pairs. See the text for details.

classifications that is differentiated, as well as the advertising-R&D measure. The correlation between the measures across industries is 0.27.

We use several measures from Chor (2010): industry human capital intensity, physical capital intensity, and external dependence (the variable was initially constructed by Rajan and Zingales, 1998). We also use the input-use concentration in each industry (originally from Levchenko, 2007) and the share of inputs that are relationship specific (originally from Nunn, 2007), a measure of sales volatility (originally from Cunat and Melitz, 2007) and a measure of job complexity in an industry (originally from Costinot,

2007). These variables are potentially important influences on trade composition, which may be important to control for.

3.4.3 *Country Data*

For each exporting and each importing country in our sample, we need to measure financial development, and controls such as GDP, area and population. We use GDP and population numbers from the Summers-Heston data set. Country size (in million acres) comes from *The Universal Almanac 1997.* Annual real exchange rates are from the IMF's International Financial Statistics (IMF, 2009). These are CPI-based traded weighted averages for each country. Bilateral real exchange rates are collected from the World Bank's World Development Finance and Global Development Indicators.

There is no consensus on how to measure financial development across countries. The ideal measure should capture the availability of financing for deserving projects regardless of risk, ownership, timing, and information issues. As our first variable, we use accounting standards, a measure of the quality of accounting in a country. The Center for International Financial Analysis and Research (CIFAR) created an index for different countries by rating the annual reports of at least three firms in every country on the inclusion or omission of 90 items. Comprehensive data on the measure dates to 1990, and is discussed in Rajan and Zingales (1998). The measure is not available for later years, but does not change much between 1980 and 1990 according to Rajan and Zingales (1998), so we use the 1990 numbers. An alternative to accounting standards is some measure of the stock of actual financing. We use the ratio of credit to the private sector to

Table 3.2 Summary Statistics for Selected Variables and Observations

Exporting country	Accounting Standards	Private Credit to GDP	Exporting country	Accounting Standards	Private Credit to GDP
Argentina	45	0.18	Mexico	60	0.34
Australia	75	0.71	Netherlands	64	1.6
Austria	54	0.93	New Zealand	70	0.85
Belgium	61	0.64	Nigeria	59	0.07
Brazil	54	0.32	Norway	74	0.83
Canada	74	0.79	Peru	38	0.12
Chile	52	0.62	Philippines	65	0.38
Colombia	50	0.35	Portugal	36	0.57
Denmark	62	0.3	Singapore	78	1.02
Egypt	24	0.32	South Africa	70	1.33
Finland	77	0.64	Spain	64	0.71
France	69	0.84	Sweden	83	1.2
Germany	62	1.02	Switzerland	68	2.17
Greece	55	0.3	Taiwan	65	1.55
Hong Kong	69	1.48	Thailand	64	1.25
India	57	0.23	Turkey	51	0.12
Israel	64	0.64	United Kingdom	78	1.12
Italy	62	0.52	Uruguay	31	0.23
Japan	65	2.07	USA	71	1.59
South Korea	62	1.21	Venezuela	40	0.09
Malaysia	76	1.13			
Average	**60.9**	**0.79**			

Notes: Summary statistics of financial development variables for all exporting countries which have data for accounting standards. Accounting standards are the fraction of 90 elements reported in annual reports in each country. Private credit refers to all credit given by banks and other financial institutions to private sector borrowers in 1995.

credit will likely reflect demand for financing as well as its supply.[115] Hence private credit may be more subject to reverse causality problems than accounting standards.[116] It

[115] We have also used the ratio of stock market capitalization to GDP. Stock market capitalization shares the same strengths and weakness as the private credit measure, and since results tend to be similar, we do not report any regression with this variable.

[116] Accounting standards may develop in response to future financing needs, which can generate similar problems in an indirect manner.

may also reflect the business cycle. The advantage of accounting standards is that it proxies for the amount of external financing available, rather than the amount actually given. However, data is available more widely for private credit than for accounting standards. We therefore report results for some of the key specifications using both measures to establish robustness.

For comparative advantage controls, we use measures of the abundance of human and physical capital, taken from Chor (2010). We also use measures of the strength of legal system (Gwartney and Lawson, 2004)[117] and a measure of labor regulation flexibility (Cunat and Melitz, 2007).[118]

We use logged distance, a dummy for common language, and the regulation of entry costs as proxies for fixed costs for exporters. Summary statistics for these key variables are reported in Table 3.1, together with the industry-based cost proxies.

3.5 Cross-Sectional Results

We use the gravity equation for international trade (see e.g. Linneman, 1966; Frankel, 1997) as our empirical starting point for examining the effect of financial development on trade. Because financial development may be affected by many features of a country (e.g., corruption, education, political system) which can determine trade through other channels, the direct estimation of a gravity equation with financial development would not identify

[117] The measure refers to 1985.

[118] Davin Chor has been most helpful in sharing institutional and comparative advantage controls from a multitude of sources.

the causal effect of financial development on trade.[119] We therefore include fixed effects for exporting and importing countries, and examine predictions about the relative effect of financial development for different types of international trade.

3.5.1 *Fixed Effect Regressions with Bilateral Data*

We first compare country-pairs where fixed costs are likely to be low and high. We predict that financial development will matter more for the latter. This is essentially a difference-in-difference test. We first run OLS regressions of the form

$$\log[1+E_{ij}] = \{EXP_i\} + \{IMP_j\} + b\, D_{ij} + g\, F_i \times COST_{ij} + e_{ij} \qquad (3.1)$$

where F_i is accounting standards or 1995 private credit to GDP in country i, the terms in brackets are two sets of country fixed effects, D_{ij} is a vector of distance measures between i and j (these are physical distance along the earth's surface, an indicator for shared border, and one for shared language, a regional trading area indicator, an indicator for shared colonial history, and regulation of entry costs)[120] and $COST_{ij}$ is a proxy for fixed costs between countries i and j (our proxies for $COST_{ij}$ include three distance measures which are elements of D_{ij}). All exporter- and importer-specific measures are dropped in these specifications since they are absorbed by the country fixed effects. These regressions test the prediction that finance should be more important for exporter-

[119] In such regressions, the estimated effect of finance is large and positive, for both measures of financial development (private credit, accounting standards). This is at best suggestive of the role of financial development for international trade.

[120] Note that when distance or the common language indicators is used as the cost proxy, regulation costs is not included as a control variable, because this would considerably reduce the sample size. Regressions with it included a control (not reported) however generate results that are comparable in magnitude and significance to those reported in Table 3.3 for both OLS and GMM specifications.

importer pairs when fixed costs are high. For each proxy of fixed costs, we expect the interaction with finance to enter in the following way: if the proxy is associated with higher dependence on external finance, i.e. large sunk costs (distance and regulation costs), we predict a positive sign (high costs imply more need for finance). For the proxy that predicts low sunk costs (common language), we predict a negative sign.

In Table 3.3, Panel A reports results with accounting standards as the measure of financial development, and Panel B reports results using private credit. For each panel, we use three proxies for fixed costs: distance, the common language dummy, and regulation costs. Panel A, column 1 reports the results using distance as the proxy for up-front costs. The interaction coefficient is positive and statistically significant at the 1% level. The coefficient estimate implies that distance has a more modest negative effect on trade volumes for exporters with well developed accounting standards, consistent with a role for financial development in allowing firms to raise funding if such funding needs are increasing in distance (as we hypothesized that fixed costs are). The next coefficient on distance for a country at the 25th percentile of accounting standards (Austria and Brazil, 54) is -1.43, whereas at the 75th percentile (New Zealand and South Africa, 70), the coefficient is just -0.96. In other words, going from the 25th to the 75th percentile of financial development reduces the coefficient on distance by 33%. Going from the 5th to the 95th percentile implies a 76% drop in the coefficient. These results are consistent with the fixed cost argument, but also with a few alternative explanations. For example, poor financial development countries might export types of goods that have low transport costs.

The next fixed cost proxy, a dummy for having a common main language between exporter and importer, is distinct from physical distance, so does not admit the same

Table 3.3 Bilateral Exports and Finance: Interactions

Panel A: Accounting standards

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Log of bilateral exports					
Type of regression	OLS	GMM	OLS	GMM	OLS	GMM
Cost proxy	Distance		Language		Regulation	
Accounting stds ×	**0.029*** **	**0.015*** **	**-0.043*** **	**-0.028*** **	**0.003**	**0.004**
Cost Proxy	(0.003)	(0.002)	(0.006)	(0.005)	(0.003)	(0.004)
Controls	Distance, Border, Language, Regional Trading Area, Colonial history					
Regulation as control	-	-	-	-	Yes	Yes
Fixed Effects	Exporter, Importer					
R^2	0.810	n/a	0.808	n/a	0.818	n/a
N	4,920	4,920	4,920	4,920	2,557	2,557

Panel B: Private Credit

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Log of bilateral exports					
Type of regression	OLS	GMM	OLS	GMM	OLS	GMM
Cost proxy	Distance		Language		Regulation	
Private credit ×	**0.771*** **	**0.474*** **	**-0.650*** **	**-0.474*** **	**0.320*** **	**0.313*** **
Cost Proxy	(0.111)	(0.123)	(0.123)	(0.123)	(0.065)	(0.064)
Controls	Distance, Border, Language, Regional Trading Area, Colonial history					
Regulation as control	-	-	-	-	Yes	Yes
Fixed Effects	Exporter, Importer					
R^2	0.802	n/a	0.801	n/a	0.8143	n/a
N	11,070	11,070	11,070	11,070	3,837	3,837

Notes: OLS and GMM regressions of the log of total bilateral 1995 exports on interactions of financial development and a proxy for sunk costs. The cost proxy is listed at the top of each column. Financial development is measured by Accounting Standards (Panel A) and Private Credit over GDP (Panel B). These regressions include fixed effects for exporting country and importing country, and bilateral controls. Distance is the log of distance between capitals. Language is a dummy equal to one if a major language is common to exporter and importer, Regulation is the sum of the regulation costs of firm entry - direct cost plus the monetized value of the entrepreneur's time as a fraction of GDP per capita in 1999 - of the importing and exporting countries. Regressions include exporter-importer pair fixed effects and have robust standard errors, clustered by exporter, which are reported under the coefficients. Note that Regulations is not included as a control variable when Distance and Language are the cost proxy, because this would considerably reduce the sample size. A star (*) denotes a significant coefficient at the 10% level, two stars denotes significance at the 5% level, and three stars at the 1% level.

alternative explanation. In column 3, results using the common language proxy are reported. Having a common language with an importing country decreases the importance of finance significantly, as predicted. The coefficient is negative and significant (the dummy for having a common language has a positive coefficient). Going from the 25th to the 75th percentile of accounting standards reduces the predicted effect of a common language by 52%. In other words, financial development has a stronger predictive effect for trade far away and to linguistically different countries, consistent with the idea that fixed costs associated with such distances are particularly problematic for (potential) exporters in countries with poor financial development. Column 5 reports OLS regression results by using regulation costs of entry as the fixed cost proxy. The coefficient is insignificant under this specification, yet has the predicted sign. This implies that financial development plays a more important role in promoting bilateral trade when the importing and/or exporting countries impose heavy regulatory requirements on entrepreneurial activities - including entry into a foreign market. Going from the 25th to the 75th percentile of accounting standards reduces the predicted effect of regulation costs by 1%.

In panel B, similar tests are performed with private credit instead of accounting standards as the measure of financial development. The sample size approximately doubles (because data on private credit is more widely available). The distance and language results have kept their significance at the 1% level, and the coefficient with regulation costs as the proxy is also strongly significant. The estimated magnitudes are comparable: the effect of going from the 25th percentile (0.32, Brazil and Egypt) to the 75th (1.13, Malaysia and close to the US) is predicted to reduce the effect of financial

distance by 42%, and that of a common language and regulation costs by 39% and 0.3% respectively.

We also run GMM estimations on the same set of variables, with results juxtaposed with the corresponding OLS regressions in Table 3.3. The results are comparable to OLS estimates in significance. When it comes to magnitude, the GMM estimates are smaller but generally comparable when distance and language are used as the fixed cost proxy, and almost identical when regulation costs is used. This confirms that financial development is an important determinant of international trade flows and the effect is more pronounced for country pairs where the hurdles for exporting are high. For both OLS and GMM specifications, we have also tried using regulation of entry costs of the importing or exporting country respectively as the proxy, which generates coefficient estimates with comparable or greater significance.

These results suggest that finance matters specifically for certain types of trade. It is natural to also consider the possibility of industry-variation, since industry patterns in trade is an important and long-standing topic in the trade literature. We next turn to industry-level data, using proxies for fixed costs that vary by industry.

3.5.2 *Fixed Effect Regressions with Industry-Level Data*

The next two proxies for fixed costs vary by industry and not country pair. Hence, we disaggregate the trade data and each observation is now exports from country i to country j in industry k. We estimate this using OLS and also with simulated method of moments (SMM) estimation (Pakes and Pollard, 1989). The SMM method allows estimation which includes zero-trade data, i.e. industry-exporter-importer observations

with no trade in 1995 (the year we use to estimate this regression). The SMM method searches numerically across a large space for parameter values that deliver predicted trade flows matching moments in the trade data. The model uses random draws for prices (which are unobserved) corresponding to trade volumes, which can be used to match data. For a detailed description, see Chor (2010). We also follow computation shortcuts used by Chor, in order to reduce the dimensionality of the problem. We use a Newtonian search algorithm to get numerical parameter estimates which include the coefficient on each variable. These coefficients have interpretations similar to OLS gravity equation coefficients. We present OLS and SMM results next to each other in Table 3.4.[121] Our estimation includes a number of comparative advantage controls, including both human and physical capital (we multiply industry factor intensity with exporting country factor abundance). We also control for a number of institutional factors: external dependence times financial development, the input-use concentration in each industry times a measure of the strength of legal system, industry job complexity times legal strength and times human capital abundance, the share of relationship-specific inputs times legal strength, and industry sales volatility times labor regulation flexibility (see the data section for sources and definitions). These variables constitute a very broad set of comparative advantage controls.

In Table 3.4, we present results for the two fixed cost proxies that vary by industry: product differentiation and advertising plus R&D. In Panel A, financial development is measured with accounting standards. Columns 1 and 2 present regression results for

[121] We did not estimate bilateral regressions (as in Table 3.3) using SMM, since there are many fewer zeros in aggregate bilateral trade, suggesting that OLS is less problematic.

Table 3.4 Bilateral Industry Exports and Finance: Interactions

Panel A: Accounting standards

	(1)	(2)	(3)	(4)
Dependent variable	Log of exports by industry			
Type of regression	OLS	SMM	OLS	SMM
Fixed cost proxy	Differentiation		Advertising plus R&D	
Accounting standards × Fixed cost proxy	**0.019****	**0.045*****	**0.169*****	**0.251*****
	(0.009)	(0.001)	(0.052)	(0.009)
General controls	Yes	Yes	Yes	Yes
Comparative advantage controls	Yes	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes	Yes
Fixed Effects	Exporter-Importer and Importer-Industry			
R^2	0.504	n/a	0.497	n/a
N	97,013	97,013	100,096	100,096

Panel B: Private Credit

	(1)	(2)	(3)	(4)
Dependent variable	Log of exports by industry			
Type of regression	OLS	SMM	OLS	SMM
Fixed cost proxy	Differentiation		Advertising plus R&D	
Private credit × Fixed cost proxy	**0.887*****	**0.996*****	**2.667***	**3.142*****
	(0.307)	(0.010)	(1.397)	(0.161)
General controls	Yes	Yes	Yes	Yes
Comparative advantage controls	Yes	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes	Yes
Fixed Effects	Exporter-Importer and Importer-Industry			
R^2	0.534	n/a	0.525	n/a
N	97,013	97,013	100,096	100,096

Notes: OLS and SMM regressions of 1995 bilateral exports on interactions of financial development and a proxy for sunk costs. The cost proxy is listed at the top of each column. Financial development is measured by Accounting Standards (Panel A) and Private Credit over GDP (Panel B). Observations are bilateral exports in each SIC industry. Differentiation is the average share of worldwide exports in the industry that are differentiated according to Rauch (1999). Advertising and R&D is the fraction of sales devoted to advertising and research and development by U.S. firms in the industry, based on Compustat data. General controls are Distance, Border, Language, Regional Trading Area, Colonial history. Comparative advantage controls are industry human or physical capital intensity times the factor abundance in the exporting country. Institutional controls are external dependence times financial development, the

input-use concentration in each industry times a measure of the strength of legal system, industry complexity times legal strength and times human capital abundance, share of relationship-specific inputs times legal strength and industry sales volatility times labor regulation flexibility. OLS regressions include exporter-importer pair fixed effects and industry fixed effects, and have robust standard errors, clustered by exporter, which are reported under the coefficients. SMM regressions are estimated with simulations. Standard errors are reported under coefficients. A star (*) denotes a significant coefficient at the 10% level, two stars denotes significance at the 5% level, and three stars at the 1% level.

differentiation using OLS and SMM, respectively. The coefficient on the interaction of accounting standards and differentiation is positive and significant in both regressions - finance has a larger positive effect on exports in industries with differentiated output than in those with undifferentiated output. The magnitude is fairly large: an increase of differentiation from zero to one (the full range) increases the effective slope on accounting standards by 0.019, which can be compared to the average effect of accounting standards on total bilateral trade, estimated to be 0.032 (unreported regressions). In other words, the effect of financial development is much stronger, both economically and statistically, for differentiated goods. The SMM coefficient estimate is slightly larger and more significant. Results for the other cost proxy, advertising plus R&D expenses, are reported in columns 3 and 4. Again, the OLS coefficient is positive and significant, and the SMM coefficient is also significant and somewhat larger. Financial development has a larger positive effect on exports in industries that tend to use more advertising and R&D. In Panel B, financial development is measured by private credit. Results are similar: financial development interacts significantly with the fixed cost proxies.

The industry-level tests with cost proxies provide results consistent with our predictions about fixed costs and the role of financial development. Taken together with the bilateral findings (using distance, the language dummy, and regulation costs as

proxies for fixed costs) this provides broad evidence of the importance of financial development for trade.[122] This is a finding about the composition of international trade. Controlling for comparative advantage, countries with better financial development tend to export more of complex products, advertising and R&D-heavy products, and to destinations far away and to countries which do not share a common language. This is suggestive evidence that the financial sector is important for exports that require incurring large up-front development costs. Our findings support the idea that fixed costs are a part of the explanation for the trade-finance link. Finance facilitates exports, and does so specifically for certain types of goods. We next consider the effect of importing countries' financial development.

3.6 Trade Dynamics and the Exchange Rate Elasticity of Exports

If financial development matters for export performance in a static sense, it is natural to ask if it affects trade dynamics as well. The presence of fixed costs will deter entry and therefore indirectly exit from export markets.[123] This will be particularly severe if financial development is low, so that entry is largely restricted to firms with internal financing (making exit even less attractive).

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[122] Beck (2003) and Svaleryd and Vlachos (2005) find that there are compositional effects in trade due to financial development. They consider somewhat different industry rankings (for example, Beck uses manufacturing vs. non-manufacturing) and do not consider how the geographical patterns of trade are affected by financial development.

[123] See especially Melitz (2003). The findings on exchange rates in this section are related to theories on how firms respond to changes in exchange rates when there are fixed costs, developed by Baldwin and Krugman (1989) and Dixit (1989).

3.6.1 *Export Elasticities: Empirical Findings*

If setting up trade requires incurring fixed costs (which some exporters need to finance externally), changing the allocation of export volumes across importing countries will sometimes force exporters to incur such fixed costs. We therefore ask the question: how does a country allocate its exports across different importing countries as exchange rates change? We test if this response is affected by financial development using a sample of all countries for which there is data on accounting standards and for which we have annual bilateral trade data. We analyze the growth of bilateral exports, either total for all industries, or total for only those industries with high fixed exporting costs.[124] We define high fixed costs either as exports in differentiated industries where advertising and R&D costs are above the median (0.027). The independent variable of interest is the real exchange rate and its interaction with accounting standards. The period covered by the data is 1963 to 2000, but since we use growth rates, the panel starts in 1964. We regress export growth on exporter-year fixed effects, importer fixed effects, lagged export growth, appreciation of the importer's currency and the interaction of accounting standards and the importer's appreciation. Because these regressions include exporter-year fixed effects, there is no concern about reverse causality between export performance and exchange rates. In other words, we take out the average trade growth in an industry-exporter-importer unit, and test if the allocation of exports across importers responds more to (lagged) relative exchange rate movements for countries with better accounting standards.

[124] Aggregating exports reduces the volatility of individual time series, and therefore helps estimation despite reducing the number of observations. Running regressions by BEA individual industry requires a large set of further fixed effects and produces similar results. Finer industries is unwieldy from a computational point of view.

If the Japanese Yen rises relative to the British Pound, exporters will see a higher relative growth of exports to Japan than to Britain on average. We estimate this effect (i.e. the coefficient on importer's exchange rate with exporter-year fixed effects) and allow it to vary with financial development.

Table 3.5 reports results from the basic regression and several variations. In column 1, total export growth is regressed on controls. The total *R*-squared is 0.119, indicating growth unexplained.[125] The coefficient on lagged export growth is negative (-0.12) and highly significant, suggesting some mean reversion. The coefficient on the exchange rate is 0.024 and significant at the 5% level, suggesting that a one standard deviation increase in the lagged exchange rate of one importing country (16%) leads to a 0.4% increase in exports to that country (relative to exports to other countries). Because the interaction with accounting standards is defined around typical accounting standards (value 60), this is the effect of exchange rates for a typical country (Mexico's accounting standards are measured as 60). For a country with accounting standards that are ten points higher (South Africa has accounting standards of 70), the predicted response to the same relative appreciation of an importer's currency is a 0.8% relative increase in exports.

This result is larger if we restrict attention to exports of differentiated products. In column 2, a similar regression using only such exports is reported, and the estimated interaction coefficient is larger by about half. In column 3, we return to aggregate bilateral export growth and include importer-year fixed effects (these absorb the lagged appreciation, so the coefficient for that is not identified), with very small changes to the

[125] If we weight regressions by export value, the R-squared is about 0.3, suggesting that some of the observations for bilateral trade with very small export volumes generate extra noise. All our results hold using value-weighted regressions.

Table 3.5 The Exchange Rate Response of Exports

	(1)	(2)	(3)	(4)
Dependent variable	Export growth	Growth of differentiated exports	Export growth	Growth of high advertising and R&D exports
Type	OLS	OLS	OLS	OLS
Importer's appreciation (t-1) × accounting standards	**0.0023****	**0.0036****	**0.0022****	**0.0024****
	(0.0004)	(0.0014)	(0.0007)	(0.0007)
Importer's appreciation vs. United States $ (t-1)	**0.0242****	**0.0042***	-	**0.0209**$^{+}$**
	(0.0108)	(0.0025)		(0.0135)
Export growth (t-1)	**-0.1212****	**-0.1692*****	**-0.1510****	**-0.1117****
	(0.0077)	(0.0158)	(0.0101)	(0.0105)
Exporter × year fixed effects	Yes	Yes	Yes	Yes
Importer fixed effects	Yes	Yes	-	Yes
Importer × year fixed effects	-	-	Yes	-
N of error clusters	33	33	33	33
R^2	0.120	0.106	0.219	0.087
N	59,867	47,490	59,867	43,633

Notes: Regressions of annual export growth rates 1964-2000 on lagged exchange rate changes, financial development and other controls. Financial development is measured by Accounting Standards. In columns 1 and 3, the dependent variable is bilateral export growth from an exporting country to an importing country in a year. In column 2 the dependent variable is the growth of differentiated exports. In column 4, the dependent variable is the growth of exports in those industries that have above average R&D and advertising expenditures. Exporting countries are all countries for which export data and accounting standards data are available. "Importer's appreciation (*t-i*) × accounting standards" refers to the exchange rate change lagged *i* periods interacted with demeaned accounting standards. Observations are excluded if trade growth is outside the [-1,1] interval or if the value of trade is below one million current US dollars. Standard errors clustered by exporter are reported under the coefficients. A star (*) denotes a significant coefficient at the 10% level, two stars denotes significance at the 5% level, and three stars at the 1% level.

estimated coefficients of column 1. Finally, in column 4, we produce results for exports in BEA industries with above-median values for the fixed cost proxy based on advertising and R&D expenses. The results are very similar to aggregate results (unlike differentiated exports). The interaction of importer's exchange rates and exporter's accounting standards is still highly significant. The reported results are also robust to a

number of further variations (not reported). For example, we have tried restricting the sample to OECD importers (which tend to have larger volumes, so less volatile changes). We have also experimented with including various lags of export growth and exchange rate changes. Including one, two or three lags of the exchange rate, or two or three lags of lagged export growth, does not materially affect the coefficient of interest. We have also used real exchange rates, again with no material change.[126]

Exports respond more to changes in export opportunities in countries with better financial systems, as captured by exchange movements. The allocation of exports across different importers is more responsive to exchange rates when financial development is higher. This effect is particularly strong for trade in differentiated goods. This result is consistent with the prediction of the fixed cost argument: whether exporters switch between different importers or start from scratch, responding to relative exchange rate shifts requires incurring fixed costs, which some firms will need to finance externally. Hence, financial development facilitates the process of allocating trade across importers.

3.7 Conclusion

The main idea of this paper is very simple. We propose that exports require significant up-front investments that are difficult to finance externally, and that the higher the up-front costs, the more important it becomes to have a well-developed financial system to finance them. We examine three predictions: high financial development

[126] Measuring the real exchange rate requires a price index. Using real exchange rates using GDP deflators from the Penn World Table, results were similar.

increases exports, especially when there are large fixed costs, and increases the elasticity of exports to exchange rate changes.

The empirical tests focus on the latter two predictions, which are less obvious and also less studied. First, for five different measures of up-front costs - importer-exporter distance, an importer-exporter common language dummy, regulation of entry costs, the degree of output differentiation in an industry, and the extent of R&D and advertising in an industry - we find that finance matters more for exports when up-front costs are high. These results suggest that financial development is an important determinant of export performance, especially when target markets are dissimilar from the exporting country, when the importing or exporting country forces entrants into a new geographic market to incur heavy regulatory burdens, or when exported products have many varieties or involve large R&D and advertising expenditure. The cost proxies using industry variation are potentially subject to comparative advantage concerns. We test this by including comparative advantage controls, and find that our findings are robust.

Second, we examine exchange rate elasticities of exports. If exports are facilitated by financial development, the response of export volumes to exchange rate changes is likely to also reflect financial development. Indeed, we find that the allocation of exports across importers is more responsive to importers' relative exchange rate changes for countries with high financial development. This effect is particularly strong for differentiated products. The advantage of tests using exporter*year fixed effects is that they difference out any exporter-level shocks, thereby reduces the econometric concerns related to exchange rate endogeneity. Our exchange rate elasticity findings suggest considerable

welfare gains from financial development (because countries with higher financial development will tend to export more to the importers with high nominal exchange rates).

Our findings constitute a new type of evidence of the real effects of finance, suggesting that international trade is heavily affected by financial development. Also, trade may be an important channel through which financial development affects economic performance.

Appendix Table 3.6 Industry List

Industry (BEA classification)			
BEA	Industry name	Differentiation	Advertising and R&D ratio
1	Grain, Mill and Bakery Products	n/a	0.06
2	Beverages	0.14	0.05
3	Tobacco Products	0	0.03
4	Other Food and Kindred Products	0.04	0.04
5	Apparel and Other Textile Products	0.83	0.01
6	Leather and Leather Products	1.0	0.01
7	Pulp, Paper, and Board Mills	0.01	0.01
8	Other Paper and Allied Products	0.55	0.02
9	Printing and Publishing	1.0	0.02
10	Drugs	0.94	0.17
11	Soaps, Cleaners, and Toilet Goods	1.0	0.14
12	Agricultural Chemicals	0	0.00
13	Industrial Chemicals and Synthetics	0.12	0.05
14	Other Chemicals	0.70	0.06
15	Rubber Products	1.0	0.06
16	Miscellaneous Plastic Products	1.0	0.02
17	Primary Metal Industries, Ferrous	0.27	0.01
18	Primary Metal Industries, Nonferrous	0.20	0.00
19	Fabricated Metal Products	1.0	0.02
20	Farm and Garden Machinery	1.0	0.01
21	Construction, Mining, etc	1.0	0.02
22	Computer and Office Equipment	1.0	0.10
23	Other Nonelectric Machinery	1.0	0.02
24	Household Appliances	1.0	0.05
25	Household Audio and Video, etc.	1.0	0.07
26	Electronic Components	n/a	0.06
27	Other Electrical Machinery	1.0	0.05
28	Motor Vehicles and Equipment	1.0	0.04
29	Other Transportation Equipment	1.0	0.03
30	Lumber, Wood, Furniture, etc.	0.74	0.01
31	Glass Products	n/a	0.01
32	Stone, Clay, Concrete, Gypsum, etc.	0.89	0.01
33	Instruments and Apparatus	1.0	0.06
34	Other Manufacturing	0.63	0.06

Notes: These are the 34 industries according to the BEA classification (not used in regressions). Differentiation is a measure of how differentiated or homogenous an industry's output is, based on Rauch (1999). Advertising-R&D ratio is the fraction of sales devoted to advertising and research and development by U.S. firms in the industry. See the text for details.

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