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A Comprehensive Analysis of Poverty in India

Arvind Panagariya and Megha Mukim*

This paper offers a comprehensive analysis of poverty in India. It shows that regardless of which of the two official poverty lines we use, we see a steady decline in poverty in all states and for all social and religious groups. Accelerated growth between fiscal years 2004–2005 and 2009–2010 also led to an accelerated decline in poverty rates. Moreover, the decline in poverty rates during these years has been sharper for the socially disadvantaged groups relative to upper caste groups so that we now observe a narrowing of the gap in the poverty rates between the two sets of social groups. The paper also provides a discussion of the recent controversies in India regarding the choice of poverty lines.

Keywords: poverty, caste, religious groups, economic growth, India
JEL codes: D30, I32

I. Introduction

This paper provides comprehensive up-to-date estimates of poverty by social and religious groups in the rural and urban areas of the largest 17 states in India. The specific measure of poverty reported in the paper is the poverty rate or headcount ratio (HCR), which is the proportion of the population with expenditure or income below a pre-specified level referred to as the poverty line. In the context of most developing countries, the poverty line usually relates to a pre-specified basket of goods presumed to be necessary for above-subsistence existence.

In so far as prices vary across states and between rural and urban regions within the same state, the poverty line also varies in nominal rupees across states and between urban and rural regions within the same state.[1] Similarly, since prices rise over time due to inflation, the poverty line in nominal rupees in a given location is also adjusted upwards over time.

The original official poverty estimates in India, provided by the Planning Commission, were based on the Lakdawala poverty lines, so named after Professor D. T. Lakdawala who headed a 1993 expert group that recommended these lines.

*Arvind Panagariya is Professor at Columbia University and Megha Mukim is an Economist at the World Bank. The views expressed in the paper are those of the authors and not of the World Bank. We thank an anonymous referee, P. V. Srinivasan, and participants of the first 2013 *Asian Development Review* conference held on 25–26 March 2013 at the Asian Development Bank headquarters in Manila, Philippines.

[1] Prices could vary not just between urban and rural regions within a state but also across subregions within rural and subregions within urban regions of a state. Therefore, in principle, we could envision many different poverty lines within rural and within urban regions in each state. To keep the analysis manageable, we do not make such finer distinctions in the paper.

Recommendations of a 2009 expert committee headed by Professor Suresh Tendulkar led to an upward adjustment in the rural poverty line relative to its Lakdawala counterpart. Therefore, while the official estimates for earlier years were based on the lines and methodology recommended by the expert group headed by Lakdawala, those for more recent years were based on the line and methodology recommended by the Tendulkar Committee. Official estimates based on both methodologies exist for only two years, 1993–1994 and 2004–2005. These estimates are provided for the overall population, for rural and urban regions of each state, and for the country as a whole. The Planning Commission does not provide estimates by social or religious groups.

In this paper, we provide estimates using Lakdawala and Tendulkar lines for different social and religious groups in rural and urban areas in all major states and at the national level. Our estimates based on Lakdawala lines are computed for all years beginning in 1983 for which large or "thick" expenditure surveys have been conducted. Estimates based on the Tendulkar line and methodology are provided for the three latest large expenditure surveys, 1993–1994, 2004–2005, and 2009–2010.

Our objective in writing the paper is twofold. First, much confusion has arisen in the policy debates in India around certain issues regarding poverty in the country—for instance, whether or not growth has helped the poor (if yes, how much and over which time period) and whether growth is leaving certain social or religious groups behind. We hope that by providing poverty estimates for various time periods, social groups, religious groups, states, and urban and rural areas, this paper will help ensure that future policy debates are based on fact. Second, researchers interested in explaining how various policy measures impact poverty might find it useful to have the poverty lines and the associated poverty estimates for various social and religious groups and across India's largest states in rural and urban areas readily available in one place.

The literature on poverty in India is vast and many of the contributions or references to the contributions can be found in Srinivasan and Bardhan (1974, 1988), Fields (1980), Tendulkar (1998), Deaton and Drèze (2002), Bhalla (2002), and Deaton and Kozel (2005). Panagariya (2008) provides a comprehensive treatment of the subject until the mid-2000s including the debates on whether or not poverty had declined in the post-reform era and whether or not reforms had been behind the acceleration in growth rates and the decline in poverty. Finally, several of the contributions in Bhagwati and Panagariya (2012a, 2012b) analyze various aspects of poverty in India using the expenditures surveys up to 2004–2005. In particular, Cain, Hasan, and Mitra (2012) study the impact of openness on poverty; Mukim and Panagariya (2012) document the decline in poverty across social groups; Dehejia and Panagariya (2012) provide evidence on the growth in entrepreneurship in services sectors among the socially disadvantaged groups; and Hnatkovska and Lahiri (2012) provide evidence on and reasons for narrowing wage inequality between the socially disadvantaged groups and the upper castes.

To our knowledge, this is the first paper to systematically and comprehensively exploit the expenditure survey conducted in 2009–2010. This is important because growth was 2–3 percentage points higher between 2004–2005 and 2009–2010 surveys than between any other prior surveys. As such, we are able to study the differential impact accelerated growth has had on poverty alleviation both directly, through improved employment and wage prospects for the poor, and indirectly, through the large-scale redistribution program known as the National Rural Employment Guarantee Scheme, which enhanced revenues made possible. In addition, ours is also the first paper to comprehensively analyze poverty across religious groups. In studying the progress in combating poverty across social groups, the paper complements our previous work, Mukim and Panagariya (2012).

The paper is organized as follows. In Section II, we discuss the history and design of the expenditure surveys conducted by the National Sample Survey Office (NSSO), which form the backbone of all poverty analysis in India. In Section III, we discuss the rising discrepancy between average expenditures as reported by the NSSO surveys and by the National Accounts Statistics (NAS) of the Central Statistical Office (CSO). In Section IV, we describe in detail the evolution of official poverty lines in India, while in Section V we discuss some recent controversies regarding the level of the official poverty line. In Sections VI to Section IX, we present the poverty estimates. In Section X, we discuss inequality over time in rural and urban areas of the 17 states. In Section XI, we offer our conclusions.

II. The Expenditure Surveys

The main source of data for estimating poverty in India is the expenditure survey conducted by the NSSO. India is perhaps the only developing country that began conducting such surveys on a regular basis as early as 1950–1951. The surveys have been conducted at least once a year since 1950–1951. However, the sample had been too small to permit reliable estimates of poverty at the level of the state until 1973–1974. A decision was made in the early 1970s to replace the smaller annual surveys by large-size expenditure (and employment–unemployment) surveys to be conducted every 5 years.

This decision led to the birth of "thick" quinquennial (5-yearly) surveys. Accordingly, the following 8 rounds of large-size surveys have been conducted: 27 (1973–1974), 32 (1978), 38 (1983), 43 (1987–1988), 50 (1993–1994), 55 (1999–2000), 61 (2004–2005), and 66 (2009–2010). Starting from the 42nd round in 1986–1987, a smaller expenditure survey was reintroduced. This was conducted annually except during the years in which the quinquennial survey was to take place. Therefore, with the exception of the 65th and 67th rounds in 2008–2009 and 2010–2011, respectively, an expenditure survey exists for each year beginning 1986–1987.

While the NSSO collects the data and produces reports providing information on monthly per-capita expenditures, it is the Planning Commission that computes the poverty lines and provides official estimates of poverty. The official estimates are strictly limited to quinquennial surveys. While they cover rural, urban, and total populations in different states and at the national level, estimates are not provided for specific social or religious groups. These can be calculated selectively for specific groups or specific years by researchers. With rare exceptions, discussions and debates on poverty have been framed around the quinquennial surveys even though the other survey samples are large enough to allow reliable estimates at the national level.

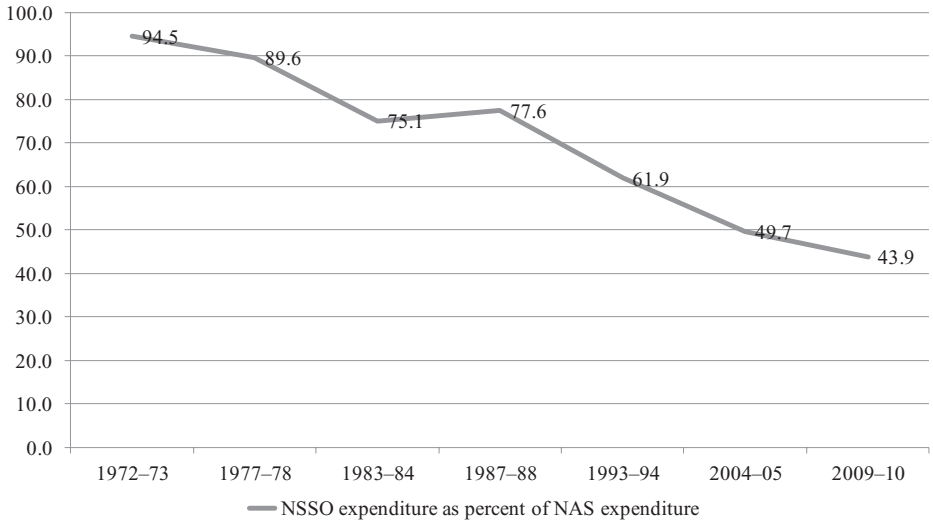
For each household interviewed, the survey collects data on the quantity of and expenditure on a large number of items purchased. For items such as education and health services, where quantity cannot be meaningfully defined, only expenditure data are collected. The list of items is elaborate. For example, the 66th round collected data on 142 items under the food category; 15 items under energy; 28 items under clothing, bedding, and footwear; 19 items under educational and medical expenses; 51 items under durable goods; and 89 in the other items category.

It turns out that household responses vary systematically according to the length of the reference period to which the expenditures are related. For example, a household could be asked about its expenditures on durable goods during the preceding 30 days or the preceding year. When the information provided in the first case is converted into annual expenditures, it is found to be systematically lower than when the survey directly asks households to report their annual spending. Therefore, estimates of poverty vary depending on the reference period chosen in the questionnaire.

Most quinquennial surveys have collected information on certain categories of relatively infrequently purchased items including clothing and consumer durables on the basis of both 30-day and 365-day reference periods. For other categories, including all food and fuel and consumer services, they have used a 30-day reference period. The data allow us to estimate two alternative measures of monthly per-capita expenditures that refer to the following: (i) a uniform reference period (URP) where all expenditure data used to estimate monthly per-capita expenditure are based on the 30-day reference period, and (ii) a mixed reference period (MRP) where expenditure data used to estimate the monthly per-capita expenditure are based on the 365-day reference period in the case of clothing and consumer durables and the 30-day reference period in the case of other items.

With rare exceptions, monthly per-capita expenditure associated with the MRP turns out to be higher than that associated with the URP. The Planning Commission's original estimate of poverty that employed the Lakdawala poverty lines had relied on the URP monthly per-capita expenditures. At some time prior to the Tendulkar Committee report, however, the Planning Commission decided to shift to the MRP estimates. Therefore, while recommending revisions that led to an upward adjustment in the rural poverty line, the Tendulkar Committee also shifted

Figure 1. **NSSO Household Total URP Expenditure Estimate as % of NAS Total Private Consumption Expenditure**



NAS = National Accounts Statistics. NSSO = National Sample Survey Office, URP = uniform reference period (based on the 30-day reference period).

Source: Authors' construction based on data from the Government of India (2008) until 2004–2005 and authors' calculations for 2009–2010.

to the MRP monthly per-capita expenditures in its poverty calculations. Therefore, the revised poverty estimates available for 1993–1994, 2004–2005, and 2009–2010 are based on the Tendulkar lines and the MRP estimates of monthly per-capita expenditures.

III. NSSO versus NAS Expenditure Estimates

We note an important feature of the NSSO expenditure surveys at the outset. The average monthly per-capita expenditure based on the surveys falls well short of the average private consumption expenditure separately available from the NAS of the CSO. Moreover, the proportionate shortfall has been progressively rising over successive surveys. These two observations hold regardless of whether we use the URP or MRP estimate of monthly per-capita expenditure available from the NSSO. Figure 1 graphically depicts this phenomenon in the case of URP monthly per-capita expenditure, which is more readily available for all quinquennial surveys since 1983.

Precisely what explains the gap between the NSSO and NAS expenditures has important implications for poverty estimates. For example, if the gap in any given year is uniformly distributed across all expenditure classes as Bhalla (2002) assumes in his work, true expenditure in 2009–2010 is uniformly more than twice of what the survey finds. This would imply that many individuals currently classified

as falling below the poverty line are actually above it. Moreover, a recognition that the proportionate gap between NSSO and NAS private expenditures has been rising over time implies that the poverty ratio is being overestimated by progressively larger margins over time. At the other extreme, if the gap between NSSO and NAS expenditures is explained entirely by underreporting of the expenditures by households classified as non-poor, poverty levels will not be biased upwards.

There are good reasons to believe, however, that the truth lies somewhere between these two extremes. The survey underrepresents wealthy consumers. For instance, it is unlikely that any of the billionaires, or most of the millionaires, are covered by the survey. Likewise, the total absence of error among households below the poverty line is highly unlikely. For example, recall that the expenditures on durables are systematically underreported for the 30-day reference period relative to that for 365-day reference period. Thus, in all probability, households classified as poor account for part of the gap so that there is some overestimation of the poverty ratio at any given poverty line.[2]

IV. The Official Poverty Lines

The 1993 expert group headed by Lakdawala defined all-India rural and urban poverty lines in terms of per-capita total consumption expenditure at 1973–1974 market prices. The underlying consumption baskets were anchored to the per-capita calorie norms of 2,400 and 2,100 in rural and urban areas, respectively. The rural and urban poverty line baskets were based on different underlying baskets, which meant that the two poverty lines represented different levels of real expenditures.

State-level rural poverty lines were derived from the national rural poverty line by adjusting the latter for price differences between national and state-level consumer price indices for agricultural laborers. Likewise, state-level urban poverty lines were derived from the national urban poverty line by adjusting the latter for price differences between the national and state-level consumer price indices for industrial laborers. National and state-level rural poverty lines were adjusted over time by applying the national and state-level price indices for agricultural workers, respectively. Urban poverty lines were adjusted similarly over time.

Lakdawala lines served as the official poverty lines until 2004–2005. The Planning Commission applied them to URP-based expenditures in the quinquennial

[2] We do not go into the sources of underestimation of expenditures in NSSO surveys. These are analyzed in detail in Government of India (2008). According to the report (Government of India 2008, p. 56), "The NSS estimates suffer from difference in coverage, underreporting, recall lapse in case of nonfood items or for the items which are less frequently consumed and increase in nonresponse particularly from affluent section of population. It is suspected that the household expenditure on durables is not fully captured in the NSS estimates, as the expensive durables are purchased more by the relatively affluent households, which do not respond accurately to the NSS surveys." Two items, imputed rentals of owner-occupied dwellings and financial intermediation services indirectly measured, which are included in the NAS estimate, are incorporated into the NSSO expenditure surveys. But these account for only 7–9 percentage points of the discrepancy.

surveys to calculate official poverty ratios. Criticisms of these estimates on various grounds led the Planning Commission to appoint an expert group under the chairmanship of Suresh Tendulkar in December 2005 with the directive to recommend appropriate changes in methodology for computing poverty estimates. The group submitted its report in 2009.

In its report, the Tendulkar committee noted three deficiencies of the Lakdawala poverty lines (Government of India 2009). First, the poverty line baskets remained tied to consumption patterns observed in 1973–1974. But more than 3 decades later, these baskets had shifted, even for the poor. Second, the consumer price index for agricultural workers understated the true price increase. This meant that over time the upward adjustment in the rural poverty lines was less than necessary so that the estimated poverty ratios understated rural poverty. Finally, the assumption underlying Lakdawala lines that health and education would be largely provided by the government did not hold any longer. Private expenditures on these services had risen considerably, even for the poor. This change was not adequately reflected in the Lakdawala poverty lines.

To remedy these deficiencies, the Tendulkar committee began by noting that the NSSO had already decided to shift from URP-based expenditures to MRP-based expenditures to measure poverty. With this in view, the committee's first step was to situate the revised poverty lines in terms of MRP expenditures in some generally acceptable aspect of the existing practice. To this end, it observed that since the nationwide urban poverty ratio of 25.7%, calculated from URP-based expenditures in the 2004–2005 survey, was broadly accepted as a good approximation of prevailing urban poverty, the revised urban poverty line could be anchored to yield this same estimate using MRP-based per-capita consumption expenditure from the 2004–2005 survey. This decision led to MRP-based per-capita expenditure of the individual at the 25.7 percentile in the national distribution of per-capita MRP expenditures becoming the national urban poverty line.

The Tendulkar committee further argued that the consumption basket associated with the national urban poverty line also be accepted as the rural poverty line consumption basket. This implied the translation of the new urban poverty line using the appropriate price index to obtain the nationwide rural poverty line. Under this approach, rural and urban poverty lines became fully aligned. Applying MRP-based expenditures, the new rural poverty line yielded a rural poverty ratio of 41.8% in 2004–2005 compared with 28.3% under the old methodology.

It is important to note that even though the method of pegging the national urban poverty line in the manner done by the Tendulkar committee left the national urban poverty in 2004–2005 originally measured at the Lakdawala urban poverty line unchanged, it did impact state-level urban poverty estimates. The methodology required that the state-level rural and urban poverty lines be derived from the national urban poverty line by applying the appropriate price indices derived from the price information within the sample surveys. In some cases, the state-level shift was

sufficiently large to significantly alter the estimate of urban poverty. For example, Lakdawala urban poverty line in Gujarat in 2004–2005 was Rs541.16 per-capita per month. The corresponding Tendulkar line turned out to be Rs659.18. This change led the urban poverty estimate in 2004–2005 to jump from 13.3% based on the Lakdawala line to 20.1% based on the Tendulkar line.

An important final point concerns the treatment of health and education spending by the Tendulkar Committee in recommending the revised poverty lines. On this issue, it is best to directly quote the Tendulkar Committee report (Government of India 2009, p. 2):

> Even while moving away from the calorie norms, the proposed poverty lines have been validated by checking the adequacy of actual private expenditure per capita near the poverty lines on food, education, and health by comparing them with normative expenditures consistent with nutritional, educational, and health outcomes. Actual private expenditures reported by households near the new poverty lines on these items were found to be adequate at the all-India level in both the rural and the urban areas and for most of the states. It may be noted that while the new poverty lines have been arrived at after assessing the adequacy of private household expenditure on education and health, the earlier calorie-anchored poverty lines did not explicitly account for these. The proposed poverty lines are in that sense broader in scope.

V. Controversies Regarding Poverty Lines[3]

We address here the two rounds of controversies over the poverty line that broke out in the media in September 2011 and March 2012. The first round of controversy began with the Planning Commission filing an affidavit with the Supreme Court stating that the poverty line at the time had been on average Rs32 and Rs26 per person per day in urban and rural India, respectively. Being based on the Tendulkar methodology, these lines were actually higher than the Lakdawala lines on which the official poverty estimates had been based until 2004–2005. However, the media and civil society groups pounced on the Planning Commission for diluting the poverty lines so as to inflate poverty reduction numbers and to deprive many potential beneficiaries of entitlements. For its part, the Planning Commission did a poor job of explaining to the public precisely what it had done and why.

The controversy resurfaced in March 2012 when the Planning Commission released the poverty estimates based on the 2009–2010 expenditure survey. The Planning Commission reported that these estimates were based on average poverty

[3] This section is partially based on Panagariya (2011).

lines of Rs28.26 and Rs22.2 per person per day in urban and rural areas, respectively. Comparing these lines to those previously reported to the Supreme Court, the media once again accused the Planning Commission of lowering the poverty lines.[4] The truth of the matter was that whereas the poverty lines reported to the Supreme Court were meant to reflect the price level prevailing in mid-2011, those underlying poverty estimates for 2009–2010 were based on the mid-point of 2009–2010. The latter poverty lines were lower because the price level at the mid-point of 2009–2010 was lower than that in mid-2011. In real terms, the two sets of poverty lines were identical.

While there was no basis to the accusations that the Planning Commission had lowered the poverty lines, the issue of whether the poverty lines remain excessively low despite having been raised does require further examination. In addressing this issue, it is important to be clear about the objectives behind the poverty line.

Potentially, there are two main objectives behind poverty lines: to track the progress made in combating poverty and to identify the poor towards whom redistribution programs can be directed. The level of the poverty line must be evaluated separately against each objective. In principle, we may want separate poverty lines for the two objectives.

With regard to the first objective, the poverty line should be set at a level that allows us to track the progress made in helping the truly destitute or those living in abject poverty, often referred to as extreme poverty. Much of the media debate during the two episodes focused on what could or could not be bought with the poverty-line expenditure.[5] There was no mention of the basket of goods that was used by the Tendulkar Committee to define the poverty line.

In Annex E of its report (Government of India 2009), the Tendulkar Committee gave a detailed itemized list of the expenditures of those "around poverty line class for urban areas in all India." Unfortunately, it did not report the corresponding quantities purchased of various commodities. In this paper, we now compute these quantities from unit-level data where feasible and report them in Table 1 for a household consisting of five members.[6] Our implicit per-person expenditures on individual items are within Rs3 of their corresponding expenditures reported in Annex E of the report of the Tendulkar Committee.

We report quantities wherever the relevant data are available. In the survey, the quantities are not always reported in weights. For example, lemons and oranges

[4] See, for example, the report by the NDTV entitled "Planning Commission further lowers poverty line to Rs28 per day." Available: http://www.ndtv.com/article/india/planning-commission-further-lowers-poverty-line-to-rs-28-per-day-187729

[5] For instance, one commentator argued in a heated television debate that since bananas in Jor Bagh (an upmarket part of Delhi) cost Rs60 a dozen, an individual could barely afford two bananas per meal per day at poverty line expenditure of Rs32 per person per day.

[6] We thank Rahul Ahluwalia for supplying us with Table 1. The expenditures in the table represent the average of the urban decile class including the urban poverty line. Since the urban poverty line is at 25.7% of the population, the table takes the average over those between the 20th and 30th percentile of the urban population.

Table 1. **The Tendulkar Poverty Line Basket**

Commodity Group	Expenditure in Current Rupees	Expenditure Share (%)	Quantity Consumed (kg)
Cereal	479.5	16.6	50.9
Pulses	97.0	3.4	3.5
Milk and milk products	223.5	7.8	16.2
Edible oil	142.5	4.9	2.7
Eggs, fish, and meat	99.0	3.4	6.2 eggs and 1.7 meat
Vegetables	191.0	6.6	23.9
Fresh Fruits	38.0	1.3	4.7
Dry Fruits	10.5	0.4	0.3
Sugar	66.5	2.3	3.7
Salt and spices	62.0	2.2	2.2
Intoxicants	64.0	2.2	n/a
Fuel	350.5	12.2	n/a
Other	138.0	4.8	n/a
Clothing	191.0	6.6	n/a
Footwear	30.5	1.1	n/a
Education	96.5	3.4	n/a
Medical: Institutional	21.5	0.7	n/a
Medical: Non-Institutional	105.0	3.6	n/a
Entertainment	30.5	1.1	n/a
Personal items	90.0	3.1	n/a
Other goods	70.5	2.4	n/a
Other services	87.5	3.0	n/a
Durables	45.0	1.6	n/a
Rent and conveyance	149.5	5.2	n/a
Total	2,880.0	100.0	n/a

Source: Authors' calculations using unit-level data (supplied by Rahul).

are reported in numbers and not in kilograms. In these cases, we have converted the quantities into kilograms using the appropriate conversion factors. The main point to note is that while the quantities associated with the poverty line basket may not permit a comfortable existence, including a balanced diet, they allow above-subsistence existence. The consumption of cereals and pulses at 50.9 kilograms (kg) and 3.5 kg compared with 48 kg and 5.5 kg, respectively, for the mean consumption of the top 30% of the population. Likewise, the consumption of edible oils and vegetables at 2.7 kg and 23.9 kg for the poor compared with 4.5 kg and 35.5 kg, respectively, for the top 30% of the population.[7] This comparison shows that, at least in terms of the provision of two square meals a day, the poverty line consumption basket is compatible with above-subsistence level consumption.

We reiterate our point as follows. In 2009–2010, the urban poverty line in Delhi was Rs1,040.3 per person per month (Rs34.2 per day). For a family of five, this amount would translate to Rs5,201.5 per month. Assuming that each family member consumes 10 kg per month of cereal and 1 kg per month of pulses and the prices of

[7]The consumption figures for the top 30% of the population are from Ganesh-Kumar et al. (2012).

the two grains are Rs15 and Rs80 per kilogram, respectively, the total expenditure on grain would be Rs1,150.[8] This would leave Rs4,051.5 for milk, edible oils, fuel, clothing, rent, education, health, and other expenditures. While this amount may not allow a fully balanced diet, comfortable living, and access to good education and health, it is consistent with an above-subsistence level of existence. Additionally, if we take into account access to public education and health, and subsidized grain and fuel from the public distribution system, the poverty line is scarcely out of line with the one that would allow exit from extreme poverty.

But what about the role of the poverty line in identifying the poor for purposes of redistribution? Ideally, this exercise should be carried out at the local level in light of resources available for redistribution, since the poor must ultimately be identified locally. Nevertheless, if the national poverty line is used to identify the poor, could we still defend the Tendulkar line as adequate? We argue in the affirmative.

Going by the urban and rural population weights of 0.298 and 0.702 implicit in the population projections for 1 January 2010, the average countrywide per-capita MRP expenditure during 2009–2010 amounts to Rs40.2 per person per day. Therefore, going by the expenditure survey data, equal distribution across the entire country would allow barely Rs40.2 per person per day in expenditures. Raising the poverty line significantly above the current level must confront this limit with regard to the scope for redistribution.

It could be argued that this discussion is based on data in the expenditure survey, which underestimates true expenditures. The scope for redistribution might be significantly greater if we go by expenditures as measured in the NAS. The response to this criticism is that the surveys underestimate not just the average national expenditure but also the expenditures of those identified as poor. Depending on the extent of this underestimation, the need for redistribution itself would be overestimated.

Even so, it is useful to test the limits of redistribution by considering the average expenditure according to the NAS. The total private final consumption expenditure at current prices in 2009–2010 was Rs37,959.01 billion. Applying the population figure of 1.174 billion as of 1 January 2010 in the NSSO 2009–2010 expenditure survey, this total annual expenditure translates to daily spending of Rs88.58 per person. This figure includes certain items such as imputed rent on owner-occupied housing and expenditures other than those by households such as the spending of civil society groups, which would not be available for redistribution. Thus, per-capita expenditures achievable through equal distribution, even when we consider the expenditures as per the NAS, is likely to be modest.

To appreciate further the folly of setting too high a poverty line for the purpose of identifying the poor, recall that the national average poverty line was Rs22.2 per

[8]These amounts of cereal and pulses equal or exceed their mean consumption levels according to the 2004–2005 NSSO expenditure survey.

person per day in rural areas and Rs28.26 in urban areas in 2009–2010. Going by the expenditure estimates for different spending classes in Government of India (2011a), raising these lines to just Rs33.3 and Rs45.4, respectively, would place 70% of the rural population and 50% of the urban population in poverty in 2009–2010. If we went a little further and set the rural poverty line at Rs39 per day and the urban poverty line at Rs81 per day in 2009–2010, we would place 80% of the population in each region below the poverty line. Will the fate of the destitute not be compromised if the meager tax revenues available for redistribution were thinly spread on this much larger population?

Before we turn to reporting the poverty estimates, we should clarify that while we have defended the current poverty line in India for both purposes—tracking abject poverty and redistribution—in general, we believe a case exists for two separate poverty lines to satisfy the two objectives. The poverty line to track abject poverty must be drawn independently of the availability of revenues for redistribution purposes and should be uniform nationally. The poverty line for redistribution purposes would in general differ from this line and, indeed, vary in different jurisdictions of the same nation depending on the availability of revenues. This should be evident from the fact that redistribution remains an issue even in countries that have entirely eradicated abject poverty.[9]

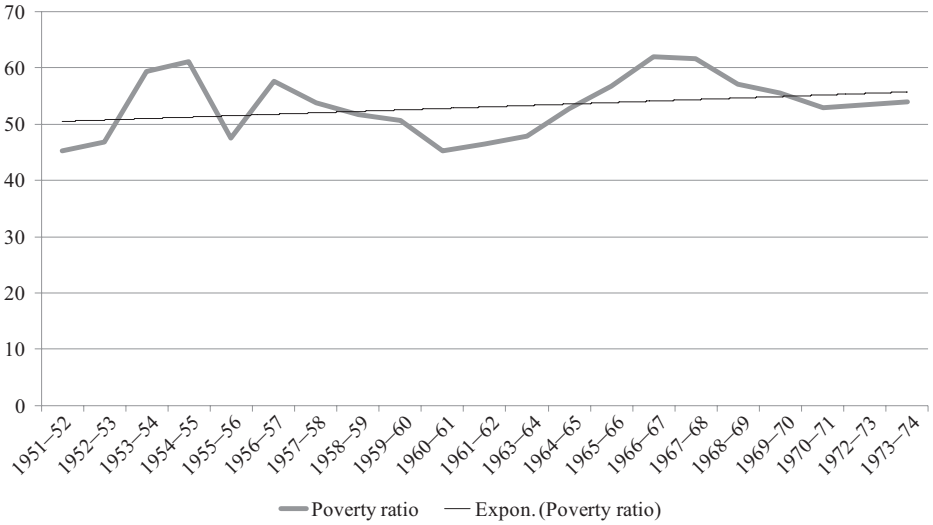
VI. Poverty at the National Level

Official poverty estimates are available at the national and state levels for the entire population, but not by social or religious groups, for all years during which the NSSO conducted quinquennial surveys. These years include 1973–1974, 1977–1978, 1983, 1987–1988, 1993–1994, 2004–2005, and 2009–2010, but not 1999–2000, as that year's survey became noncomparable to other quinquennial surveys due to a change in sample design. The Planning Commission has published poverty ratios for the first six of these surveys based on the Lakdawala lines and for the last three based on the Tendulkar lines. These ratios were estimated for rural and urban areas at the national and state levels.

In this paper, we provide comparable poverty rates for all of the last five quinquennial surveys including 2009–2010 derived from Lakdawala lines. For this purpose, we update the 2004–2005 Lakdawala lines to 2009–2010 using the price indices implicit in the official Tendulkar lines for 2004–2005 and 2009–2010 at the national and state levels. We provide estimates categorized by social as well as religious groups for all quinquennial surveys beginning in 1983 based on the

[9] Recently, Panagariya (2013) has suggested that if political pressures necessitate shifting up the poverty line, the government should opt for two poverty lines in India—the Tendulkar line, which allows it to track those in extreme poverty, and a higher one that is politically more acceptable in view of the rising aspirations of the people.

Figure 2. **The Poverty Ratio in India, 1951–1952 to 1973–1974 (%)**



Source: Datt, Gaurav. 1998. Poverty in India and Indian States: An Update. IFPRI Discussion Paper No. 47. Washington, DC: International Food Policy Research Institute.

Lakdawala lines and for the years relating to the last three such surveys based on the Tendulkar lines at the national and state levels.

While we focus mainly on the evolution of poverty since 1983 in this paper, it is useful to begin with a brief look at the poverty profile in the early years. This is done in Figure 2 using the estimates in Datt (1998) for years 1951–1952 to 1973–1974. The key message of the graph is that the poverty ratio hovered between 50% and 60% with a mildly rising trend.

This is not surprising, as India had been extremely poor at independence. Unlike economies such as Taipei,China; the Republic of Korea; Singapore; and Hong Kong, China, the country then grew very slowly. Growth in per-capita income during these years had been a mere 1.5% per year. Such low growth coupled with a very low starting per-capita income meant at best limited scope for achieving poverty reduction even through redistribution. As argued above, even today, after more than 2 decades of almost 5% growth in per-capita income, the scope for redistribution remains limited.[10]

We are now in a position to provide the poverty rates for the major social groups based on the quinquennial expenditure surveys beginning 1983. The social groups identified in the surveys are scheduled castes (SC), scheduled tribes (ST), other backward castes (OBC), and the rest, which we refer to as forward castes (FC). In addition, we define the nonscheduled castes as consisting of the OBC, and FC. The

[10]The issue is discussed at length in Bhagwati and Panagariya (2013).

Table 2. **National Rural and Urban Poverty Rates by Social Group Based on Lakdawala Lines (%)**

Social Group	1983	1987–1988	1993–1994	2004–2005	2009–2010
			Rural		
ST	64.9	57.8	51.6	47.0	30.5
SC	59.0	50.1	48.4	37.2	27.8
OBC				25.9	18.7
FC				17.5	11.6
NS	41.0	32.8	31.3	22.8	16.2
All groups	46.6	38.7	37.0	28.2	20.2
			Urban		
ST	58.3	56.2	46.6	39.0	31.7
SC	56.2	54.6	51.2	41.1	31.5
OBC				31.3	25.1
FC				16.2	12.1
NS	40.1	36.6	29.6	22.8	18.2
All groups	42.5	39.4	33.1	26.1	20.7
			Rural + Urban		
ST	64.4	57.6	51.2	46.3	30.7
SC	58.5	50.9	48.9	38.0	28.6
OBC				27.1	20.3
FC				17.0	11.8
NS	40.8	33.9	30.8	22.8	16.8
All groups	45.7	38.9	36.0	27.7	20.3

FC = forward castes, NS = non-scheduled, OBC = other backward castes, SC = scheduled castes, ST = scheduled tribes.
Source: Authors' calculations.

NSSO began identifying the OBC beginning 1999–2000. Since we are excluding this particular survey due to its lack of comparability with other surveys, the OBC as a separate group begins appearing in our estimates from 2004–2005 only.

In Table 2, we provide the poverty rates based on the Lakdawala lines in rural and urban areas and at the national level. Four features of this table are worthy of note. First, poverty rates have continuously declined for every single social group in both the rural and urban areas. Contrary to common claims, growth has been steadily helping the poor from every broad social group escape poverty rather than leaving the socially disadvantaged behind.

Second, the rates in rural India have consistently been the highest for the ST followed by the SC, OBC, and FC in that order. This pattern also holds in urban areas but with some exceptions. In particular, in some years, poverty rates of scheduled tribes are lower than that of scheduled castes, but this is not of great significance since more than 90% of the scheduled tribe population live in rural areas.

Third, with growth accelerating to above 8% beginning 2003–2004, poverty reduction between 2004–2005 and 2009–2010 has also accelerated. The percentage point reduction during this period has been larger than during any other 5-year period. Most importantly, the acceleration has been the greatest for the ST and SC

Table 3. **National Rural and Urban Poverty Rates by Social Group Based on the Tendulkar Line (%)**

Social Group	1993–1994	2004–2005	2009–2010
		Rural	
ST	65.7	64.5	47.4
SC	62.1	53.6	42.3
OBC		39.9	31.9
FC		27.1	21.0
NS	43.8	35.1	28.0
All groups	50.1	41.9	33.3
		Urban	
ST	40.9	38.7	30.4
SC	51.4	40.6	34.1
OBC		30.8	24.3
FC		16.2	12.4
NS	28.1	22.6	18.0
All groups	31.7	25.8	20.9
		Rural + Urban	
ST	63.5	62.4	45.6
SC	60.2	51.0	40.6
OBC		37.9	30.0
FC		23.0	17.6
NS	39.3	31.5	24.9
All groups	45.5	37.9	29.9

FC = forward castes, NS = non-scheduled, OBC = other backward castes, SC = scheduled castes, ST = scheduled tribes.
Source: Authors' calculations.

in that order so that at last, the gap in poverty rates between the scheduled and nonscheduled groups has declined significantly.

Finally, while the rural poverty rates were slightly higher than the urban poverty rates for all groups in 1983, the order switched for one or more groups in several of the subsequent years. Indeed, in 2009–2010, the urban rates turned out to be uniformly higher for every single group. This largely reflects progressive misalignment of the rural and urban poverty lines with the former becoming lower than the latter. It was this misalignment that led the Tendulkar Committee to revise the rural poverty line and realign it to the higher, urban line.

Table 3 reports the poverty estimates based on the Tendulkar lines. Recall that the Tendulkar line holds the urban poverty ratio at 25.7% in 2004–2005 when measuring poverty at MRP expenditures. Our urban poverty ratio in Table 3 reproduces this estimate within 0.1 of a percentage point.

The steady decline in poverty rates for the various social groups in rural as well as urban areas, which we noted based on the Lakdawala lines in Table 2, remains valid at the Tendulkar lines. Moreover, rural poverty ratios turn out to be higher than their urban counterparts for each group in each year. As in Table 2, the decline had been sharpest during the high-growth period between 2004–2005 and 2009–2010.

Table 4. **National Rural and Urban Poverty Rates by Religious Group Based on Lakdawala Lines (%)**

Religion	1983	1987–1988	1993–1994	2004–2005	2009–2010
			Rural		
Buddhism	59.4	57.7	53.8	43.4	33.6
Christianity	38.3	33.2	34.9	19.6	12.9
Hinduism	47.0	40.0	36.6	28.0	20.4
Islam	51.3	44.1	45.1	33.0	21.7
Jainism	12.9	7.8	14.1	2.6	0.0
Sikhism	12.0	10.1	11.7	10.4	3.7
Others	46.1	46.9	41.5	51.4	24.2
Total	46.5	39.8	37.0	28.2	20.2
			Urban		
Buddhism	51.1	62.1	51.9	42.2	39.3
Christianity	30.7	30.1	24.5	15.3	13.0
Hinduism	38.8	37.5	31.0	23.8	18.5
Islam	55.1	55.1	47.8	40.7	33.7
Jainism	18.5	17.7	6.4	4.5	2.1
Sikhism	19.7	11.3	11.1	3.2	5.5
Others	35.9	45.5	34.2	18.1	7.9
Total	40.4	39.8	33.1	26.1	20.7
			Rural + Urban		
Buddhism	57.5	58.9	53.2	43.0	36.0
Christianity	36.3	32.3	31.6	18.2	13.0
Hinduism	45.5	39.5	35.3	27.0	20.0
Islam	52.2	47.5	46.0	35.5	25.8
Jainism	16.8	14.2	8.3	4.1	1.9
Sikhism	13.4	10.4	11.6	8.8	4.2
Others	42.7	45.7	39.4	47.0	20.1
Total	45.4	39.8	36.0	27.7	20.4

Source: Authors' calculations.

Finally and most importantly, the largest percentage-point decline between these years in rural and urban areas combined had been for the ST followed by the SC, OBC, and FC in that order. Given that scheduled tribes also had the highest poverty rates followed by scheduled castes and other backward castes in 2004–2005, the pattern implies that the socially disadvantaged groups have achieved significant catching up with the better-off groups. This is a major break with past trends.

Next, we report the national poverty rates by religious groups. In Table 4, we show the poverty rates based on Lakdawala lines of rural and urban India and of the country taken as a whole. Three observations follow. First, at the aggregate level (rural plus urban), poverty rates show a steady decline for Hindus, Muslims, Christians, Jains, and Sikhs. Poverty among the Buddhists also consistently declined except for 1983 and 1987–1988. With one exception (Muslims in rural India between 1987–1988 and 1993–1994), the pattern of declining poverty rates between any two successive surveys also extends to the rural and urban poverty rates in the case of the two largest religious communities, Hindus and Muslims.

Table 5. **National Rural and Urban Poverty Rates by Religious Group Based on Tendulkar Lines (%)**

Religion	1993–1994	2004–2005	2009–2010
		Rural	
Buddhism	73.2	65.8	44.1
Christianity	44.9	29.8	23.8
Hinduism	50.3	42.0	33.5
Islam	53.5	44.6	36.2
Jainism	24.3	10.6	0.0
Sikhism	19.6	21.8	11.8
Others	57.3	57.8	35.3
Total	50.1	41.9	33.3
		Urban	
Buddhism	47.2	40.4	31.2
Christianity	22.6	14.4	12.9
Hinduism	29.5	23.1	18.7
Islam	46.4	41.9	34.0
Jainism	5.5	2.7	1.7
Sikhism	18.8	9.5	14.5
Others	31.5	18.8	13.6
Total	31.7	25.8	20.9
		Rural + Urban	
Buddhism	64.9	56.0	39.0
Christianity	38.4	25.0	20.5
Hinduism	45.4	37.5	29.7
Islam	51.1	43.7	35.5
Jainism	10.2	4.6	1.5
Sikhism	19.4	19.0	12.5
Others	51.2	52.5	29.9
Total	45.5	37.8	29.9

Source: Authors' calculations.

Second, going by the poverty rates in 2009–2010 in rural and urban areas combined, Jains have the lowest poverty rates followed by Sikhs, Christians, Hindus, Muslims, and Buddhists. Prosperity among Jains and Sikhs is well known, but not the lower level of poverty among Christians relative to Hindus. Also interesting is the relatively small gap of just 5.8 percentage points between poverty rates among Hindus and Muslims.

Finally, the impact of accelerated growth on poverty between 2004–2005 and 2009–2010 that we observed across social groups can also be seen across religious groups. Once again, we see a sharper decline in the poverty rate for the largest minority, the Muslims, relative to Hindus who form the majority of the population.

This broad pattern holds when we consider poverty rates by religious groups based on the Tendulkar line, as seen in Table 5. Jains have the lowest poverty rates followed by Sikhs, Christians, Hindus, Muslims, and Buddhists. With one exception (Sikhs in rural India between 1993–1994 and 2004–2005), poverty had declined steadily for all religious groups in rural as well as urban India. The only

difference is that the decline in poverty among Muslims in rural and urban areas combined between the periods 2004–2005 and 2009–2010 had not been as sharp as that estimated from the Lakdawala lines. As a result, we do not see a narrowing of the difference in poverty between Hindus and Muslims. We do see a narrowing of the difference in urban poverty but this gain is neutralized by the opposite movement in the rural areas due to a very sharp decline in poverty among Hindus, perhaps due to the rapid decline in poverty among scheduled castes and scheduled tribes.

Before we turn to poverty estimates by state, we should note that in this paper, we largely confine ourselves to reporting the extent of poverty measured based on the two poverty lines. Other than occasional references to the determinants of poverty such as growth and caste composition, we make no systematic effort to identify them. Evidently, many factors influence the decline in poverty. For instance, the acceleration in growth between 2004–2005 and 2009–2010 also led to increased revenue that made it possible for the government to introduce the National Rural Employment Guarantee Scheme under which one adult member of each rural household is guaranteed 100 days per year of employment at a pre-specified wage. The employment guarantee scheme may well have been a factor in the recent acceleration in poverty reduction.

In a similar vein, rural–urban migration may also impact the speed of decline of poverty. Once again, rapid growth, which inevitably concentrates disproportionately in urban areas, may lead to some acceleration in rural-to-urban migration. If, in addition, the rural poor migrate in proportionately larger numbers in search of jobs, poverty ratios could fall in both rural and urban areas. In the rural areas, the ratio could fall because proportionately more numerous poor than in the existing rural population migrate. In the urban areas, the decline may result from these individuals being gainfully employed at wages exceeding the urban poverty line. Migration may also reinforce the reduction in rural poverty by generating extra rural income through remittances. Evidence suggests that this effect may have been particularly important in the state of Kerala.

VII. Poverty in the States: Rural and Urban

We now turn to the progress made in poverty alleviation in different states. Though our focus in this paper is on poverty by social and religious groups, we first consider poverty at the aggregate level in rural and urban areas. India has 28 states and 7 union territories. To keep the analysis manageable, we limit ourselves to the 17 largest states.[11] Together, these states account for 95% of the total population.

[11] Although Delhi has its own elected legislature and chief minister, it remains a union territory. For example, central home ministry has the effective control of the Delhi police through the lieutenant governor who is the de jure head of the Delhi government and appointed by the Government of India.

Table 6. **Rural and Urban Population in the Largest 17 States of India, 2009–2010**

State	Rural (%)	Urban (%)	Total (million)
Uttar Pradesh	80	20	175
Maharashtra	58	42	97
Bihar	90	10	84
Andhra Pradesh	72	28	77
West Bengal	76	24	75
Tamil Nadu	55	45	64
Madhya Pradesh	76	24	62
Rajasthan	76	24	62
Gujarat	62	38	54
Karnataka	65	35	53
Orissa	86	14	36
Kerala	74	26	31
Assam	90	10	28
Jharkhand	80	20	26
Haryana	70	30	23
Punjab	65	35	23
Chhattisgarh	82	18	22
Total (17 largest states)	74	26	993
Total (all India)	73	27	1,043

Source: Authors' calculations.

We exclude all seven union territories including Delhi; the smallest six of the seven northeastern states (retaining only Assam); and the states of Sikkim, Goa, Himachal Pradesh, and Uttaranchal. Going by the expenditure survey of 2009–2010, each of the included states has a population exceeding 20 million while each of the excluded states has a population less than 10 million. Among the union territories, only Delhi has a population exceeding 10 million.

A. Rural and Urban Populations

We begin by presenting the total population in each of the 17 largest states and the distribution between rural and urban areas as revealed by the NSSO expenditure survey of 2009–2010 (Table 6).[12] The population totals in the expenditure survey are lower than the corresponding population projections by the registrar general and census commissioner of India (2006) as well as those implied by Census 2011.[13] Our choice is dictated by the principle that poverty estimates should be evaluated with reference to the population underlying the survey design instead of those suggested by external sources. For example, the urban poverty estimate in Kerala in 2009–2010

[12]Our absolute totals for rural and urban areas of the states and India in Table 6 match those in Tables 1A-R and 1A-U, respectively, in Government of India (2011b).

[13]The Planning Commission derives the absolute number of poor from poverty ratios using census-based population projections. Therefore, the population figure underlying the absolute number of poor estimated by the Planning Commission are higher than those in Table 6, which are based on the expenditure survey of 2009–2010.

must be related to the urban population in the state covered by the expenditure survey in 2009–2010 instead of projections based on the censuses in 2001 and 2011.[14]

As shown in Table 6, 27% of the national population lived in urban areas, while the remaining 73% resided in rural areas in 2009–2010. This composition understates the true share of the urban population, revealed to be 31.2% in the 2011 census. The table shows 10 states having populations of more than 50 million (60 million according to the 2011 census). We will refer to these 10 states as the "large" states. They account for a little more than three-fourths of the total population of India. At the other extreme, eleven "small" states (excluded from our analysis and therefore not shown in Table 6) have populations of less than ten million (13 million according to the Census 2011) each. The remaining seven states, which we call "medium-size" states, have populations ranging from 36 million in Orissa to 22 million in Chhattisgarh (42 million in Orissa to 25.4 million in Chhattisgarh, according to the 2011 census).

Among the large states, Tamil Nadu, Maharashtra, Gujarat, and Karnataka, in that order, are the most urbanized with a rate of urbanization of 35% or higher. Bihar is the least urbanized among the large states, with an urbanization rate of just 10%. Among the medium-size states, only Punjab has an urban population of 35%. The rest have urbanization rates of 30% or less. Assam and Orissa, with an urban population of just 10% and 14%, respectively, are the least urbanized medium-size states.

B. Rural and Urban Poverty

We now turn to the estimates of rural and urban poverty in the 17 largest states. To conserve space, we confine ourselves to presenting the estimates based on the Tendulkar line. We report the estimates based on the Lakdawala lines in the Appendix. Recall that the estimates derived from the Tendulkar line are available for 3 years: 1993–1994, 2004–2005, and 2009–2010. Disregarding 1973–1974 and 1977–1978, which are outside the scope of our paper, estimates based on the Lakdawala lines are available for an additional 2 years: 1983 and 1987–1988.

Table 7 reports the poverty estimates with the states arranged in descending order of their populations. Several observations follow. First, taken as a whole, poverty fell in each of the 17 states between 1993–1994 and 2009–2010. When we disaggregate rural and urban areas within each state, we still find a decline in poverty in all states in each region over this period. Indeed, if we take the 10 largest states, which account for three-fourths of India's population, every state except Madhya Pradesh experienced a consistent decline in both rural and urban poverty. The reduction in poverty with rising incomes is a steady and nationwide

[14] This distinction is a substantive one in the case of states in which the censuses reveal the degree of urbanization to be very different from that underlying the design of the expenditure surveys. For example, the expenditure survey of 2009–2010 places the urban population in Kerala at 26% of the total in 2009–2010, but the census in 2011 finds the rate of urbanization in the state to be 47.7%.

Table 7. **Rural and Urban Poverty in Indian States (%)**

State	Rural			Urban			Total		
	1993–1994	2004–2005	2009–2010	1993–1994	2004–2005	2009–2010	1993–1994	2004–2005	2009–2010
Uttar Pradesh	50.9	42.7	39.4	38.2	34.1	31.7	48.4	41.0	37.9
Maharashtra	59.2	47.8	29.5	30.2	25.6	18.3	48.4	38.9	24.8
Bihar	62.3	55.7	55.2	44.6	43.7	39.4	60.6	54.6	53.6
Andhra Pradesh	48.0	32.3	22.7	35.1	23.4	17.7	44.7	30.0	21.3
West Bengal	42.4	38.3	28.8	31.2	24.4	21.9	39.8	34.9	27.1
Tamil Nadu	51.0	37.6	21.2	33.5	19.8	12.7	44.8	30.7	17.4
Madhya Pradesh	48.8	53.6	42.0	31.7	35.1	22.8	44.4	49.3	37.3
Rajasthan	40.7	35.9	26.4	29.9	29.7	19.9	38.2	34.5	24.8
Gujarat	43.1	39.1	26.6	28.0	20.1	17.6	38.2	32.5	23.2
Karnataka	56.4	37.4	26.2	34.2	25.9	19.5	50.1	33.9	23.8
Orissa	63.0	60.7	39.2	34.3	37.6	25.9	59.4	57.5	37.3
Kerala	33.8	20.2	12.0	23.7	18.4	12.1	31.4	19.8	12.0
Assam	55.0	36.3	39.9	27.7	21.8	25.9	52.2	35.0	38.5
Jharkhand	65.7	51.6	41.4	41.8	23.8	31.0	61.1	47.2	39.3
Haryana	39.9	24.8	18.6	24.2	22.4	23.0	35.8	24.2	19.9
Punjab	20.1	22.1	14.6	27.2	18.7	18.0	22.2	21.0	15.8
Chhattisgarh	55.9	55.1	56.1	28.1	28.4	23.6	51.1	51.0	50.3
Total	50.1	41.9	33.3	31.7	25.8	20.9	45.5	37.9	29.9

Source: Authors' calculations.

phenomenon and not driven by the gains made in a few specific states or certain rural or urban areas of a given state.

Second, acceleration in poverty reduction in percentage points per year during the highest growth period (2004–2005 to 2009–2010) over that in 1993–1994 to 2004–2005 can be observed in 13 out of the total 17 states. The exceptions are Uttar Pradesh and Bihar among the large states and Assam and Haryana among medium-size states. Of these, Uttar Pradesh and Assam had experienced at best modest acceleration in gross state domestic product (GSDP) during the second period while Haryana had already achieved a relatively low level of poverty by 2004–2005. The most surprising had been the negligible decline in poverty in Bihar between 2004–2005 and 2009–2010, as GSDP in this state had grown at double-digit rates during this period.

Finally, among the large states, Tamil Nadu had the lowest poverty ratio followed by Andhra Pradesh and Gujarat. Tamil Nadu, Karnataka, and Andhra Pradesh—all of them from the south—made the largest percentage-point improvements in poverty reduction among the large states between 1993–1994 and 2009–2010. Among the medium-size states, Kerala and Haryana had the lowest poverty rates while Orissa and Jharkhand made the largest percentage-point gains during 1993–1994 to 2009–2010.

It is useful to relate poverty levels to per-capita spending. In Table 8, we present per-capita expenditures in current rupees in the 17 states in the 3 years

Table 8. **Per-capita Expenditures in Rural and Urban Areas in the States (current Rs)**

	1993–1994 URP		2004–2005 MRP		2009–10 MRP	
State	**Rural**	**Urban**	**Rural**	**Urban**	**Rural**	**Urban**
Uttar Pradesh	274	389	539	880	832	1,512
Maharashtra	273	530	597	1,229	1,048	2,251
Bihar	218	353	445	730	689	1,097
Andhra Pradesh	289	409	604	1,091	1,090	2,015
West Bengal	279	474	576	1,159	858	1,801
Tamil Nadu	294	438	602	1,166	1,017	1,795
Madhya Pradesh	252	408	461	893	803	1,530
Rajasthan	322	425	598	945	1,035	1,577
Gujarat	303	454	645	1,206	1,065	1,914
Karnataka	269	423	543	1,138	888	2,060
Orissa	220	403	422	790	716	1,469
Kerala	390	494	1,031	1,354	1,763	2,267
Assam	258	459	577	1,130	867	1,604
Jharkhand			439	1,017	724	1,442
Haryana	385	474	905	1,184	1,423	2,008
Punjab	433	511	905	1,306	1,566	2,072
Chhattisgarh			445	963	686	1,370
All-India	281	458	579	1,105	953	1,856

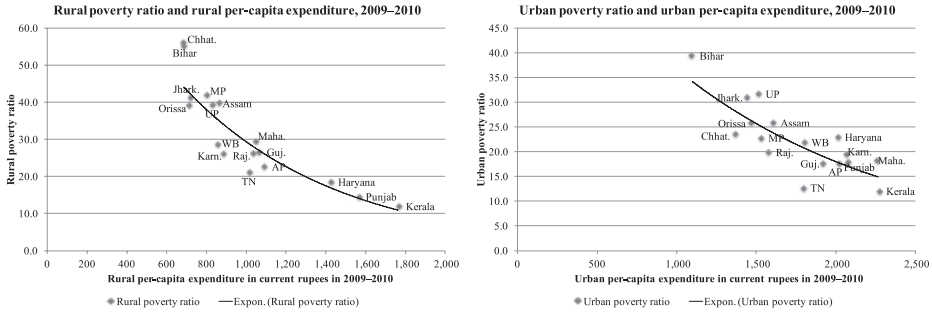
MRP = mixed reference period, URP = uniform reference period.
Source: Authors' calculations.

for which we have poverty ratios, with the states ranked in descending order of population. Ideally, we should have the MRP expenditures for all 3 years, but since they are available for only the last 2 years, we report the URP expenditures for 1993–1994. Several observations follow from a comparison of Tables 7 and 8.

First, high per-capita expenditures are associated with low poverty ratios. Consider, for example, rural poverty in 2009–2010. Kerala, Punjab, and Haryana, in that order, have the highest rural per-capita expenditures. They also have the lowest poverty ratios, in the same order. At the other extreme, Chhattisgarh and Bihar have the lowest rural per-capita expenditures and also the highest rural poverty ratios. More broadly, the top nine states by rural per-capita expenditure are also the top nine states in terms of low poverty ratios. A similar pattern can also be found for urban per-capita expenditures and urban poverty. Once again, Kerala ranks at the top and Bihar at the bottom in terms of each indicator. Figure 3 offers a graphical representation of the relationship in rural and urban India in 2009–2010 using state level data.

One state that stands out in terms of low poverty ratios despite a relatively modest ranking in terms of per-capita expenditure is Tamil Nadu. It ranked eighth in terms of rural per-capita expenditure but fourth in terms of rural poverty in 2009–2010. In terms of urban poverty, it did even better, ranking a close second despite its ninth rank in urban per-capita expenditure. Gujarat also did very well in terms of urban poverty, ranking third in spite of the seventh rank in urban per-capita expenditure.

Figure 3. **Poverty and Per-capita MRP Expenditure in Rural and Urban Areas in Indian States, 2009–2010**



Source: Authors' calculations.

Finally, there is widespread belief that Kerala achieved the lowest rate of poverty despite its low per-capita income through more effective redistribution. Table 8 entirely repudiates this thesis. In 1993–1994, Kerala already had the lowest rural and urban poverty ratios and enjoyed the second highest rural per-capita expenditure and third highest urban per-capita expenditure among the 17 states. Moreover, in terms of percentage-point reduction in poverty, all other southern states dominate Kerala. For example, between 1993–1994 and 2004–2005, Tamil Nadu achieved a 27.4 percentage-point reduction in poverty compared to just 19.3 for Kerala. We may also add that Kerala experienced very high inequality of expenditures. In 2009–2010, the Gini coefficient associated with spending in the state was by far the highest among all states in rural as well as urban areas.

VIII. Poverty in the States by Social Group

In this section we decompose population and poverty by social group. As previously mentioned, the expenditure surveys traditionally identified the social group of the households using a three-way classification: scheduled castes, scheduled tribes, and nonscheduled castes. However, beginning with the 1999–2000 survey, the last category had been further subdivided into other backward castes and the rest, the latter sometimes referred to as forward castes, a label that we use in this paper.

We begin by describing the shares of the four social groups in the total population of the 17 states.

A. Population Distribution by Social Group within the States

Table 9 reports the shares of various social groups in the 17 largest states according to the expenditure survey of 2009–2010. We continue to rank the states according to population from the largest to the smallest.

Table 9. **Shares of Different Social Groups in the State Population, 2009–2010 (%)**

State	ST	SC	OBC	FC	NS	Total (million)
Uttar Pradesh	1	25	51	23	74	175
Maharashtra	10	15	33	43	75	97
Bihar	2	23	57	18	75	84
Andhra Pradesh	5	19	49	27	76	77
West Bengal	6	27	7	60	67	75
Tamil Nadu	1	19	76	4	79	64
Madhya Pradesh	20	20	41	19	60	62
Rajasthan	14	21	46	19	65	62
Gujarat	17	11	37	35	72	54
Karnataka	9	18	45	28	73	53
Orissa	22	21	32	25	57	36
Kerala	1	9	62	27	90	31
Assam	15	12	26	47	73	28
Jharkhand	29	18	38	15	53	26
Haryana	1	29	30	40	70	23
Punjab	1	39	16	44	61	23
Chhattisgarh	30	15	41	14	55	22
India (17 states)	8	21	43	28	71	993
India (all states)	9	20	42	29	71	1,043

FC = forward castes, NS = non-scheduled, OBC = other backward castes, SC = scheduled castes, ST = scheduled tribes.
Source: Authors' calculations from the NSSO expenditure survey conducted in 2009–2010.

Nationally, the Scheduled Tribes constitute 9% of the total population of India according to the expenditure survey of 2009–2010. In past surveys and the Census 2001, this proportion was 8%. The scheduled castes form 20% of the total population according to the NSSO expenditure surveys, though the Census 2001 placed this proportion at 16%. The OBC are not identified as a separate group in the censuses so that their proportion can be obtained from the NSSO surveys only. The figure has varied from 36% to 42% across the three quinquennial expenditure surveys since the OBC began to be recorded as a separate group.

The scheduled tribes are more unevenly divided across states than the remaining social groups. In so far as these groups had been very poor at independence and happened to be outside the mainstream of the economy, ceteris paribus, states with high proportions of ST population may be at a disadvantage in combating poverty. From this perspective, the four southern states enjoy a clear advantage: Kerala and Tamil Nadu have virtually no tribal populations while Andhra Pradesh and Karnataka have proportionately smaller tribal populations (5% and 9% of the total, respectively) than some of the northern states which had high concentrations.

Among the large states, Madhya Pradesh, Gujarat, and Rajasthan have proportionately the largest concentrations of ST populations. The ST constitute 20%, 17%, and 14% of their respective populations. Some of the medium-size states, of course, have proportionately even larger concentrations. These include Chhattisgarh,

Table 10. **Distribution of the National Population across Social Groups and Regions (%)**

Region	ST	SC	OBC	FC	NS	Total (million)
Rural	89	80	75	60	69	761
Urban	11	20	25	40	31	282
Total	100	100	100	100	100	1,043

FC = forward castes, NS = non-scheduled, OBC = other backward castes, SC = scheduled castes, ST = scheduled tribes.
Source: Authors' calculations.

Jharkhand, and Orissa with the ST forming 30%, 29%, and 22% of their populations, respectively.

Since the traditional exclusion of the SC has meant they began with a very high incidence of abject poverty and low levels of literacy, states with high proportions of these groups also face an uphill task in combating poverty. Even so, since the SC populations are not physically isolated from the mainstream of the economy, there is greater potential for the benefits of growth reaching them than the ST. This is illustrated, for example, by the emergence of some rupee millionaires among the SC but not the ST during the recent high-growth phase (Dehejia and Panagariya 2012).

Once again, at 9%, Kerala has proportionately the smallest SC population among the 17 states listed in Table 9. Among the largest 10 states, West Bengal, Uttar Pradesh, Bihar, Rajasthan, and Madhya Pradesh have the highest concentrations. Among the medium-size states, Punjab, Haryana, and Orissa in that order have proportionately the largest SC populations.

The SC and ST populations together account for as much as 40% and 35%, respectively, of the total state population in Madhya Pradesh and Rajasthan. At the other extreme, in Kerala, these groups together account for only 10% of the population. These differences mean that, ceteris paribus, Madhya Pradesh, and Rajasthan face a significantly more difficult battle in terms of combating poverty than Kerala.

The ST populations also differ from the SC in that they are far more heavily concentrated in rural areas than in urban areas. Table 10 illustrates this point. In 2009–2010, 89% of the ST population was classified as rural. The corresponding figure was 80% for the SC, 75% for the OBC, and 60% for FC.

An implication of the small ST population in the urban areas in all states and in both rural and urban areas in a large number of states is that the random selection of households results in a relatively small number of ST households being sampled. The problem is especially severe in many of the smallest states where the total sample size is small in the first place. A small sample translates into a large error in the associated estimate of the poverty ratio. We will present the poverty estimates in all states and regions as long as a positive group is sampled. Nevertheless, we caution the reader on the possibility of errors in Table 11 that may be associated with the number of ST households in the 2009–2010 survey.

Table 11. **Number of Scheduled Tribe Households in the 2009–2010 Expenditure Survey**

State	Rural	Urban	Rural + Urban
Uttar Pradesh	46	30	76
Maharashtra	468	150	618
Bihar	66	21	87
Andhra Pradesh	312	76	388
West Bengal	230	74	304
Tamil Nadu	38	33	71
Madhya Pradesh	569	127	696
Rajasthan	407	75	482
Gujarat	467	81	548
Karnataka	153	107	260
Orissa	669	149	818
Kerala	31	13	44
Assam	488	84	572
Jharkhand	610	136	746
Haryana	13	9	22
Punjab	7	12	19
Chhattisgarh	520	98	618
India (all states)	5,359	1,323	6,682

Source: Authors' calculations.

B. Poverty by Social Group

We now turn to poverty estimates by social groups. We present statewide poverty ratios based on the Tendulkar line for the ST, SC, and nonscheduled castes in Table 12. We present the ratios for the OBC and FC in Table 13. As before, we arrange the states from the largest to the smallest according to population. Separate rural and urban poverty estimates derived from the Tendulkar lines and Lakdawala lines are relegated to the Appendix.

With one exception, Chhattisgarh, the poverty ratio declines for each group in each state between 1993–1994 and 2009–1010. There is little doubt that rising incomes have helped all social groups nearly everywhere. In the vast majority of the states, we also observe acceleration in the decline in poverty between 2004–2005 and 2009–2010 compared to between 1993–94 and 2004–2005. Reassuringly, the decline in ST poverty among scheduled tribes and scheduled castes and SC poverty has sped up recently with the gap in poverty rates between these groups and the nonscheduled castes narrowing.

The negative relationship between poverty ratios and per-capita expenditures that we depicted in Figure 3 can also be observed for the social groups taken separately. Using rural poverty estimates by social group in the Appendix, we show this relationship between SC poverty and per capita rural expenditures in the left panel of Figure 4 and that between the ST poverty and per capita rural expenditures in the right panel. Figure 4 closely resembles Figure 3. The fit in the right panel is poorer than that in the left panel as well as those in Figure 3. This is partially

Table 12. **Poverty in the States by Social Groups Based on the Tendulkar Line (%)**

	ST			SC			NS		
State	1993–1994	2004–2005	2009–2010	1993–1994	2004–2005	2009–2010	1993–1994	2004–2005	2009–2010
Uttar Pradesh	45.7	41.7	40.1	68.1	55.2	52.4	42.8	36.7	32.9
Maharashtra	71.5	68.1	48.5	65.0	52.9	34.7	41.9	32.3	19.8
Bihar	72.1	59.1	62.0	75.4	77.0	67.7	56.0	48.2	49.2
Andhra Pradesh	56.7	59.3	37.6	61.7	40.3	24.5	39.8	24.7	19.4
West Bengal	64.2	54.0	31.6	48.5	37.9	32.6	33.5	31.9	24.5
Tamil Nadu	47.4	41.9	14.1	64.0	48.6	28.8	39.4	25.5	14.7
Madhya Pradesh	68.3	77.4	61.0	55.6	62.0	41.9	33.0	35.9	27.9
Rajasthan	62.1	57.9	35.4	54.0	49.0	37.1	29.6	25.2	18.7
Gujarat	51.2	54.7	47.6	54.1	40.1	21.8	32.6	27.1	17.6
Karnataka	68.6	51.2	24.2	69.1	53.8	34.4	43.6	27.6	21.2
Orissa	80.4	82.8	62.7	60.6	67.4	47.1	50.6	44.8	24.0
Kerala	35.2	54.4	21.2	50.3	31.2	27.4	29.4	17.8	10.4
Assam	54.1	28.8	31.9	57.8	44.3	36.6	51.3	35.2	40.2
Jharkhand	71.2	59.8	50.9	72.5	59.7	43.5	53.3	38.9	31.5
Haryana	65.7	6.7	57.4	59.1	47.4	37.8	27.4	16.3	12.1
Punjab	36.8	18.7	15.5	37.7	37.9	29.2	13.9	11.5	7.3
Chhattisgarh	64.0	62.9	65.0	52.6	48.0	60.1	42.1	44.5	39.6
Total	63.5	62.4	45.6	60.2	51.0	40.6	39.3	31.5	24.9

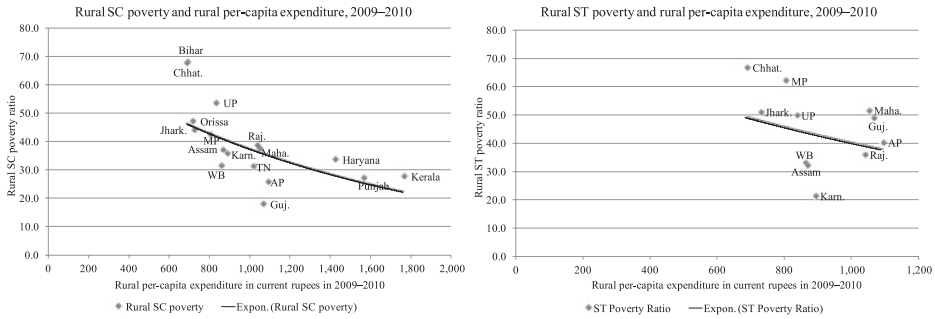
NS = non-scheduled, SC = scheduled castes, ST = scheduled tribes.
Source: Authors' calculations.

Table 13. **Poverty among Nonscheduled Castes Based on the Tendulkar Line (%)**

	OBC		FC	
State	2004–2005	2009–2010	2004–2005	2009–2010
Uttar Pradesh	42.2	38.7	24.4	20.3
Maharashtra	39.1	25.2	27.5	15.6
Bihar	52.5	55.0	33.9	30.2
Andhra Pradesh	29.7	23.3	16.3	12.3
West Bengal	27.5	27.0	32.3	24.2
Tamil Nadu	26.6	15.1	10.1	6.9
Madhya Pradesh	45.3	31.1	19.2	21.1
Rajasthan	28.0	22.1	19.4	10.5
Gujarat	40.5	28.1	12.4	6.3
Karnataka	34.6	23.9	20.1	16.7
Orissa	51.3	25.6	33.2	21.9
Kerala	21.3	12.3	10.1	5.9
Assam	31.4	30.2	36.5	45.8
Jharkhand	43.0	36.6	27.0	18.8
Haryana	28.1	19.5	8.1	6.5
Punjab	21.3	16.5	6.9	3.9
Chhattisgarh	48.4	43.3	26.3	28.6
Total	37.9	30.0	23.0	17.6

FC = forward castes, OBC = other backward castes.
Source: Authors' calculations.

Figure 4. **Scheduled Caste and Scheduled Tribe Poverty Rates and Per-capita MRP Expenditures in Rural Areas, 2009–2010**



MRP = mixed reference period, SC = scheduled castes, ST = scheduled tribes.
Source: Authors' calculations.

because the ST are often outside the mainstream of the economy and therefore less responsive to rising per-capita incomes. This factor is presumably exacerbated by the fact that the number of observations in the case of the ST has been reduced to 11 due to the number of ST households in the sample dropping to below 100 in six of the 17 states.

For years 2004–2005 and 2009–2010, we disaggregate the nonscheduled castes into the OBC and FC. The resulting poverty estimates are provided in Table 13. Taking the estimates in Tables 12 and 13, one can see that on average poverty rates are at their highest for the ST followed by SC, OBC, and FC in that order. At the level of individual states, ranking of the poverty rates of scheduled castes and scheduled tribes is not clear-cut, but with rare exceptions, poverty rates of these two groups exceed systematically those of other backward castes, which in turn exceed rates of forward castes.

An interesting feature of the poverty rates of forward castes is their low level in all but a handful of the states. For example, in 2009–2010, the statistic computed to just 3.9% in Punjab, 5.9% in Kerala, 6.5% in Haryana, 6.9% in Tamil Nadu, and 10.5% even in Rajasthan. In 14 out of the largest 17 states, it fell below 25%. The states with low FC poverty rates generally also have low OBC poverty rates making the proportion of the SC and ST population the key determinant of the statewide rate.

This point is best illustrated by a comparison of poverty rates of Punjab and Kerala. Poverty rates for the nonscheduled caste population in 2009–2010 was 7.3% in Punjab and 10.4% in Kerala, while those for scheduled castes stood at 29.2% and 27.4%, respectively, in the two states. But since scheduled castes constitute 39% of the population in Punjab but only 9% in Kerala, statewide poverty rate turned out to be 15.8% in the former and 12% in the latter.

The caste composition also helps explain the differences in poverty rates between Maharashtra and Gujarat on the one hand and Kerala on the other. In 2009–2010, statewide poverty rates were 24.8% and 23.2%, respectively, in the

Table 14. **Composition of Population by Religion and Rural–Urban Division of Each Group, 2009–2010 (%)**

Religion	Rural	Urban	Population (million)
Hinduism	74	26	856
Islam	66	34	133
Christianity	70	30	24
Sikhism	75	25	18
Buddhism	60	40	7
Jainism	13	87	3
Zoroastrianism	3	97	0.16
Others	79	21	3
Total	73	27	1,043

Source: Authors' calculations.

former and 12% in the latter (Table 10). In part, the differences follow from the significantly higher per-capita expenditures in Kerala, as seen from Table 11.[15] But Maharashtra and Gujarat also face a steeper uphill task in combating poverty on account of significantly higher proportions of the scheduled tribe and scheduled caste populations. These groups account for 17% and 11%, respectively, of the total population in Gujarat, and 10% and 15% in Maharashtra. In comparison, only 1% of the population comprises scheduled tribes in Kerala, while just 9% comprise scheduled castes (Table 9).

IX. Poverty in the States by Religious Group

Finally, we turn to poverty estimates by religious group in the states. India is home to many different religious communities including Hindus, Muslims, Christians, Sikhs, Jains, and Zoroastrians. Additionally, tribes follow their own religious practices. Though tribal religions often have some affinity with Hinduism, many are independent in their own right.

Table 14 provides the composition of population by religious group as well as the rural–urban split of each religious group based on the expenditure survey of 2009–2010. Hindus comprise 82% of the population, Muslims 12.8%, Christians 2.3%, Sikhs 1.7%, Jains 0.3%, and Zoroastrians 0.016%. The remaining comprises just 0.3%.

Together, Hindus and Muslims account for almost 95% of India's total population. With 34% of the population in urban areas compared with 26% in the case of Hindus, Muslims are more urbanized than Hindus. Among the other communities, Jains and Zoroastrians are largely an urban phenomenon. Moreover, while Muslims can be found in virtually all parts of India, other smaller minority communities tend

[15]This is true in spite of significantly higher per-capita GSDP in Maharashtra presumably due to large remittances flowing into Kerala. According to the Government of India (2011a), one in every three households in both rural and urban Kerala reports at least one member of the household living abroad.

Table 15. **Number of Households Sampled by Religious Groups in the States, 2009–2010**

	Hindus			Muslims			Others		
State	**Rural**	**Urban**	**Total**	**Rural**	**Urban**	**Total**	**Rural**	**Urban**	**Total**
Uttar Pradesh	5,079	2,155	7,234	812	894	1,706	15	38	53
Maharashtra	3,599	2,971	6,570	188	600	788	228	409	637
Bihar	2,789	1,098	3,887	498	164	662	12	9	21
Andhra Pradesh	3,540	2,380	5,920	254	468	722	134	116	250
West Bengal	2,425	2,405	4,830	1,102	322	1,424	49	22	71
Tamil Nadu	3,068	2,817	5,885	83	271	354	169	230	399
Madhya Pradesh	2,611	1,662	4,273	92	248	340	28	56	84
Rajasthan	2,395	1,205	3,600	129	267	396	59	81	140
Gujarat	1,584	1,406	2,990	130	251	381	5	48	53
Karnataka	1,825	1,648	3,473	189	304	493	22	82	104
Orissa	2,880	991	3,871	39	44	83	56	20	76
Kerala	1,389	1,078	2,467	614	423	1,037	603	345	948
Assam	1,749	719	2,468	779	97	876	88	15	103
Jharkhand	1,388	799	2,187	165	94	259	205	96	301
Haryana	1,311	1,105	2,416	51	35	86	78	40	118
Punjab	360	951	1,311	30	36	66	1,170	568	1,738
Chhattisgarh	1,458	659	2,117	6	45	51	32	32	64
Total	39,450	26,049	65,499	5,161	4,563	9,724	2,953	2,207	5,160

Source: Authors' calculations.

to be geographically concentrated. Sikhs cluster principally in Punjab, Christians in Kerala and adjoining southern states, Zoroastrians in Maharashtra and Gujarat, and Jains in Gujarat, Rajasthan, Karnataka, and Tamil Nadu.

Given their small shares in the total population and their geographical concentration, random sampling of households in the expenditure surveys yields less than 100 observations for minority religious communities other than Muslims in the vast majority of the states. Indeed, as Table 15 indicates, only 13 out of the 17 largest states had a sufficiently large number of households even for Muslims to allow poverty to be reliably estimated. Orissa, Haryana, Punjab, and Chhattisgarh each had fewer than 100 Muslim households in the survey. Thus, we attempt poverty estimates by religious groups in the states separately for Hindus and Muslims only. We do provide estimates for the catch-all "other" category but caution that, in many cases, these estimates are based on less than 100 observations and therefore subject to large statistical errors.

As before, we present the estimates for statewide poverty of the religious groups using the Tendulkar line, placing the more detailed estimates for rural and urban areas and estimates based on the Lakdawala lines in the Appendix. Table 15 reports the estimates for Hindus, Muslims, and other minority religion groups for the years 1993–1994, 2004–2005, and 2009–2010.

Religious groups replicate the broad pattern seen in the context of poverty by social group. Poverty has fallen in every single state between 1993–1994 and 2009–2010 for Hindus as well as for Muslims, though the change is not always monotonic. While the level of poverty in 2009–2010 is higher for Muslims than

Hindus in the majority of the states, the reverse is true in Bihar, Tamil Nadu, Madhya Pradesh, and Karnataka. An anomaly is the marginal increase in the poverty rate between 2004–2005 and 2009–2010 in Bihar for Hindus and in Gujarat for Muslims. The observation is particularly surprising since we simultaneously observe a significant decline in poverty during the same period for Muslims in Bihar and for Hindus in Gujarat. Interestingly, as documented in the Appendix, poverty rates for both Hindus and Muslims decline in both states based on the Lakdawala lines between 2004–2005 and 2009–2010.

X. Inequality

Although the focus of this paper is on poverty, we find it useful to briefly report the evolution of inequality at the state and national levels in rural and urban areas. At the outset, it is important to note that the issue of inequality is complex partly because it can be measured in numerous ways.[16] The potential list of measures is almost endless, and there is no guarantee that these different measures will move in the same direction. Therefore, it is quite easy to show simultaneously that inequality has risen as well as fallen depending on the choice of measure.

In this paper, we use one measure of overall inequality based on the same expenditure survey data we used to report poverty measures in the previous sections: specifically, the Gini coefficient of household expenditures in rural and urban areas in the 17 states and in India as a whole using URP expenditures in 1983, 1993–1994, 1999–2000, 2004–2005, and 2009–2010. Table 17 and Table 18 report the Gini coefficient in rural and urban areas, respectively. As before, we arrange the states in descending order of population size.

An immediate observation from Tables 17 and 18 is that, with rare exceptions, rural inequality tends to be lower than urban inequality. At the national level in 2009–2010, the Gini coefficient was 0.291 in rural areas and 0.382 in urban areas. These values reflect a difference of 9 percentage points. This is not surprising. The vast majority of the villagers are small farmers or wage laborers. As a result, variation in their incomes and therefore expenditures are not large. In contrast, cities serve as home to much of the industry and formal sector services as well as to a large informal sector which attracts migrant workers. This results in greater variation in incomes and expenditures.

The tables show no clear trend in the Gini in rural areas but do show a tendency for it to rise in urban areas. At the national level, rural Gini fell between 1983 and 1999–2000, rose between 1999–2000 and 2004–2005, and fell again between 2004–2005 and 2009–2010, with a small net decline over the entire period. In contrast, the urban Gini has climbed steadily.

[16]For instance, inequality could be measured as the ratio of the top 10% to bottom 10% of the population, the ratio of rural to urban per-capita incomes, the ratio of skilled to unskilled wages (or formal and informal sector wages), and through the Gini coefficient (nationally or across states).

Table 16. **Poverty by Religious Group (%)**

State	Hindus			Muslims			Others		
	1993–1994	2004–2005	2009–2010	1993–1994	2004–2005	2009–2010	1993–1994	2004–2005	2009–2010
Uttar Pradesh	48.3	39.7	36.2	50.5	47.4	46.1	9.3	26.0	4.3
Maharashtra	47.7	37.4	23.7	49.9	45.6	28.5	55.4	47.7	33.6
Bihar	59.0	53.5	54.0	69.0	61.0	52.4	56.6	35.1	26.8
Andhra Pradesh	44.5	30.0	21.2	44.3	30.3	22.6	49.9	32.8	22.1
West Bengal	36.2	29.7	23.9	51.2	48.6	34.5	59.2	47.3	43.4
Tamil Nadu	45.2	31.6	17.8	35.5	18.8	12.7	50.5	29.7	15.1
Madhya Pradesh	45.1	49.9	38.2	38.9	46.7	27.6	26.4	4.7	5.0
Rajasthan	37.9	34.8	24.6	48.1	36.9	31.6	22.8	19.2	9.3
Gujarat	38.0	32.7	21.9	42.3	36.5	37.6	35.9	11.5	1.4
Karnataka	50.8	33.9	24.5	51.5	38.3	20.6	26.7	8.4	7.5
Orissa	59.4	57.5	36.9	52.6	38.6	38.0	74.8	80.6	69.6
Kerala	30.8	20.3	12.1	38.8	25.9	15.2	25.1	10.1	7.9
Assam	48.0	27.1	30.8	62.6	50.3	53.6	66.4	43.9	42.3
Jharkhand	59.9	45.1	37.8	68.3	51.4	49.0	65.4	58.8	43.8
Haryana	34.0	24.1	19.4	62.3	44.6	33.8	41.0	15.0	16.9
Punjab	23.6	21.6	18.1	40.4	32.3	11.6	20.4	20.8	14.6
Chhattisgarh	52.8	51.3	51.3	11.5	48.6	15.7	11.3	35.2	21.6
Total	45.5	37.6	29.7	51.0	43.7	35.4	34.3	26.3	19.4

Source: Authors' calculations.

Table 17. **The Gini Coefficient in Rural Areas**

State	1983	1993–1994	1999–2000	2004–2005	2009–2010
Uttar Pradesh	0.290	0.278	0.246	0.286	0.356
Maharashtra	0.283	0.302	0.258	0.308	0.268
Bihar	0.255	0.222	0.207	0.205	0.226
Andhra Pradesh	0.292	0.285	0.235	0.289	0.278
West Bengal	0.284	0.251	0.224	0.270	0.239
Tamil Nadu	0.324	0.307	0.279	0.316	0.264
Madhya Pradesh	0.292	0.277	0.242	0.265	0.292
Rajasthan	0.340	0.260	0.209	0.246	0.225
Gujarat	0.252	0.236	0.234	0.269	0.253
Karnataka	0.299	0.266	0.241	0.263	0.235
Orissa	0.266	0.243	0.244	0.281	0.262
Kerala	0.330	0.288	0.270	0.341	0.417
Assam	0.192	0.176	0.201	0.195	0.244
Jharkhand				0.225	0.240
Haryana	0.271	0.301	0.239	0.322	0.301
Punjab	0.279	0.265	0.239	0.279	0.288
Chhattisgarh				0.295	0.276
India	0.297	0.282	0.260	0.300	0.291

Source: Planning Commission website (accessed 4 February 2013).

This is hardly surprising since rapid growth, which can produce increased inequality, is concentrated in urban areas. In the Indian case, a dualism of sorts exists within urban areas. Output growth has been concentrated in the formal sector, while employment has been disproportionately concentrated in the informal sector. Unlike the Republic of Korea and Taipei,China in the 1960s and 1970s and the People's

Table 18. **The Gini Coefficient in Urban Areas**

State	1983	1993–1994	1999–2000	2004–2005	2009–2010
Uttar Pradesh	0.312	0.323	0.328	0.366	0.329
Maharashtra	0.329	0.351	0.348	0.372	0.410
Bihar	0.297	0.307	0.319	0.330	0.332
Andhra Pradesh	0.306	0.320	0.313	0.370	0.382
West Bengal	0.328	0.334	0.341	0.378	0.384
Tamil Nadu	0.347	0.344	0.381	0.356	0.332
Madhya Pradesh	0.290	0.327	0.315	0.393	0.364
Rajasthan	0.301	0.290	0.282	0.367	0.378
Gujarat	0.264	0.287	0.286	0.305	0.328
Karnataka	0.330	0.315	0.323	0.364	0.334
Orissa	0.294	0.304	0.292	0.350	0.389
Kerala	0.371	0.338	0.321	0.400	0.498
Assam	0.248	0.286	0.309	0.316	0.324
Jharkhand				0.351	0.358
Haryana	0.304	0.280	0.287	0.360	0.360
Punjab	0.321	0.276	0.290	0.393	0.371
Chhattisgarh				0.434	0.326
India	0.325	0.340	0.342	0.371	0.382

Source: Planning Commission website (accessed 4 February 2013).

Republic of China more recently, employment in the formal sector has not grown in India due to the poor performance of labor-intensive sectors. Growth in India has been concentrated in skilled labor and capital-intensive sectors.

The data do not support the hypothesis that high levels of poverty reflect high levels of inequality. At least in the Indian case, the two outcomes are at best unrelated and at worst negatively associated. For example, at the national level, rural inequality has remained more or less unchanged and urban inequality has risen, while both rural and urban poverty have steadily and significantly declined over time.

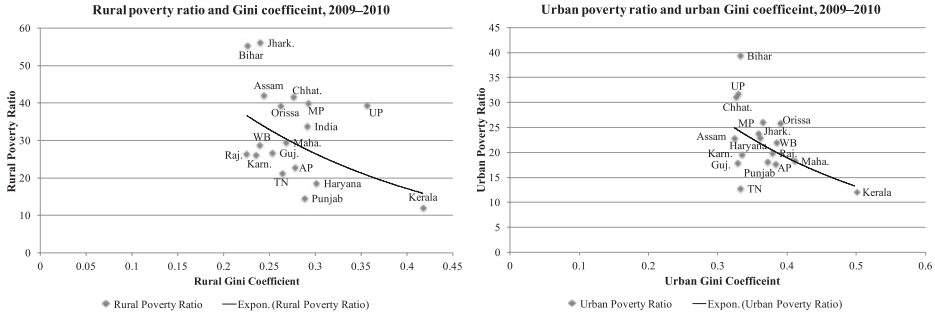
Looking at a cross section of the data, Kerala offers the most dramatic example. In 2009–2010, it had the lowest levels of rural and urban poverty and by far the highest rural and urban Gini coefficients. At the other extreme, Bihar had the second lowest rural Gini coefficient but the highest rural poverty ratio during the same period.

At a more aggregate level, the left panel in Figure 5 plots the rural Gini against the rural poverty ratio, while the right panel plots the urban Gini against the urban poverty ratio. The exponential trend line has a negative slope in each case, though the fit is poor. In other words, there is no evidence of a positive relationship between poverty and inequality, but there is some evidence of a negative relationship.

XI. Concluding Remarks

In this paper, we have provided a comprehensive analysis of poverty in India along six different dimensions: across time, across states, between rural and urban areas, across social and religious groups, and based on two different poverty lines

Figure 5. **Gini Coefficients and Poverty Ratios in Rural and Urban Areas in Indian States, 2009–2010**



Source: Authors' calculations.

(Lakdawala and Tendulkar). To keep the exposition manageable, we have concentrated on estimates based on the Tendulkar line except when we discuss poverty at the national level. In the latter case, we report estimates in rural and urban India derived from both the Lakdawala and Tendulkar lines. Our detailed estimates by social and religious groups, by rural and urban areas, and by state based on both the Lakdawala and Tendulkar lines are provided in the Appendix.

The following are some of the key conclusions of the paper. First, poverty has declined between 1993–1994 and 2009–2010 along every dimension. Indeed, poverty has fallen for every social and religious group in every state and in rural and urban areas, separately as well as jointly. Estimates based on the Lakdawala line show that the decline can be observed steadily since 1983 for all social and religious groups in all 17 large states.

Second, acceleration in growth rates between 2004–2005 and 2009–2010 has been accompanied by acceleration in poverty reduction. Poverty rates have fallen rapidly for all major social and religious groups at the national level. This phenomenon also holds true for most states across various social and religious groups.

Third, for the first time, poverty reduction between 2004–2005 and 2009–2010 has been larger for the scheduled castes and scheduled tribes than the upper caste groups. Thus, the gap in poverty rates between the socially disadvantaged and upper caste groups has narrowed over time. This pattern provides clear evidence to refute the claim that reforms and growth have failed to help the socially disadvantaged or that they are leaving these groups behind. A continuation of this trend, bolstered by further reforms and higher growth rates, would help eliminate the difference in poverty rates between the historically disadvantaged and the privileged.

Fourth, interstate comparisons reveal that the states with large scheduled castes and scheduled tribe populations face a steeper climb in combating poverty. The point is most forcefully brought out by a comparison of Punjab and Kerala. When we compare poverty rates in 2009–2010 by social group, the two states have very similar poverty rates. But because the poverty rates for the scheduled castes

are higher than those for the nonscheduled castes in both states and the scheduled castes account for a much larger proportion of the population, the aggregate poverty rate in Punjab turns out to be significantly higher.

Finally, we find that in the case of India, there is no robust relationship between inequality and poverty. Indeed, to the extent that such a relationship exists, this would suggest that more unequal states enjoy lower levels of poverty. Kerala offers the most dramatic example. It has had one of the highest Gini coefficients for rural as well as urban areas and also one of the lowest poverty ratios for both regions. In 2009–2010, its Gini coefficients were by far the highest among the large states in both rural and urban areas, while its poverty ratios were the smallest.

Given space limitations, we have deliberately limited ourselves to providing one specific indicator of poverty—the headcount ratio—in different states and for different social and religious groups based on the two official poverty lines. There are at least two broad complementary directions in which the work in this paper can be extended.

First, it may be desirable for certain purposes to estimate alternative indicators of poverty such as the poverty gap or its close cousin, the Foster-Greer-Thorbecke index. Such an index allows one to gauge the resources needed to bring all those below the poverty line to a level above it. In a similar vein, we have focused on progress in combating poverty among social and religious groups that are more vulnerable. Alternatively, we could focus on a different dimension of vulnerability such as male-headed versus female-headed households and evaluate the progress in combating poverty among female-headed households.

The second direction in which the work of this paper could be extended is towards explaining the determinants of poverty. Within this broad category, we have left many questions unanswered. For instance, it would be useful to separate the contributions of growth and redistribution policies in explaining the decline in poverty. Likewise, we may want to know what role, if any, rural-to-urban migration may have played—directly as well as through remittances. Similarly, we might ask what role the division of population among various social and religious groups plays in determining the progress in combating poverty. Finally, we might also wish to study the role that education plays in bringing down poverty. The recent work by Hnatkovska and Lahiri (2012) shows that education has indeed been pivotal in bridging the wage gap between scheduled castes and scheduled tribes on the one hand and nonscheduled castes on the other. This suggests an important role for education in eradicating poverty.

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Appendix A

Table A1. **Lakdawala Poverty Lines (Rs)**

	1983		1987–1988		1993–1994		1999–2000		2004–2005		2009–2010[a]	
State	**Rural**	**Urban**	**Rural**	**Urban**	**Rural**	**Urban**	**Rural**	**Urban**	**Rural**	**Urban**	**Rural**	**Urban**
Andhra Pradesh	72.66	106.43	91.94	151.88	163.02	278.14	262.94	457.40	292.95	542.89	468.93	893.06
Assam	98.32	97.51	127.44	126.60	232.05	212.42	365.43	343.99	387.64	378.84	560.94	549.92
Bihar	97.48	111.80	120.36	150.25	212.16	238.49	333.07	379.78	354.36	435	536.00	640.95
Chhattisgarh									322.41	560	498.91	879.41
Delhi	88.57	123.29	122.90	176.91	233.79	309.48	362.68	505.45	410.38	612.91	566.84	992.44
Gujarat	83.29	123.22	115.00	173.18	202.11	297.22	318.94	474.41	353.93	541.16	512.22	781.06
Haryana	88.57	103.48	122.90	143.22	233.79	258.23	362.81	420.20	414.76	504.49	620.16	785.56
Himachal Pradesh	88.57	102.26	122.90	144.10	233.79	253.61	367.45	420.20	394.28	504.49	536.41	739.82
Jharkhand									366.56	451.24	558.09	705.88
Karnataka	83.31	120.19	104.46	171.18	186.63	302.89	309.59	511.44	324.17	599.66	488.30	925.91
Kerala	99.35	122.64	130.61	163.29	243.84	280.54	374.79	477.06	430.12	559.39	620.63	794.74
Madhya Pradesh	83.59	122.82	107.00	178.35	193.10	317.16	311.34	481.65	327.78	570.15	507.15	826.64
Maharashtra	88.24	126.47	115.61	189.17	194.94	328.56	318.63	539.71	362.25	665.90	555.60	1,012.89
Orissa	106.28	124.81	121.42	165.40	194.03	298.22	323.92	473.12	325.79	528.49	453.08	782.15
Punjab	88.57	101.03	122.90	144.98	233.79	253.61	362.68	388.15	410.38	466.16	626.70	697.09
Rajasthan	80.24	113.55	117.52	165.38	215.89	280.85	344.03	465.92	374.57	559.63	591.63	833.31
Tamil Nadu	96.15	120.30	118.23	165.82	196.53	296.63	307.64	475.60	351.86	547.42	509.04	783.13
Uttar Pradesh	83.85	110.23	114.57	154.15	213.01	258.65	336.88	416.29	365.84	483.26	558.00	726.45
Uttaranchal									478.02	637.67	707.34	951.23
West Bengal	105.55	105.91	129.21	149.96	220.74	247.53	350.17	409.22	382.82	449.32	552.85	651.88
All India	89.50	115.65	115.20	162.16	205.84	281.35	327.56	454.11	356.30	538.60		

[a]Calculated by adjusting the 2004–2005 lines using the index implicit in the official Tendulkar lines for 2004–2005 and 2009–2010.
Source: Planning Commission, Government of India, Data Tables.

Table A2. **Tendulkar Poverty Lines (Rs)**

	1993–1994		2004–2005		2009–2010	
State	**Rural**	**Urban**	**Rural**	**Urban**	**Rural**	**Urban**
Andhra Pradesh	244.1	282	433.43	563.16	693.8	926.4
Assam	266.3	306.8	478.00	600.03	691.7	871.0
Bihar	236.1	266.9	433.43	526.18	655.6	775.3
Chhattisgarh	229.1	283.5	398.92	513.7	617.3	806.7
Delhi	315.4	320.3	541.39	642.47	747.8	1,040.3
Gujarat	279.4	320.7	501.58	659.18	725.9	951.4
Haryana	294.1	312.1	529.42	626.41	791.6	975.4
Himachal Pradesh	272.7	316	520.4	605.74	708	888.3
Jharkhand	227.7	304.1	404.79	531.35	616.3	831.2
Karnataka	266.9	294.8	417.84	588.06	629.4	908.0
Kerala	286.5	289.2	537.31	584.7	775.3	830.7
Madhya Pradesh	232.5	274.5	408.41	532.26	631.9	771.7
Maharashtra	268.6	329.0	484.89	631.85	743.7	961.1
Orissa	224.2	279.3	407.78	497.31	567.1	736.0
Punjab	286.9	342.3	543.51	642.51	830.0	960.8
Rajasthan	271.9	300.5	478.00	568.15	755.0	846.0
Tamil Nadu	252.6	288.2	441.69	559.77	639.0	800.8
Uttar Pradesh	244.3	281.3	435.14	532.12	663.7	799.9
Uttaranchal	249.5	306.7	486.24	602.39	719.5	898.6
West Bengal	235.5	295.2	445.38	572.51	643.2	830.6
All India			446.68	578.8	672.8	859.6

Source: Planning Commission, Government of India, Data Tables.

Appendix B

Table B1. **Rural Poverty by State and by Social Group Based on the Lakdawala Lines Using URP Expenditures: Scheduled Castes, Scheduled Tribes, and All Groups (%)**

	ST					SC					All Groups				
State	1983	1987–1988	1993–1994	2004–2005	2009–2010	1983	1987–1988	1993–1994	2004–2005	2009–2010	1983	1987–1988	1993–1994	2004–2005	2009–2010
Andhra Pradesh	35.73	39.56	26.4	28.3	19.5	36.72	28.49	26.0	15.5	9.0	26.77	21.03	15.9	10.5	7.8
Assam	48.60	45.66	41.9	12.6	16.0	43.86	34.71	45.3	25.7	20.1	43.32	39.42	45.2	22.1	20.2
Bihar	74.61	61.44	69.3	56.2	39.9	81.56	70.57	70.6	64.2	53.0	64.94	53.91	58.0	42.6	36.4
Chhattisgarh				54.8	42.0				32.0	46.8				40.8	37.9
Delhi[a]						9.32	7.68	12.4	0.0	0.0	6.99	1.28	2.0	6.9	0.0
Gujarat	56.59	43.52	30.5	34.3	17.4	37.07	35.91	32.9	22.8	11.4	29.41	28.32	22.2	18.9	8.4
Haryana	0.00	3.31	41.5	0.0	6.2	37.40	30.72	46.3	26.0	21.6	22.42	15.34	28.3	13.2	9.8
Himachal Prad.	11.00	10.94	64.9	15.7	15.3	28.57	20.39	37.1	19.9	5.2	17.79	16.68	30.4	10.5	3.4
Jharkhand				54.1	43.4				57.5	40.1				46.2	35.6
Karnataka	56.93	37.31	38.7	21.4	13.7	54.20	54.80	46.1	31.3	17.1	36.21	32.63	30.1	20.7	13.5
Kerala	42.80	35.38	37.4	40.1	22.2	63.51	38.01	37.6	21.6	17.7	39.75	29.27	25.4	13.2	7.1
Madhya Pradesh	66.98	61.81	57.0	58.4	43.5	58.80	47.97	45.3	43.3	25.7	49.68	42.02	40.7	36.8	27.6
Maharashtra	62.55	54.22	51.8	56.3	23.6	60.19	54.33	51.4	44.8	20.6	45.95	40.91	37.9	29.6	14.1
Orissa	87.08	83.82	71.3	75.8	54.4	76.08	65.53	49.8	49.9	29.5	68.43	58.63	49.8	46.9	27.5
Punjab	16.18	22.92	25.9	30.7	0.0	27.50	26.37	22.1	14.5	7.1	14.38	12.80	11.7	9.0	3.8
Rajasthan	63.46	57.10	45.7	32.5	16.1	44.98	35.80	38.1	28.3	22.9	38.58	33.30	26.4	18.3	11.7
Tamil Nadu	70.98	56.14	45.9	27.2	8.6	69.14	63.88	44.4	30.4	15.0	56.73	44.50	32.9	23.0	10.7
Uttar Pradesh	44.34	40.98	35.6	32.2	44.0	58.15	57.82	59.4	44.7	38.9	47.15	40.27	42.3	33.3	27.5
Uttarakhand				44.5	39.6				53.3	24.7				40.6	18.2
West Bengal	76.71	63.21	62.1	42.7	22.6	73.30	58.06	46.3	28.9	21.6	63.80	48.83	41.2	28.4	19.7
Total	64.88	57.77	51.6	47.0	30.5	58.97	50.07	48.4	37.2	27.8	46.60	38.70	37.0	28.2	20.2

SC = scheduled castes, ST = scheduled tribes, URP = uniform reference period.

[a]Delhi is 95% urban. The SC and ST estimates in this case are based on too few households and therefore subject to substantial sampling errors.

Source: Authors' calculations.

Table B2. **Rural Poverty by State and by Social Group Based on the Lakdawala Lines Using URP Expenditures: Nonscheduled Castes, Other Backward Castes, and Forward Castes (%)**

	NC				OBC		FC	
State	**1983**	**1987–1988**	**1993–1994**	**2004–2005**	**2009–2010**	**2004–2005**	**2009–2010**	**2004–2005**	**2009–2010**
Andhra Pradesh	23.51	17.63	11.7	7.0	6.5	8.6	8.0	3.8	2.6
Assam	42.02	38.68	45.9	24.1	21.2	18.1	12.7	18.9	26.1
Bihar	59.90	49.13	52.7	36.0	30.9	38.5	35.8	49.1	14.3
Chhattisgarh				33.5	32.4	34.1	30.3	28.3	42.3
Delhi[a]	6.68	0.00	0.0	8.1	0.0	0.0	0.0	10.6	0.0
Gujarat	19.96	22.61	17.3	13.6	4.2	18.5	5.9	4.5	0.9
Haryana	17.70	10.47	21.0	8.3	4.8	13.7	7.5	3.9	2.6
Himachal Pradesh	14.33	15.62	26.1	6.4	1.6	8.8	3.1	5.7	1.1
Jharkhand				39.4	28.4	40.0	30.7	36.9	19.5
Karnataka	31.06	27.76	24.4	17.6	12.3	20.8	15.4	13.7	5.9
Kerala	36.47	27.91	23.8	11.5	5.6	13.6	7.1	7.1	2.3
Madhya Pradesh	36.67	29.43	30.1	24.7	20.8	29.3	22.1	13.2	16.7
Maharashtra	41.27	36.96	32.1	21.3	10.7	24.1	12.5	18.6	8.7
Orissa	58.50	47.42	40.2	32.9	15.7	37.1	15.2	11.8	16.5
Punjab	9.02	5.56	4.8	5.2	1.1	10.5	2.8	2.3	0.4
Rajasthan	31.63	26.54	18.2	11.4	6.5	12.6	7.4	8.0	3.2
Tamil Nadu	52.79	37.99	28.5	20.2	9.3	20.2	9.5	18.8	0.0
Uttar Pradesh	44.04	34.82	36.9	29.4	22.8	32.9	26.6	32.4	12.5
Uttarakhand				36.2	14.9	44.4	13.9	33.5	15.1
West Bengal	58.27	42.69	35.6	26.3	18.5	17.7	17.7	32.6	18.6
Total	40.96	32.78	31.3	22.8	16.2	25.9	18.7	17.5	11.6

FC = forward castes, NC = nonscheduled castes, OBC = other backward castes.

[a]Only 5% of Delhi by population is rural. SC and ST estimates in this case are based on too few households and therefore subject to substantial sampling errors.

Source: Authors' calculations.

Table B3. **Urban Poverty by State and by Social Group Based on the Lakdawala Lines Using URP Expenditures: Scheduled Castes, Scheduled Tribes, and All Groups (%)**

	ST					SC					All Groups				
State	1983	1987–1988	1993–1994	2004–2005	2009–2010	1983	1987–1988	1993–1994	2004–2005	2009–2010	1983	1987–1988	1993–1994	2004–2005	2009–2010
Andhra Pradesh	43.0	51.8	45.6	51.9	24.9	52.1	49.7	45.8	37.4	20.7	38.0	41.1	38.8	27.4	19.7
Assam	18.7	4.4	8.3	2.9	15.1	43.7	20.9	16.5	5.1	5.4	22.1	11.3	7.9	3.6	6.7
Bihar	51.2	54.6	35.0	57.2	11.4	64.6	62.5	57.0	66.9	45.2	48.9	51.9	34.8	36.1	29.3
Chhattisgarh				42.1	32.5				52.7	43.3				42.2	34.1
Delhi	5.4	11.0	9.1	0.0	68.9	53.0	47.6	48.9	40.5	36.4	28.6	15.5	16.1	16.3	17.7
Gujarat	83.2	64.0	35.6	21.0	13.4	43.8	50.0	45.9	17.8	23.3	41.3	38.5	28.3	13.3	11.4
Haryana	20.1	20.1	0.0	0.0	66.2	48.5	41.2	25.3	33.3	19.9	28.1	18.4	16.5	14.5	11.6
Himachal Prad.	20.4	0.0	0.0	2.4	18.6	23.7	18.4	20.1	5.0	15.0	12.6	7.2	9.3	3.2	7.9
Jharkhand				42.5	36.9				48.8	37.8				20.3	25.2
Karnataka	51.6	69.9	62.7	61.9	36.9	50.6	62.6	62.8	50.3	36.9	43.0	49.2	39.9	32.6	25.7
Kerala	59.5	30.6	0.0	21.8	18.3	60.1	58.0	33.4	33.4	26.1	45.5	40.0	24.3	20.0	14.1
Madhya Pradesh	54.8	66.8	66.4	44.7	45.8	68.4	69.9	63.9	68.4	47.3	53.7	47.2	48.1	42.7	30.5
Maharashtra	67.0	64.1	60.5	40.9	35.7	66.0	61.2	53.8	42.8	38.2	41.0	40.3	35.0	32.1	23.7
Orissa	73.7	61.4	62.8	64.6	51.2	69.8	59.5	45.5	74.5	51.8	49.7	42.6	40.6	44.7	33.3
Punjab	56.3	18.7	0.0	2.4	0.7	36.1	26.2	26.9	14.3	13.8	23.5	13.7	10.9	6.3	7.3
Rajasthan	50.6	27.9	8.4	24.9	24.4	49.1	54.6	49.7	55.1	34.0	38.4	37.9	31.0	32.3	21.9
Tamil Nadu	74.8	51.8	25.0	33.1	20.5	69.6	63.3	61.5	41.2	28.4	50.8	40.2	39.9	22.5	15.3
Uttar Pradesh	33.4	49.8	27.9	37.6	18.7	57.8	57.1	59.0	43.5	36.4	51.1	44.9	35.1	30.1	27.8
Uttarakhand				69.0	0.0				70.1	38.0				36.5	32.9
West Bengal	42.4	43.3	23.5	22.2	12.1	48.9	49.8	38.7	25.5	21.8	33.4	33.8	22.9	13.5	11.4
Total	58.3	56.2	46.6	39.0	31.7	56.2	54.6	51.2	41.1	31.5	42.5	39.4	33.1	26.1	20.7

SC = scheduled castes, ST = scheduled tribes, URP = uniform reference period.
Source: Authors' calculations.

Table B4. **Urban Poverty by State and by Social Group Based on the Lakdawala Lines Using URP Expenditures: Nonscheduled Castes, Other Backward Castes, and Forward Castes (%)**

	NS					OBC		FC	
State	1983	1987–1988	1993–1994	2004–2005	2009–2010	2004–2005	2009–2010	2004–2005	2009–2010
Andhra Pradesh	36.4	39.7	37.9	24.8	19.4	28.7	22.7	20.2	16.1
Assam	19.0	10.2	7.3	3.5	6.4	5.4	3.8	1.4	7.1
Bihar	46.4	50.1	31.4	32.1	27.0	40.3	34.1	8.6	8.9
Chhattisgarh				40.3	32.3	53.9	41.9	22.3	22.3
Delhi	21.2	9.1	8.3	8.3	11.9	20.3	22.1	6.3	8.2
Gujarat	39.1	34.9	25.6	12.5	10.0	23.8	19.3	6.9	5.0
Haryana	24.7	13.2	14.6	10.3	8.0	20.5	14.2	5.7	4.1
Himachal Pradesh	9.4	3.2	6.9	2.8	5.0	9.8	22.0	1.8	3.2
Jharkhand				13.0	20.9	17.4	33.6	8.2	7.0
Karnataka	41.8	47.0	35.7	29.0	23.7	38.2	23.9	21.0	23.4
Kerala	44.3	39.0	23.9	18.8	13.3	24.0	16.6	7.2	5.0
Madhya Pradesh	50.9	42.0	42.8	37.7	26.3	56.2	37.3	21.3	14.5
Maharashtra	37.5	36.9	30.6	29.5	20.5	35.6	29.7	63.4	16.3
Orissa	41.8	37.9	36.3	37.1	23.7	48.6	30.0	29.7	20.5
Punjab	19.6	10.6	6.3	3.3	4.9	5.7	10.8	2.5	2.9
Rajasthan	36.3	34.7	27.9	26.4	18.9	32.1	30.0	20.9	7.5
Tamil Nadu	48.4	37.1	36.6	19.2	13.1	20.8	14.2	7.0	1.0
Uttar Pradesh	50.2	43.2	31.3	28.0	26.5	36.0	36.6	19.0	15.5
Uttarakhand				29.3	32.5	43.9	55.8	25.1	19.3
West Bengal	30.6	31.1	19.7	10.3	8.6	7.4	11.7	5.2	8.3
Total	40.1	36.6	29.6	22.8	18.2	31.3	25.1	16.2	12.1

FC = forward castes, NS = nonscheduled castes, OBC = other backward castes, URP = uniform reference period.
Source: Authors' calculations.

Table B5. **Rural + Urban Poverty by State and by Social Group Based on the Lakdawala Lines Using URP Expenditures: Scheduled Castes, Scheduled Tribes, and All Groups (%)**

	ST					SC					All Groups				
State	**1983**	**1987–1988**	**1993–1994**	**2004–2005**	**2009–2010**	**1983**	**1987–1988**	**1993–1994**	**2004–2005**	**2009–2010**	**1983**	**1987–1988**	**1993–1994**	**2004–2005**	**2009–2010**
Andhra Pradesh	36.4	40.5	28.3	30.5	20.2	38.6	31.8	28.6	20.1	11.3	29.2	25.4	21.9	14.8	11.1
Assam	48.5	44.7	40.9	12.3	15.9	43.8	32.9	43.1	23.2	17.8	41.5	37.1	41.4	20.4	18.8
Bihar	73.2	61.0	66.6	56.3	38.5	79.9	69.7	69.5	64.3	52.7	62.9	53.7	55.2	42.0	35.7
Chhattisgarh				53.8	41.5				34.7	46.1				41.0	37.2
Delhi	3.7	9.5	8.1	0.0	67.1	49.5	42.8	45.5	38.9	32.5	27.5	13.8	14.6	15.7	16.8
Gujarat	58.4	45.7	30.9	33.1	17.1	39.3	39.7	36.9	21.3	15.4	32.9	31.1	24.1	17.0	9.6
Haryana	6.8	6.9	39.2	0.0	19.4	39.0	32.5	42.4	27.4	21.1	23.7	16.0	25.2	13.6	10.3
Himachal Pradesh	11.7	10.6	62.3	15.0	15.4	28.3	20.3	35.9	18.9	6.0	17.4	16.0	28.6	9.8	3.8
Jharkhand				53.4	42.9				56.2	39.7				42.0	33.5
Karnataka	56.4	43.4	41.8	26.5	18.3	53.3	56.3	49.3	35.5	21.0	38.1	37.4	32.9	24.3	17.8
Kerala	44.2	34.8	32.3	38.8	21.5	63.1	39.8	37.0	23.8	19.1	40.8	31.1	25.1	14.8	8.9
Madhya Pradesh	66.5	62.1	57.6	57.5	43.7	60.7	51.2	49.7	48.3	30.0	50.4	43.0	42.4	38.2	28.3
Maharashtra	63.1	55.8	53.1	54.3	25.6	61.9	56.4	52.3	43.9	27.6	44.3	40.7	36.8	30.6	18.1
Orissa	86.2	82.3	70.8	75.2	54.0	75.5	65.2	49.4	52.6	32.3	66.2	56.8	48.6	46.6	28.3
Punjab	26.1	21.5	22.3	18.7	0.4	29.2	26.3	23.1	14.4	8.8	16.7	13.0	11.5	8.1	5.0
Rajasthan	63.0	55.6	44.5	32.2	16.8	45.6	39.1	40.5	34.2	25.4	38.6	34.2	27.5	21.4	14.2
Tamil Nadu	72.6	54.8	39.6	29.7	13.6	69.2	63.7	48.4	33.1	19.2	54.7	42.3	35.4	22.8	12.7
Uttar Pradesh	43.3	44.7	34.5	33.2	35.7	58.1	57.7	59.3	44.6	38.7	47.8	41.6	40.9	32.7	27.5
Uttarakhand				46.0	36.7				56.3	27.2				39.7	22.0
West Bengal	73.9	61.6	59.8	41.7	21.5	70.0	57.0	45.3	28.2	21.6	56.6	45.2	36.9	24.7	17.7
Total	64.4	57.6	51.2	46.3	30.7	58.5	50.9	48.9	38.0	28.6	45.7	38.9	36.0	27.7	20.3

SC = scheduled castes, ST = scheduled tribes, URP = uniform reference period.
Source: Authors' calculations.

Table B6. **Rural + Urban Poverty by State and by Social Group Based on the Lakdawala Lines Using URP Expenditures:Nonscheduled Castes, Other Backward Castes, and Forward Castes (%)**

	NS					OBC		FC	
State	1983	1987–1988	1993–1994	2004–2005	2009–2010	2004–2005	2009–2010	2004–2005	2009–2010
Andhra Pradesh	26.7	23.0	19.7	12.0	10.5	13.5	11.5	9.5	8.6
Assam	39.8	36.2	41.3	22.0	19.6	16.9	12.1	23.6	23.8
Bihar	58.0	49.3	49.7	35.6	30.5	38.7	35.7	25.2	13.6
Chhattisgarh				34.9	32.3	36.9	32.2	25.3	32.6
Delhi	20.7	8.0	7.4	8.3	11.4	17.8	20.7	6.6	8.0
Gujarat	26.5	26.4	20.5	13.2	6.9	19.8	10.1	5.8	3.5
Haryana	19.4	11.1	19.2	8.9	5.8	15.2	9.4	4.5	3.1
Himachal Pradesh	14.0	14.8	24.3	6.0	1.9	8.8	3.9	5.2	1.3
Jharkhand				33.9	26.3	36.7	31.3	25.9	13.6
Karnataka	34.3	33.8	28.1	21.5	16.9	26.2	18.4	16.5	14.6
Kerala	37.9	29.9	23.8	13.3	7.7	16.1	9.7	7.1	3.0
Madhya Pradesh	40.3	32.7	33.9	28.7	22.5	35.3	25.8	16.8	15.6
Maharashtra	39.9	36.9	31.5	24.8	15.2	27.7	18.1	22.8	12.9
Orissa	56.1	46.0	39.6	33.7	17.0	38.3	16.7	25.3	17.5
Punjab	11.9	7.1	5.3	4.5	2.7	9.1	5.9	2.4	1.5
Rajasthan	32.7	28.5	20.8	15.3	10.0	16.2	12.1	13.2	5.2
Tamil Nadu	51.1	37.5	31.6	19.8	11.1	20.5	11.6	9.7	0.9
Uttar Pradesh	45.3	37.5	35.7	29.1	23.6	33.4	28.3	19.4	13.6
Uttarakhand				34.4	19.9	44.3	32.2	31.3	16.1
West Bengal	50.3	39.2	30.9	21.7	15.8	15.9	16.5	22.3	15.7
Total	40.8	33.9	30.8	22.8	16.8	27.1	20.3	17.0	11.8

FC = forward castes, NS = nonscheduled castes, OBC = other backward castes, URP = uniform reference period.
Source: Authors' calculations.

Table B7. **Rural Poverty by State and by Social Group Based on the Tendulkar Line Using MRP Expenditures: Scheduled Castes, Scheduled Tribes, and All Groups (%)**

	ST			SC			All Groups		
State	1993–1994	2004–2005	2009–2010	1993–1994	2004–2005	2009–2010	1993–1994	2004–2005	2009–2010
Andhra Pradesh	58.1	60.3	40.2	64.2	41.8	25.7	48.0	32.3	22.7
Assam	55.3	28.8	32.0	58.4	45.3	36.9	55.0	36.3	39.9
Bihar	73.3	59.3	64.4	76.0	77.6	68.1	62.3	55.7	55.2
Chhattisgarh	65.9	65.5	66.8	53.4	48.6	67.6	55.9	55.1	56.1
Delhi	0.0		0.0	27.9	0.0	0.0	16.2	15.6	7.6
Gujarat	53.1	57.1	48.6	56.3	49.3	17.9	43.1	39.1	26.6
Haryana	69.7	0.0	49.6	63.1	47.5	33.6	39.9	24.8	18.6
Himachal Pradesh	62.4	35.4	22.0	43.6	39.4	14.4	36.7	25.0	9.1
Jharkhand	72.6	60.6	51.0	73.7	61.0	44.1	65.7	51.6	41.4
Karnataka	70.3	50.5	21.3	72.4	57.4	35.6	56.4	37.4	26.2
Kerala	40.9	56.9	24.4	53.3	30.8	27.7	33.8	20.2	12.0
Madhya Pradesh	69.8	80.0	61.9	59.3	62.5	42.4	48.8	53.6	42.0
Maharashtra	74.2	73.2	51.7	73.8	66.1	37.6	59.2	47.8	29.5
Orissa	82.1	84.4	66.0	62.8	67.9	47.1	63.0	60.7	39.2
Punjab	35.9	30.7	16.1	34.6	38.4	27.2	20.1	22.1	14.6
Rajasthan	63.7	59.3	35.9	55.3	48.5	38.6	40.7	35.9	26.4
Tamil Nadu	57.0	47.3	11.5	66.3	51.2	31.2	51.0	37.6	21.2
Uttar Pradesh	49.6	42.0	49.8	68.6	56.6	53.6	50.9	42.7	39.4
Uttarakhand	54.9	32.4	20.0	43.5	46.2	20.0	36.7	35.1	13.7
West Bengal	66.5	54.3	32.9	48.2	37.1	31.5	42.4	38.3	28.8
Total	65.7	64.5	47.4	62.1	53.6	42.3	50.1	41.9	33.3

MRP = mixed reference period, SC = scheduled castes, ST = scheduled tribes.
Source: Authors' calculations

Table B8. **Rural Poverty by State and by Social Group Based on the Tendulkar Line Using MRP Expenditures: Nonscheduled Castes, Other Backward Castes, and Forward Castes (%)**

	NS			OBC		FC	
State	1993–1994	2004–2005	2009–2010	2004–2005	2009–2010	2004–2005	2009–2010
Andhra Pradesh	42.4	26.4	20.4	31.6	24.3	16.1	10.3
Assam	54.5	37.1	42.2	31.9	31.0	38.9	48.7
Bihar	57.7	49.1	50.8	52.6	56.4	36.1	32.3
Chhattisgarh	48.2	49.6	45.4	51.0	45.6	38.7	44.3
Delhi	14.4	18.3	13.7	27.0	28.5	15.5	0.0
Gujarat	37.2	32.1	19.1	41.7	27.2	13.7	3.1
Haryana	30.1	16.1	11.8	25.7	19.0	8.2	5.9
Himachal Pradesh	33.0	18.4	5.7	19.0	8.3	18.3	4.9
Jharkhand	59.6	44.8	33.6	46.7	35.7	37.4	25.3
Karnataka	50.0	30.3	23.8	35.8	27.2	23.7	16.5
Kerala	31.5	18.0	10.0	21.3	11.6	10.8	6.5
Madhya Pradesh	35.9	38.5	32.4	44.7	32.9	22.9	30.9
Maharashtra	53.0	39.3	23.4	44.6	26.6	34.0	19.7
Orissa	54.6	47.8	25.2	52.6	25.6	37.3	24.5
Punjab	10.7	11.1	4.3	21.7	11.4	5.1	1.5
Rajasthan	30.7	25.7	19.5	27.2	21.1	21.1	13.7
Tamil Nadu	45.4	32.4	18.1	32.6	17.9	22.2	32.9
Uttar Pradesh	45.2	37.9	33.7	42.2	38.2	26.1	21.5
Uttarakhand	33.4	31.8	11.5	43.5	8.0	27.9	12.3
West Bengal	36.0	36.8	27.1	28.3	26.3	37.7	27.3
Total	43.8	35.1	28.0	39.9	31.9	27.1	21.0

FC = forward castes, MRP = mixed reference period, NS = nonscheduled castes, OBC = other backward castes.
Source: Authors' calculations.

Table B9. **Urban Poverty by State and by Social Groups Based on the Tendulkar Line Using MRP Expenditures: Scheduled Castes, Scheduled Tribes, and All Groups (%)**

	ST			SC			All Groups		
State	1993–1994	2004–2005	2009–2010	1993–1994	2004–2005	2009–2010	1993–1994	2004–2005	2009–2010
Andhra Pradesh	43.9	50.1	21.2	45.6	35.0	19.8	35.1	23.4	17.7
Assam	17.0	29.8	29.2	49.7	37.2	34.9	27.7	21.8	25.9
Bihar	43.1	57.2	16.5	66.5	71.2	61.0	44.6	43.7	39.4
Chhattisgarh	18.6	32.7	28.6	48.5	44.6	29.7	28.1	28.4	23.6
Delhi	9.1	0.0	67.9	48.8	26.2	33.7	15.7	12.9	14.3
Gujarat	31.0	31.2	32.2	49.3	18.7	29.4	28.0	20.1	17.6
Haryana	0.0	22.2	85.0	41.8	46.9	48.3	24.2	22.4	23.0
Himachal Pradesh	0.0	2.4	19.6	26.9	9.2	20.4	13.6	4.6	12.5
Jharkhand	56.1	47.2	49.5	67.9	52.6	40.5	41.8	23.8	31.0
Karnataka	56.9	55.7	35.6	55.4	41.2	29.5	34.2	25.9	19.5
Kerala	0.0	21.8	5.0	34.7	33.0	25.8	23.7	18.4	12.1
Madhya Pradesh	51.2	42.6	41.6	45.1	59.6	39.2	31.7	35.1	22.8
Maharashtra	56.1	34.8	32.4	48.2	36.0	30.4	30.2	25.6	18.3
Orissa	56.5	53.4	34.1	39.0	63.7	47.1	34.3	37.6	25.9
Punjab	42.1	2.4	15.0	50.6	36.2	35.3	27.2	18.7	18.0
Rajasthan	12.6	26.8	28.9	49.1	51.0	31.6	29.9	29.7	19.9
Tamil Nadu	25.4	34.7	17.6	56.5	40.7	23.4	33.5	19.8	12.7
Uttar Pradesh	27.9	40.3	20.2	63.8	44.2	42.2	38.2	34.1	31.7
Uttaranchal		39.0	0.0	0.0	47.5	28.1	18.7	26.2	25.0
West Bengal	28.1	48.0	20.6	50.1	40.9	38.2	31.2	24.4	21.9
Total	40.9	38.7	30.4	51.4	40.6	34.1	31.7	25.8	20.9

MRP = mixed reference period, SC = scheduled castes, ST = scheduled tribes.
Source: Authors' calculations.

Table B10. **Urban Poverty by State and by Social Group Based on the Tendulkar Line Using MRP Expenditures: Nonscheduled Castes, Other Backward Castes, and Forward Castes (%)**

	NS			OBC		FC	
State	**1993–1994**	**2004–2005**	**2009–2010**	**2004–2005**	**2009–2010**	**2004–2005**	**2009–2010**
Andhra Pradesh	33.9	20.4	17.2	23.8	19.7	16.5	14.7
Assam	26.5	18.5	23.5	26.7	19.7	15.9	24.6
Bihar	42.1	40.2	36.3	49.6	43.9	22.6	16.8
Chhattisgarh	25.5	24.5	21.8	32.5	31.3	14.0	12.0
Delhi	7.9	8.6	8.3	22.7	17.8	6.1	4.9
Gujarat	25.1	19.7	15.9	36.5	30.3	11.4	8.2
Haryana	20.5	16.8	12.8	36.5	20.9	8.1	7.7
Himachal Pradesh	10.7	3.5	9.5	10.8	22.0	2.5	8.2
Jharkhand	33.2	16.5	26.3	22.0	39.9	10.3	11.4
Karnataka	30.3	22.6	17.4	32.1	17.8	14.3	16.9
Kerala	23.2	17.0	11.3	21.2	14.0	7.9	4.3
Madhya Pradesh	26.7	29.8	18.5	46.9	25.8	14.6	10.7
Maharashtra	25.9	23.0	15.5	26.8	22.4	21.4	12.3
Orissa	29.9	31.1	18.0	42.4	26.0	23.8	14.0
Punjab	20.2	12.3	11.5	20.2	24.7	9.6	7.1
Rajasthan	26.6	24.0	16.5	31.3	25.9	17.0	7.0
Tamil Nadu	29.8	16.0	11.0	17.3	11.8	6.5	1.3
Uttar Pradesh	34.1	32.5	30.1	42.7	41.1	20.9	18.1
Uttaranchal	19.8	21.8	24.8	35.0	40.4	17.9	16.0
West Bengal	27.4	19.7	17.6	23.6	29.9	19.5	16.6
Total	28.1	22.6	18.0	30.8	24.3	16.2	12.4

FC = forward castes, MRP = mixed reference period, NS = nonscheduled castes, OBC = other backward castes.
Source: Authors' calculations.

Table B11. **Rural Poverty by State and by Religious Group Based on the Lakdawala Lines Using URP Expenditures: Hindus, Muslims, and All Groups (%)**

	Hindus					Muslims					Others				
State	1983	1987–1988	1993–1994	2004–2005	2009–2010	1983	1987–1988	1993–1994	2004–2005	2009–2010	1983	1987–1988	1993–1994	2004–2005	2009–2010
Andhra Pradesh	26.6	21.5	15.8	10.5	7.8	27.0	26.7	12.4	10.2	7.0	32.7	20.1	23.9	50.0	8.2
Assam	41.3	36.0	40.7	15.0	17.0	46.0	44.7	54.9	35.3	26.9	61.6	58.3	61.4	7.7	3.3
Bihar	65.2	53.8	56.3	41.0	37.3	64.2	57.3	67.0	52.1	32.1	53.1	55.3	64.9	39.0	12.1
Chhattisgarh				40.9	38.4				41.8	52.5				12.0	0.7
Delhi	4.2	2.0	2.2	7.2	0.0	19.4	0.0	0.0	0.0	0.0		0.0	0.0	0.0	
Gujarat	29.6	28.8	22.2	19.5	8.9	20.6	18.2	15.8	12.5	2.9	31.9	22.5	37.2	4.1	0.0
Haryana	23.3	15.5	26.3	12.8	9.6	15.6	28.2	52.8	29.9	12.2	15.0	5.7	32.6	9.1	3.8
Himachal Pradesh	18.0	17.5	30.7	10.6	3.4	6.6	4.4	35.7	7.5	0.0	23.8	12.5	11.0	23.1	7.0
Jharkhand				44.9	35.2				46.4	39.4				53.3	38.7
Karnataka	36.6	33.1	29.9	20.7	13.6	33.7	30.9	34.4	25.0	13.1	34.1	32.9	23.6	1.8	34.3
Kerala	40.3	28.5	24.5	13.6	6.7	49.2	41.3	32.0	17.1	10.6	30.5	16.3	20.7	2.4	2.9
Madhya Pradesh	50.1	43.6	41.2	37.0	28.5	37.7	38.3	27.8	35.0	4.4	25.5	42.2	26.6	1.2	24.2
Maharashtra	44.8	40.0	36.4	28.8	12.6	53.9	38.1	43.1	26.4	14.3	53.5	55.0	50.9	11.0	7.1
Orissa	68.6	58.6	49.6	46.4	26.9	62.9	38.6	40.7	26.3	21.0	65.2	81.7	67.6	37.4	62.3
Punjab	21.4	22.4	12.0	6.8	4.1	32.4	30.9	20.5	4.2	0.0	11.4	10.7	11.4	10.0	6.2
Rajasthan	38.8	34.5	26.3	18.5	11.2	45.6	33.9	32.2	16.1	26.4	15.9	5.1	15.4	8.2	0.3
Tamil Nadu	57.1	46.6	32.7	23.4	11.0	52.0	41.8	24.7	10.0	7.6	54.6	52.8	40.7	10.5	2.6
Uttar Pradesh	46.3	42.1	42.5	32.7	27.7	52.5	46.2	42.9	36.5	26.1	33.3	34.2	6.3	38.3	0.0
Uttarakhand				40.5	19.0				44.2	17.1				25.6	3.0
West Bengal	63.2	48.5	38.4	24.4	16.7	65.2	49.6	48.5	36.9	25.1	67.7	45.9	57.6	35.7	17.2
Total	47.0	40.0	36.6	28.0	20.4	51.3	44.1	45.1	33.0	21.7	30.3	26.6	27.9	19.4	11.7

URP = uniform reference period.
Source: Authors' calculations.

Table B12. **Urban Poverty by State and by Religious Group Based on the Lakdawala Lines Using URP Expenditures: Hindus, Muslims, and All Groups (%)**

	Hinduism					Islam					Others				
State	1983	1987–1988	1993–1994	2004–2005	2009–2010	1983	1987–1988	1993–1994	2004–2005	2009–2010	1983	1987–1988	1993–1994	2004–2005	2009–2010
Andhra Pradesh	37.0	39.1	37.2	25.9	17.9	41.1	53.8	49.7	39.5	27.8	21.9	29.0	26.3	11.1	3.4
Assam	22.0	13.2	6.2	3.1	4.5	24.0	12.2	22.2	6.1	20.0	24.9	17.2	0.0	20.0	2.3
Bihar	50.3	50.9	31.6	33.0	26.2	73.8	56.9	47.8	50.6	44.4	40.8	35.9	28.9	2.5	6.3
Chhattisgarh				42.6	33.2				61.1	48.7				7.3	23.5
Delhi	29.5	19.1	15.3	16.8	17.4	29.9	27.7	30.2	23.0	26.3	18.7	3.8	6.2	0.0	1.5
Gujarat	39.0	36.5	25.4	11.6	8.7	56.1	51.8	46.8	29.4	28.5	15.3	24.5	24.4	20.7	0.8
Haryana	29.4	20.7	15.6	15.0	10.3	28.8	0.0	39.8	9.5	35.6	14.7	9.6	23.2	0.4	20.0
Himachal Pradesh	12.1	9.1	9.9	3.6	8.5	0.0	0.0	0.0	1.3	0.0	20.5	0.0	0.0	0.0	0.0
Jharkhand				18.4	25.5				40.1	32.6				27.4	7.6
Karnataka	41.5	46.0	35.9	29.1	24.8	53.5	64.2	57.8	48.5	34.5	18.9	35.8	22.7	7.9	4.6
Kerala	42.0	37.3	24.6	20.0	15.0	62.1	49.8	26.8	28.2	20.6	37.0	33.0	21.3	9.5	2.0
Madhya Pradesh	50.2	47.0	47.2	40.1	30.3	69.5	58.8	59.7	61.3	36.6	51.0	44.2	31.3	6.3	0.8
Maharashtra	33.5	38.0	32.4	27.0	20.0	55.4	55.2	49.6	54.7	39.6	27.0	36.7	31.9	14.1	11.9
Orissa	49.7	42.5	39.6	44.2	31.9	49.3	69.0	64.1	47.0	61.6	62.6	38.3	24.4	42.8	0.0
Punjab	23.0	14.2	10.9	7.4	8.0	35.2	34.7	22.5	13.7	15.3	22.2	11.6	10.6	5.5	2.0
Rajasthan	37.4	36.9	27.7	31.2	20.3	47.4	49.1	55.7	44.2	30.7	21.8	23.4	13.9	3.8	15.9
Tamil Nadu	48.2	41.2	39.5	23.0	15.1	60.7	45.3	46.0	21.7	11.7	43.8	30.0	34.3	29.6	5.6
Uttar Pradesh	50.5	37.9	31.0	25.6	21.6	69.5	63.5	46.4	40.5	43.5	19.1	36.5	7.1	10.6	8.6
Uttarakhand				35.2	23.7				51.3	61.4				0.0	26.1
West Bengal	31.0	30.0	19.9	11.1	9.8	53.5	57.8	42.5	28.8	22.0	28.8	16.7	27.3	16.7	15.9
Total	38.8	37.5	31.0	23.8	18.5	55.1	55.0	47.8	40.7	33.7	28.6	27.9	23.4	12.6	10.9

URP = uniform reference period.
Source: Authors' calculations.

Table B13. **Rural Poverty by State and by Religious Group Based on the Tendulkar Line Using MRP Expenditures: Hindus, Muslims, and All Groups (%)**

	Hinduism			Islam			Others		
State	1993–1994	2004–2005	2009–2010	1993–1994	2004–2005	2009–2010	1993–1994	2004–2005	2009–2010
Andhra Pradesh	48.0	32.4	22.9	44.2	28.4	20.3	65.2	63.4	22.7
Assam	51.2	27.8	32.3	63.1	51.6	53.6	46.3	33.0	48.1
Bihar	60.7	54.8	56.0	71.1	61.1	51.6	45.5	46.9	51.7
Chhattisgarh	57.0	55.4	56.5	0.0	41.8	49.3	12.0	14.8	14.9
Delhi	17.7	16.3	7.6	0.0	0.0	0.0	0.0	0.0	
Gujarat	43.1	39.9	26.4	36.4	31.0	31.4	39.3	9.4	0.0
Haryana	38.0	24.7	18.1	63.6	44.2	29.7	64.0	15.8	30.0
Himachal Pradesh	36.6	24.8	9.1	46.6	34.3	15.7	50.8	26.2	7.0
Jharkhand	64.6	50.3	39.6	70.6	51.5	50.7	23.8	59.4	49.5
Karnataka	57.5	38.1	26.7	52.5	35.8	20.9	29.4	9.4	67.7
Kerala	33.2	20.8	11.9	41.8	26.5	14.6	15.6	22.6	8.5
Madhya Pradesh	49.1	54.1	42.8	42.4	44.2	22.0	38.9	25.1	34.6
Maharashtra	57.8	47.1	28.7	61.0	40.0	23.3	45.1	18.5	9.6
Orissa	62.8	60.4	38.4	52.5	27.9	45.1	58.7	43.8	73.2
Punjab	20.1	23.2	19.0	36.9	23.0	3.5	18.5	18.6	13.1
Rajasthan	40.8	36.3	26.4	45.2	31.3	34.6	10.1	12.8	9.9
Tamil Nadu	51.2	38.0	21.8	35.7	18.0	15.8	36.8	18.1	4.7
Uttar Pradesh	51.2	42.0	38.6	50.4	46.9	44.4	30.4	38.3	0.0
Uttarakhand	37.9	34.3	14.7	51.5	43.5	8.2	5.1	32.7	3.0
West Bengal	39.4	33.2	25.6	50.3	49.1	34.4	39.8	45.5	31.3
India	50.3	42.1	33.5	53.4	44.6	36.1	37.8	30.7	21.4

MRP = mixed reference period.
Source: Authors' calculations.

Table B14. **Urban Poverty by State and by Religious Group Based on the Tendulkar Line Using MRP Expenditures: Hindus, Muslims, and All Groups (%)**

	Hinduism			Islam			Others		
State	1993–1994	2004–2005	2009–2010	1993–1994	2004–2005	2009–2010	1993–1994	2004–2005	2009–2010
Andhra Pradesh	33.8	22.1	16.0	44.5	32.7	24.7	19.4	11.6	3.2
Assam	25.2	21.5	21.4	50.4	24.2	52.7	0.0	22.7	13.1
Bihar	38.7	40.1	35.9	59.2	60.8	56.5	14.6	2.5	6.3
Chhattisgarh	30.4	28.1	25.2	16.7	54.4	10.4	5.6	4.4	3.0
Delhi	15.2	12.8	14.9	33.1	21.7	14.1	0.0	2.7	1.5
Gujarat	25.7	17.7	13.8	45.6	42.3	42.4	12.4	20.8	2.1
Haryana	23.3	22.5	22.2	51.7	46.5	42.4	38.5	0.4	20.0
Himachal Pradesh	13.7	5.2	11.7	0.0	1.7	51.4	20.7	0.0	0.0
Jharkhand	40.4	21.7	30.6	55.0	49.8	44.3	18.5	29.5	9.8
Karnataka	30.7	23.0	19.9	50.6	40.3	20.4	13.1	2.2	6.5
Kerala	23.7	19.0	12.6	27.6	23.7	17.1	18.2	9.6	2.4
Madhya Pradesh	31.5	33.5	22.0	36.4	48.3	31.7	34.5	2.7	0.8
Maharashtra	27.5	20.1	15.2	44.0	47.9	30.9	14.5	12.0	10.2
Orissa	33.5	36.4	26.3	52.8	44.2	27.6	10.5	41.7	0.0
Punjab	27.5	20.5	17.3	50.8	40.5	23.7	23.7	20.9	7.6
Rajasthan	26.7	28.0	18.0	52.5	42.4	29.5	22.4	7.0	16.2
Tamil Nadu	33.3	20.1	12.6	35.4	19.1	11.2	29.6	29.3	4.3
Uttar Pradesh	33.4	27.5	24.7	50.7	48.4	49.5	23.1	32.3	8.5
Uttarakhand	18.5	24.2	17.1	32.5	44.3	49.4	0.0	0.0	26.1
West Bengal	27.3	20.9	20.0	56.1	45.7	34.9	20.6	22.1	15.9
Total	29.5	23.1	18.7	46.4	41.9	33.9	22.8	13.5	12.9

MRP = mixed reference period.
Source: Authors' calculations.

Foreign Direct Investment and the Survival of Domestic Private Firms in Viet Nam

Ari Kokko and Tran Toan Thang*

Foreign direct investment (FDI) may benefit local firms in the host country through various kinds of spillovers, but it may also raise competition and result in the crowding out of domestic firms. Using detailed firm-level data for the period 2001–2008, this paper examines the aggregate effect of FDI on the survival of domestic private firms in Viet Nam. We estimate the impact of both horizontal and vertical FDI and explore how the presence of state-owned enterprises (SOEs) influences the exit hazard for private firms. The results suggest that horizontal and upstream FDI raise the exit hazard significantly, while downstream FDI may reduce the hazard. The presence of SOEs has a direct negative effect on the survival odds of local private firms in the same industry, but there is also an indirect impact on the exit hazard from FDI. Local firms are more vulnerable to foreign entry in sectors with high SOE shares. Looking at the net effects of FDI during the period 2001–2008, we find that results vary between sectors and over time but that the overall impact has been surprising small. The paper also discusses policy conclusions and implications for empirical analyses of spillovers from FDI.

Keywords: FDI, state-owned enterprises, exit hazard, survival, Viet Nam
JEL codes: F23, F21, L11

I. Introduction

Much of the academic literature on the host-country effects of foreign direct investment (FDI) has focused on various types of external effects or spillovers that may benefit or harm local firms. In particular, technology and knowledge spillovers have been subject to extensive research. The overall evidence is mixed, with several studies finding evidence of positive spillovers, but others arguing that the impact of FDI on technology and productivity in local firms is insignificant or even negative (Blomström and Kokko 1998, Görg and Greenaway 2004, Meyer and Sinani 2009, Wooster and Diebel 2010). One reason for the mixed findings could be that the ability of local firms to absorb spillovers differs between countries and industries, depending on the nature of competition between foreign and local

*Ari Kokko is Professor at the Department of International Economics and Management at the Copenhagen Business School and Associate Fellow at the Ratio Institute in Stockholm, Sweden. Tran Toan Thang is a researcher at the Central Institute of Economic Management in Ha Noi. We are grateful for helpful comments by the participants at the *Asian Development Review* conference held in Manila on 25–26 March 2013 and two anonymous referees.

firms, the development level of the host economy, the trade policy environment, and various other market conditions.

However, the entry and presence of foreign investors does not only have an impact on the technologies used in domestic firms, but it also affects various other characteristics of the host-country market. Apart from their direct and indirect effects on technology choice and knowledge, foreign investors may have some influence on the nature and intensity of competition and on the demand, supply, and quality of inputs and intermediate goods. These impacts may also influence the empirical measurement of various spillovers and externalities. Some of the effects are likely to be highly beneficial to local industry—it is hard to argue that access to new knowledge and technology, better and cheaper intermediate goods, or increased demand for locally produced inputs would be harmful to local firms—but other consequences of foreign entry and presence may well be negative. For example, FDI typically results in increased competition in both output markets and markets for skilled labor and other inputs, which may result in the crowding out of domestic firms.

Consequently, some studies have also focused on the crowding out or survival effects of FDI. These studies typically try to estimate how the inflow of FDI affects hazard rates or the likelihood that local firms are forced to leave the market. This strand of literature tends to suggest that there is often a negative horizontal survival effect, i.e., that the competition from foreign multinational corporations (MNCs) raises the likelihood of exit for domestic firms in the industry. The pattern for vertical FDI may be different since there is no direct competition effect when foreign investors are engaged in upstream or downstream sectors. However, there are relatively few studies on the survival effects of FDI in general and a particular scarcity of studies on developing countries and transitional economies.

The purpose of this paper therefore is to explore the impact of FDI on the survival of domestic private firms (DPFs) in Viet Nam. We will also discuss the relationship between the survival of DPFs and the presence of state-owned enterprises (SOEs) in the local market. This question is worthy of investigation because of the imperfect market competition in Viet Nam. Since private entrepreneurship emerged only recently in the country, DPFs are in general relatively young and small and have to compete with SOEs that are not only larger but also benefit from various policy-related privileges. If the presence of foreign-owned firms is expected to influence the survival odds of DPFs, the same is sure to hold also for the presence of SOEs.

From a policy perspective, it is clear that questions about the survival of firms are important. Together with firms' entry and growth patterns, survival and exit shape the dynamics of domestic industry development. In particular, there are important political connotations in the short run if it is found that FDI forces domestic firms out of business, possibly contributing to lower growth rates and unemployment problems. These concerns may be especially relevant in the context

of Asian development, where the nature and motives of inward FDI has recently shifted from cost savings to market penetration (Fujita 2011).

The paper will make at least four contributions to the extant literature. First, we examine the survival effects from both horizontal and vertical FDI, in contrast to earlier studies which focus on the survival impact of horizontal FDI alone. Second, unlike most of the earlier studies that consider domestic firms as a homogenous group, we highlight the role of SOEs for industry dynamics. Third, we explicitly try to manage estimation problems related to the endogeneity of covariates. Fourth, in addition to identifying the partly offsetting effects of horizontal and vertical FDI, we also attempt to calculate the net effect of foreign presence. In addition, our concluding discussion throws some doubt on the value of existing microlevel estimates of the spillover effects of FDI, which are typically based on enterprise samples that include firms that are crowded out (and by definition do not benefit from technology spillovers) as well as firms that survive (and may benefit from spillovers).

Apart from this introduction, the paper consists of five sections. Section II outlines the empirical setting for the study and describes some of the specific characteristics of DPFs in Viet Nam. The context is obviously distinct from that in developed countries. The heritage from central planning and the ongoing transition process also distinguishes Viet Nam from many other developing economies. Section III provides a brief literature review. Section IV presents the data, empirical model, and variables used in the regression analysis, while section V discusses the estimation results. Section VI concludes with a discussion of the policy consequences and theoretical relevance of the findings.

II. Domestic Private Firms in Viet Nam

In 1986, Viet Nam launched an economic reform process to address some of the weaknesses of central planning and to introduce elements of private entrepreneurship and market economics. This process known as *Doi Moi* or renovation has resulted in the gradual liberalization and privatization of both input and output markets, although SOEs remain important actors in many sectors of the economy. By the mid-1990s, as part of the *Doi Moi* process, the Government of Viet Nam had introduced a series of reforms supporting the development of the private sector, including the promulgation of a Company Law and a Private Enterprise Law in 1990, changes in the Constitution recognizing the role of private enterprise in 1992, a Domestic Investment Promotion Law in 1992, and a Bankruptcy Law in 1993. After many years of being heavily restricted or even considered an illegal business form, DPFs gradually gained recognition as important economic actors, with almost the same status as SOEs and foreign owned firms.

However, the development of DPFs has not been straightforward. In the late 1990s, more than 10 years after the initiation of the reform process, domestic policy

Table 1. **Total Output Share by Sector, 2008 (%)**

Sectors	FDI Firms	SOEs	DPFs
Food processing	24.34	42.73	32.25
Textile, leather, wood	39.80	26.46	33.56
Gas, chemicals	35.54	44.28	20.03
Construction	7.36	35.08	57.44
Metal, machinery	51.26	24.47	24.13
Electricity, energy	13.83	58.12	27.94
Commerce, repairs	5.85	36.77	57.27
Transportation	22.50	22.29	55.13
Telecommunication	21.78	48.83	28.40
Financial services	19.92	50.38	29.55
Research and development	37.56	47.16	15.26
Real estate	28.85	28.39	42.71
Other services	13.20	55.42	31.34

DPF = domestic private firm, FDI = foreign direct investment, SOE = state-owned enterprise.
Source: Authors' calculations from the enterprise census.

makers still had mixed views regarding DPFs (Hakkala and Kokko 2008). The development of the private sector was therefore slow and cautious, and only around 40,000 private enterprises were in existence at the end of 2000, contributing less than 10% of GDP (CIEM-UNDP 2010). The introduction of the Enterprise Law in 2001, which greatly simplified the procedures for establishing DPFs, together with political statements confirming the importance of private enterprise, became a turning point for the development of the domestic private sector. The number of newly established private firms started growing steeply from 2001.

By 2008, the number of DPFs had surpassed 150,000. They accounted for 27.3% of total output, with much higher shares in sectors such as construction (57%), commerce (57%), and transportation (55%). Although SOEs maintain a dominant position, DPFs and foreign firms jointly account for well over half of total output in most broad industry groups (Table 1).

Despite their growing number, DPFs remain fairly weak in comparison with SOEs and foreign enterprises. In 2008, almost three-quarters of DPFs were found in labor-intensive industries with low technology and poor management. Most DPFs also belong to the small-sized and medium-sized enterprise sector. While the average number of employees in SOE was 425 and that in foreign owned firms about 325, the average DPF had just 24 employees. Only 12% of Viet Nam's DPFs had more than 50 employees in 2008 (Table 2).

It is not surprising that affiliates of foreign multinational firms are larger and stronger than DPFs. SOEs also hold a favored position in Viet Nam's "socialist market economy" and have special privileges in terms of access to output markets, land, and credit sources (Hakkala and Kokko 2008). DPFs, by contrast, generally struggle to manage the large sunk costs involved with the international market. Only a small number of DPFs were engaged in direct exports in the early 2000s, and most of them did not have appropriate strategies for the rapidly internationalizing

Table 2. **Firm Size Distribution, 2008 (%)**

Firm Size	SOEs	DPFs	FDI	Total
0–50	20.97	87.20	29.46	76.61
50–100	17.29	6.27	18.10	8.12
100–200	21.09	3.59	18.03	6.36
200–300	11.11	1.17	9.66	2.75
300–1,000	22.21	1.44	18.52	4.72
1,000–3,000	6.05	0.30	4.71	1.19
3,000–5,000	0.89	0.02	0.90	0.17
5,000–10,000	0.35	0.02	0.38	0.07
>10,000	0.05	0.00	0.24	0.02
Total	100.00	100.00	100.00	100.00

DPF = domestic private firm, FDI = foreign direct investment, SOE = state-owned enterprise.
Source: Authors' calculations from the enterprise census.

economy (Kokko and Sjöholm 2005). DPFs also struggle to compete with the SOEs in factor markets. Hansen, Rand, and Tarp (2009) and Carlier and Son (2004) concluded that the main problem related to the funding of business in Viet Nam was not the shortage of capital but rather unequal access to capital. In 2003, SOEs accounted for less than 4% of total employment but received nearly half of total official credit.

DPFs face problems in the formal credit market because of the unwillingness of banks to extend credit to private firms, which are often considered to be more risky than state-backed SOEs and rarely able to provide collateral (generally land). The constrained access to bank credit limits access to land markets, and vice versa, creating a vicious cycle for the DPFs. Shortages of investment capital also limit DPFs' ability to upgrade technology, which often leads to slow productivity growth. This is a severe handicap for both the growth and survival of DPFs.

III. Literature Review

A term commonly used in discussions about survival and exit hazards is the "crowding out effect." An early argument for this effect is provided by Grossman's (1984) occupational choice model, which suggests that in an open economy, inward FDI may lead to the failure of domestic firms because foreign firms typically pay higher wages than DPFs. He argued that the best potential entrepreneurs are also the best workers. Therefore, by paying higher wages, foreign firms may discourage individuals from entrepreneurship. The higher wages paid by foreign firms may also force local companies to compete for the most productive labor, adding to wage costs and raising the exit hazard (Driffield and Girma 2003, Pesola 2006). However, effects operating through the labor market are not the only channels through which FDI influences the survival odds of local firms. Later studies have gone beyond arguments based on labor markets and conceptualize the crowding

out effect of inward FDI as the sum of competition, spillovers, and production linkages.

Looking at the horizontal impact of inward FDI in the local market, the expected effect on domestic firms is largely negative. Foreign entry does not only add to increased competition for labor and other inputs but also reduces output prices—the price effects on both the input and output sides raise the exit hazard for domestic firms, all other things being equal. The variable that adds some uncertainty is the possible presence of horizontal productivity or technology spillovers. Those local firms that are able to learn from the technologies or practices employed by their foreign competitors may be able to improve their efficiency and productivity. This positive spillover effect may in some cases be strong enough to mitigate the increased exit hazard, but it is clear that many local firms will mainly be influenced by increasing competition rather than positive spillovers, at least in the short run (Aitken and Harrison 1999; Blomström, Kokko, and Zejan 2000; Caves 2007; Crespo and Fontoura 2007). Several factors have been identified as determinants of spillovers, including the complexity of foreign technology and the technology gap between domestic and foreign firms (Kokko 1994), the absorptive capability of domestic firms (Cohen and Levinthal 1990, Kinoshita 2001), and the strategic role of the foreign affiliates in the network of their parent company (Kokko and Kravtsova 2008, 2012).

The expected effects of FDI in upstream and downstream sectors are not equally clear-cut because vertical FDI does not include any direct competition effect. Instead, impacts are largely determined by the nature of the linkages between local firms and their foreign suppliers and customers. Vertical FDI is often expected to generate various productivity spillovers, e.g., through the increase in product varieties and the use of specialized inputs from backward linkages, or the technical support and guidance provided through forward linkages (Rodriguez-Clare 1996, Markusen and Venables 1999). However, negative effects are also possible, in particular when FDI results in changes in technological standards and quality requirements. FDI in upstream sectors may crowd out competing local firms that supply inputs with lower price and quality, forcing downstream firms to adjust technologies. Downstream FDI may crowd out the traditional customers of local firms operating in upstream sectors. Downstream FDI may also stimulate the entry of foreign suppliers, adding to the competition in the upstream sectors. The aggregate impact of vertical FDI on the exit hazard for domestic firms is therefore hard to predict on theoretical grounds, although it is reasonable to expect a priori that the impact of downstream FDI should be less negative than that of horizontal FDI.

More generally, the positive effects from linkages seem to depend on the characteristics of the incoming FDI (Veugelers and Houte 1990, Pradhan 2006). For example, depending on entry mode, foreign firms can influence the number of domestic firms by directly replacing domestic firms or by inducing domestic firms to merge in order to manage tougher competition. In a longer term perspective,

foreign entries with high research and development (R&D) investment may crowd out research and development investment of domestic firms, reducing the domestic firms' long-term survival odds (Haller 2005). Similarly, and in line with Grossman (1984), foreign firms can trigger brain drain in domestic firms in upstream or downstream sectors.

Using social network analysis, Giuliani (2008) showed that backward linkages do not necessarily create information linkages and knowledge transfer. In his empirical analysis of Costa Rica, only 21% of the backward linkages overlapped with information linkages, which is below the expectation from theoretical literature. Lin and Saggi (2007) suggested that the backward linkages between foreign firms and suppliers can result in the delinking of existing connections between local producers and their local suppliers, hence making some local producers worse off. Other studies (Navaretti and Venables 2004, Carluccio and Fally 2010, Markusen and Stähler 2011) also suggest the possibility that fierce competition in factor markets due to the foreign entry may harm domestic firms in upstream and downstream sectors. The entry of foreign firms may result in the entry of new suppliers, lead to tougher competition, and induce more exits in the intermediate goods market.

A. Theoretical Models of FDI and Survival

Markusen and Venables (1999) provide one of the first formal models that combine these different effects. They note that FDI generates a competition effect, which is likely to be particularly strong in final goods markets and will lead to lower market prices that may force less efficient domestic firms out of the market. At the same time, they posit that foreign firms in downstream industries may foster the formation of local suppliers through production linkages, as they demand inputs for their production processes. They may therefore induce domestic firms to enter the intermediate goods market, which in turn leads to reductions in input prices. Such price reductions suggest two effects: increased entry of domestic firms in the downstream sectors and more exits in the upstream sectors. Hence, Markusen and Venables (1999) predict both horizontal and vertical impacts, with entry as well as exit effects following from the presence of foreign firms in the domestic market.

More recent studies have extended the theoretical analysis. Navaretti and Venables (2004) use a monopolistic competition model to derive the welfare effects of multinational entry on domestic firms. Under the assumption that foreign firms produce at a similar marginal cost as domestic firms, they suggest that the entry of foreign firms replaces domestic firms one by one. This prediction is repeated by Markusen and Stähler (2011) under assumptions of fixed and endogenous domestic market structures. They suggest that if the market structure is endogenous, changes in the foreign firm's output level will not change aggregate production and the size of active domestic firms, but instead result in market entry or exit. If the market structure is fixed, foreign investment will lead to an increase in aggregate output but

a reduction in the output and the profit of domestic firms. That reduction may in the longer term result in further exit of domestic firms.

Kosova (2010) links the survival question to the firm's growth and suggests that the determinants of growth and survival may be similar. Her static model is the dominant-fringes model, in which the dominant firms are foreign and the fringe firms are domestic. Foreign firms operate as market leaders and select an output level (where the marginal revenue equals marginal cost) which determines the market price and hence the quantity sold by the fringe firms. A proportion of fringe firms will be crowded out if their marginal costs are substantially larger than those of foreign firms. The dynamic version of Kosova (2010) is based on a combination of the dominant-fringes model and dynamic industrial models (Jovanovic 1982) and technological shock models (Sun 2002). The results suggest that the exit hazard of domestic firms decreases with higher output prices, positive technology shocks, and the expectation of higher efficiency. Moreover, a higher growth rate of foreign firms leads to a higher exit rate of domestic firms. This effect is described as the dynamic crowding out effect.

B. Empirical Evidence on FDI and Survival

There are few empirical studies on the impact of FDI on the survival of domestic firms. Appendix 1 briefly summarizes the results from the most well-known studies, and shows that the empirical evidence is contradictory. Studies by Iurchenko (2009) for manufacturing in the Ukraine; Ferragina, Pettiglio, and Reganati (2009) for the service sector in Italy during the period 2005–2007; Burke, Görg, and Hanley (2008) for the United Kingdom (UK) manufacturing in 1997–2002; and Girma and Görg (2003) for Ireland in 1973–1996, all find evidence of positive survival effects from FDI. These results stand in contrast to negative or nonsignificant effects of horizontal FDI found by Louri, Peppas, and Tsionas (2006) for Greece in 1997–2003 and Girma and Görg (2003) for the UK in 1973–1996.

One explanation for the contradictory results could be that none of the studies mentioned above examine the survival effect from vertical FDI. To the best of our knowledge, Wang (2010) is the only study to explicitly consider the effect of vertical FDI on the survival of domestic firms. He analyzes the survival probability of 47,000 manufacturing firms in Canada during the period 1973–1996. The findings suggest that the competition from horizontal FDI shortens the domestic firms' expected survival span, but that FDI in forward and backward sectors have positive effects as a result of positive technology spillovers. Overall, the conclusion is that the positive effects outweigh the negative effects, but it should be noted that this conclusion is based only on a comparison of marginal effects.

There are no earlier studies of the impact of FDI on firm survival in Viet Nam. Vijverberg and Haughton (2002) examine the life spans of household enterprises using the Viet Nam Living Standard Survey. Carlier and Son (2004) discuss survival

and exit of firms in a qualitative study that is based on a small sample of enterprises. Hansen, Rand, and Tarp (2009) use the Cox proportional hazard function to estimate the impact of government support on the survival and growth of small and medium-sized enterprises for a sample of 2,500 enterprises. All three studies focus on local and sectoral conditions as determinants of firm survival, but do not include FDI among the covariates.

C. Other Determinants of Firm Survival

FDI is obviously not the only variable influencing firm survival, and recent literature has identified a number of other key determinants of the survival and exit of firms. The relationship between a firm's survival and its age and size is described in a number of industrial organization studies (Jovanovic 1982, Hopenhayn 1992, Ericson and Pakes 1995, Lambson 1991). A core argument in this line of research is that firms do not know about their "true" efficiency before entering the market. Upon entry, they find out about their true efficiency and respond accordingly when faced with uncertainty and productivity shocks; some firms survive and grow while others exit. There is a learning process (Jovanovich 1982) so that older firms, which have had more learning opportunities, are more likely to survive until the next time period. There is also a correlation between firm size and survival, meaning that larger firms have a higher propensity to survive.

Exits of firms are not only caused by the characteristics of individual firms, but also by sectoral characteristics. For example, Lambson (1991) posits that firm performance depends on prevailing market conditions such as input prices and market demand. If market conditions change frequently and sunk costs are large, both the entry and exit of firms will be influenced by changes in input prices. Industry dynamics are also affected by demand shocks that influence firms' expectations about future demand.

The role of competition has also been highlighted in many studies (Agarwal, Sarkar, and Echambadi 2002; Nelson and Winter 1982). Competition does not only lead to the crowding out of inefficient firms, but exerts a more complex effect. Market concentration may stimulate collusion, creating more scope for profits and therefore a higher probability to survive. Market concentration may also result in the establishment of barriers to entry, which could allow some inefficient incumbent firms to survive longer than would otherwise be the case.

A factor closely related to competition is technological change. Klepper (2002) and Klepper and Simons (2005) highlight the importance of technological events that may lead to a shakeout in the industry. These events may originate within or outside the industry and affect both potential new entrants and incumbents. Externally generated innovations will result in a race to adapt to or take advantage of the new technologies. Firms that manage to adopt new technologies gain lower unit costs and expand to a greater optimal size, while firms that fail to adjust become

unprofitable and exit. Internally generated innovations developed by incumbent firms often set new standards for products. Consumer demand shifts to such standardized designs, and firms compete to produce the standard product at the lowest possible cost. Exit risk rises as firms shift from a past regime of product innovation to a new regime of process improvement. Firms that do not succeed at process innovation are driven out of business.

The empirical evidence for the determinants of survival is reasonably consistent with the theoretical predictions. Manjon-Antolin and Arauzo-Carod (2008) provide a comprehensive summary that classifies the firm's survival determinants into internal and external categories. For internal determinants, most empirical studies find that size and age, R&D, and ownership of firms are key determinants. Evidence suggests that the effects of age and size are not uniform: size may have a nonlinear impact, and there may also be an inverse U-shaped impact of age. The role of R&D has been confirmed in many studies, e.g., by Audretsch (1995) for the US and Esteve-Perez and Manez-Castillejo (2005) for Spain. The evidence of ownership focuses mainly on the distinction between foreign and domestic firms. Most studies, including Mata and Portugal (2002), Kimura and Fujii (2003), Görg and Strobl (2003b), and Esteve-Perez, Manez-Castillejo, and Sanchis-Llopis (2008) report findings suggesting that foreign firms are more footloose than domestic firms, meaning their threshold for exit from the host market is lower. Esteve-Perez and Manez-Castillejo (2005) find no difference in the hazard rate for limited and unlimited liability companies.

For the external factors, the most prevalent determinants are industry characteristics, spatial factors, and the business cycle. Agarwal, Sarkar, and Echambadi (2002) and Esteve-Perez, Manez-Castillejo, and Sanchis-Llopis (2008) both point to a higher hazard rate for firms in high-tech industries. This is explained by the rapid obsolescence of the firms' technological endowment in rapidly changing high-tech sectors. In addition, the entry rate (Mata and Portugal 2002, Lopez-Garcia and Puente 2006) and the minimum efficient scale (MES) of production (Audretsch and Mahmood 1995, Strotmann, 2007) determine the probability of a firm's survival. A high rate of entry puts pressure on incumbents, while a high MES acts as a barrier to both entry and exit. The evidence for spatial factors is mixed. For example, Strotmann (2007) found that rural firms are more likely than urban firms to survive. Louri and Barbosa (2000) and Fritsch, Brixy, and Falck (2006) report contradictory results.

Survival is also related to fluctuations in the business cycle (Manjon-Antolin and Arauzo-Carod 2008). In many studies, sectoral growth is an important determinant of survival: firms fail more often in recessions. Researchers using cohort dummies and time dummies generally confirm the importance of macroeconomic conditions (Lopez-Garcia and Puente 2006; Esteve-Perez, Manez-Castillejo, and Sanchis-Llopis 2008; Disney, Haskel, and Heden 2003).

IV. Data, Estimation Issues, and Variables

A. Data

The data for this study are extracted from Viet Nam's yearly enterprise census, which is conducted by the General Statistical Office (GSO). The census includes all known active firms in the economy in each year. The current dataset covers the period 2001–2008.

The data are merged to form a panel dataset. Each firm has an identifier that is the tax code. The tax codes are not always available immediately to new firms, which means that some of the newly established firms have instead been identified using telephone/fax numbers. Over 5,000 firms had to be excluded from the sample due to the lack of information necessary to identify them from the annual data.

After merging all annual observations and dropping the firms that could not be identified over time, the dataset makes up an unbalanced panel dataset containing a total of 86,108 individual firms, starting with 28,358 firms in the year 2001 and ending with 55,701 firms at the end of 2008. Three types of firms can be identified in this dataset: DPFs, SOEs, and foreign-owned firms. A large number of firms enter and exit during this period. Among the DPFs operating at the beginning of the period, only 45.6% survived until 2008. The corresponding number for foreign firms is 52.6%.

The survival function of a firm is defined as the probability that the firm survives past time t given that the firm has survived until time t. In this context, time t is defined as the length of a year. Therefore, firms at risk of failure at time t are firms in their t'th year in the dataset. From 2001 to 2008 there are 7 time-intervals coded from 1 to 7, and time t in this analysis is considered as an interval-discrete time period rather than continuous.

A firm's exit or death at time interval t is identified when the firm is observed in interval t but does not exist in subsequent intervals. This means that the time of a firm's exit is not exactly known: only the interval in which the firm exited is known. Similarly the firm's entry into the market is not known exactly until it is observed in the interval time period t in the dataset. Both cases require the use of discrete time models instead of continuous time models.

After arranging the data in order to implement the discrete time model, the data is expanded according to time intervals, so that each firm has one observation or more than one observation, depending on how long they survive. For example, a firm surviving through the entire sample period will have seven observations in the final dataset, which altogether has 312,506 observations. There were 53,109 firms in the dataset at the end of the analysis period. Since we do not have information about events after 2008, we do not know how long they survive afterwards. Hence, they are classified as right censored.

B. Model Specification

The survival function $S(t)$ of a firm at time t is defined as the probability of that firm remaining in the market beyond time t (see Jenkins 2005 for the details):

$$\boldsymbol{S(t) \equiv Pr(T > t) = 1 - F(t)} \tag{1}$$

where $F(t) = Pr(T \leq t)$ is called the failure function, representing the areas below the density function $f(t)$ of time spells. A related concept—the hazard rate or hazard function—is defined as:

$$\boldsymbol{\theta(t) = \frac{f(t)}{1 - F(t)} = \frac{f(t)}{S(t)}} \tag{2}$$

The hazard rate is not expressed exactly, but as the conditional probability of the firm to exit shortly after surviving up to time *t.* Because of the close relationship between the survival function and the hazard function, it is common to estimate the hazard rate instead of estimating survival time.

Survival literature distinguishes two types of hazard functions: the proportional hazard model (PH) and the accelerated failure time model (AFT). With a few exceptions (e.g., Wang 2010), most of the existing analyses of survival use variants of the PH model, such as the cloglog and lognormal models (Bandick and Görg 2010, Kosova 2010) or models based on the Cox proportional hazard function (Görg and Strobl 2003b, Taymaz and Özler 2007; Burke, Görg, and Hanley 2008). The choice of hazard function is based on the assumption of how the firm's survival odds change over time. For the PH type, the typical characteristic is a separability assumption stating that:

$$\boldsymbol{\theta(t, X) = \theta_0(t)\, exp(\beta' X)} \tag{3}$$

where $\theta(t, X)$ is the hazard rate at survival time t for a firm with covariate vector X; $\theta_0(t)$ is called a baseline hazard function, depending on t but not X (expressing a common exit pattern for all firms in the dataset); and $exp(\beta' X)$ is a non-negative function of covariates X. This assumption implies that the absolute difference in X reflects the proportionate difference in the hazard at each time t.

If it is assumed that time is continuous, then it is appropriate to use continuous models like the Cox proportional hazard model. If time is instead defined as a discrete variable, it is more appropriate to use discrete models like logit or complementary logit models (cloglog). In the current dataset, it can be argued that although time is continuous, the spell length is measured only in 1-year intervals (from July to July). Firms can exit the market at any time within the interval. In survival language, this means the failure can occur within a specific interval, but it is not known exactly

when it happens. Datasets with this nature can be described as censored interval data, and in combination with the proportional hazard assumption, it is most appropriate to use the cloglog model for estimation purposes. Therefore, the hazard function for interval j used below will take the form:

$$log(-log[1 - h_j(X)]) = \beta' X + \gamma_j \; or \; h(a_j, X) = 1 - exp[-exp(\beta' X + \gamma_j)] \quad (4)$$

where X is the vector of covariates and γ_j is the log of the difference between the integrated hazard $\theta_0(t)$ evaluated at the end and the beginning of the interval.

A further issue is the fact that the dataset is right-censored. At the end of the period of analysis, there is a group of firms that remain active, but it is unknown how long they will survive. OLS estimation for this type of censored data can be biased. We solve this problem by using standard survival estimation procedures. To do that, the dataset is rearranged into a particular form by splitting the observations by the number of spells (years) in the dataset. As shown by Jenkins (2005), this makes it possible to use a standard binary estimation procedure. In all, four steps are done before the estimations:

i. We expand the data for each firm in accordance with its survival time. This means that each firm will have more than one observation in the dataset if it survives for more than one time interval. We end up with 312,506 observations out of which 51,710 are right-censored observations.

ii. We construct the time-varying covariate vector (X) and merge it with the firm-year based data.

iii. We select the functional form for the hazard function. As noted above, we focus on models with PH properties, where the clogclog model is our preferred choice.

iv. We estimate the model using binary dependent variable regression models.

C. Construction of Covariates

The vector X for the hazard function (4) is constructed on the basis of the findings in the literature review as well as the availability of information in the current dataset. It includes three components: sectoral characteristics, firm characteristics, and dummy variables.

D. Sectoral Characteristics

HFDI, DownFDI, and *UpFDI* represent the foreign presence within a sector as well as in downstream and upstream sectors. These variables are the key variables

for testing the hypotheses related to the impact of foreign presence. The calculation for foreign presence variables is based on the output share of foreign firms in 3-digit sectors, as follows:

$$HFDI_{jt} = \frac{\Sigma_i FDI_{ijt}}{\Sigma_i R_{ijt}} \tag{5}$$

where FDI_{ijt} are the output values of foreign firm i in 3-digit VSIC sector j at time t while the denominator (R_{ijt}) is the total output of all firms in the sector.

UpFDI and *DownFDI* are calculated as the product of horizontal FDI in downstream and upstream sectors weighted by the coefficients of the IO table α_{st} and its transposed matrix δ_{st}:

$$UpFDI_{jt} = \Sigma\alpha_{st} HFDI_{jt} \quad \text{and} \quad DownFDI_{jt} = \Sigma\delta_{st} HFDI_{jt} \tag{6}$$

HFDI is expected to raise the hazard of exit and hence reduce the survival odds of DPFs (a static crowding out effect), while *DownFDI* and *UpFDI* are also expected to influence DPFs through spillovers and demand creation and may therefore either raise or reduce the hazard.

In some of the regressions, we will also use the corresponding measures for SOEs. They are denoted *HSOE*, *UpSOE*, and *DownSOE*, and they are defined similarly as the FDI variables. $GFDI_j$ reflects the output growth of FDI in the sector j. Based on Kosova (2010), we would expect the variable to have a negative effect on the survival of domestic firms. However, Audretsch (1991, 1995) argues that demand and output growth could elevate price above average cost, allowing firms to improve their price cost margins and their survival probability. Because of these contradictory prior expectations, we have no firm expectation for the sign of this variable in the model. The output growth rate of SOEs, *GSOE*, is defined in an analogous manner.

EXPORT is the ratio of exports to total sales for each 3-digit sector. This variable is intended to control for the fact that firms in export-oriented sectors may have better survival odds thanks to the demand from the world market.

Another reason for expecting exports to be important is that exporting is likely to influence the competition between DPFs and foreign-owned firms. More specifically, holding the volume of FDI-generated output constant, it can be expected that the competitive pressure felt by DPFs is lower when the foreign firms are export-oriented rather than focused on the local market. We therefore include the variable *EXPRATIO*, which reflects the export to sales ratio of foreign-owned firms in each 3-digit sector. To explore the relation between export oriented FDI and local competition further, we also interact *EXPRATIO* with *HFDI*.

IMPORT is the ratio of imports to total sales for each 3-digit sector. In the short term, an increase in imports of final goods is supposed to raise the exit hazard of DPFs.

The Herfindahl index represents the concentration in the market. It is calculated as the sum of squares of the output shares in the sector (see Tirole 1988):

$$HERF_{jt} = \Sigma \left(\frac{x_{ijt}}{X_{jt}} \right)^2 \tag{7}$$

where x_{ijt} is the output of firm i in sector j at time t. X_{jt} is total output of sector j. The Herfindahl index is included in the model because it is closely correlated with the market power of larger firms. The effect of this variable on the survival of DPFs, however, is not unambiguous. High concentration typically results in high price-cost margins, which means that incumbent firms should have a lower hazard rate and a higher probability of survival. At the same time, high concentration suggests that less efficient firms face pressure to leave the market.

MSCALE is the MES of the industry, measured as the log of median employment size in the 3-digit sector. Audretsch (1995) argued that a firm may be forced to exit the market if its production scale is below the technically efficient minimum level required by the industry. Sectors which have a high minimum scale are believed to have high price-cost margins and hence ensure a higher survival rate for those firms that can reach this scale. The average effect, however, is unclear. New firms in sectors with high *MSCALE* also encounter more difficulties than firms that enter other sectors (Görg and Strobl 2003b).

ENTRY is the entry rate in 3-digit sectors, computed as the ratio between the total number of firms entering into the sector and the total number of firms operating at that time. A high entry rate reflects a low cost of entry to the market. In addition, a high entry rate also reflects high competition and may lead to slower growth for individual firms as well as a high exit rate. In fact, Siegfried and Evans (1994) suggest that there is a direct relationship between the entry and exit rates because inefficient incumbents will be replaced by more efficient entrants. The *ENTRY* variable is included to test for this replacement hypothesis.

E. Regional Characteristics

NBR and *DIVER* are geographical variables. They are included to capture the impact of density and therefore competition in a geographical context and to capture the agglomeration effect on firms' survival. The inclusion of these variables is motivated by the heterogeneity across different provinces in Viet Nam.

NBR is a proxy for neighborhood concentration. It is computed as the neighbor agglomeration index:

$$NBR_{jr} = \sum_{k \neq r}^{61} \frac{C_{jk}}{d_k^2} \tag{8}$$

where C_{jk} is the total output of sector j of province k, and d_k is the distance (in kilometers) from province k to province r. In other words, this variable is the sum of the distance-weighted outputs of other provinces. If a province is located in a more concentrated region, the value of *NBR* is higher. Hence, this indicator reflects the local competition and is expected to raise the hazard of exit.

DIVER is the diversity index computed as:

$$D_r = \Sigma_j \left(q_{jr}\right)^2 \tag{9}$$

where q_{jr} is the share of output from sector j in province r. Diversity comes into effect through the availability of complementary goods and services and choices, and it is assumed to reduce the vulnerability to external shocks and the exit hazard.

F. Firm Characteristics

REL_SIZE is measured as the ratio between the firm's employment and the average size of firms in its industry (3-digit VSIC). Larger firms are expected to have a lower hazard of exit because they may benefit from scale economies and have more capacity to do R&D as well as to expand their networks and diversify their products.

CAP_INT denotes the capital intensity of the firm and is measured as the ratio of fixed assets (deflated by the gross domestic product [GDP] deflator) to the total number of employees. The variable is included to capture the effect of specific capital costs as well as the underlying efficiency level as analyzed by Kejzar and Kumar (2006).

AGE is the age of the firm, measured as the number of years since their establishment. Firms' age reflects the experience of the firm in the market, also covering the learning process that could be either passive or active (Jovanovic 1982, Hopenhayn 1992). The older the firm, and the longer the learning process, the lower the hazard of exit.

In addition, to control for sectoral/regional heterogeneity, dummies for sectors and regions are included. There are 10 sectoral dummies that are based on the 1-digit VSIC, capturing all subsectors in agriculture, mining, manufacturing, and services. Seven regional dummies are constructed on the basis of the standard regional classification in Viet Nam. They capture both geographical and economic development differences between regions.

It should be noted that like many other survival studies, the present model does not include time dummies. A first reason is that they would be highly correlated with the variable *AGE* in the model. Second, the business cycle, technological progress, and other temporal shocks are already included in the time varying covariates in the model, like the growth of SOEs and FDI, as well as changes in the market

Table 3. **Variable Definitions**

Variables	Computation	Mean	Std. Dev.	Expected Sign[a]
$HFDI_{jt}$	Foreign presence at time interval *t* in sector *j*, measured as total output value of FDI firms in sector *j*/total output value of the sector	0.220	0.267	+
$UpFDI_{jt}$	Foreign presence at time interval *t* in upstream sectors relative to sector *j*, computed from *HFDI* and transposed IO table coefficients	0.260	0.133	+/–
$DownFDI_{jt}$	Foreign presence at time interval *t* in downstream sectors relative to sector *j*, computed from *HFDI* and IO table coefficients	0.307	0.128	–
$HSOE_{jt}$	SOE presence at time interval *t* in sector *j*, measured as total output value of SOEs in sector *j*/total output value of the sector	0.478	0.303	+
$UpSOE_{jt}$	SOEs presence at time interval *t* in upstream sectors, computed from *HSOE* and transposed IO table coefficients	0.381	0.156	+/–
$DownSOE_{jt}$	SOEs presence at time interval *t* in downstream sectors, computed as sum product of *HSOE* and IO table coefficients	0.328	0.125	–
$GFDI_{jt}$	Sales growth of FDI firms in sector *j* at time *t*	1.255	0.819	+/–
$GSOE_{jt}$	Sales growth of SOEs in sector *j* at time *t*	1.043	0.693	+
$EXPORT_{jt}$	Export ratio of sector *j* at time *t*	0.062	0.165	–
$EXPRATIO_{jt}$	Export ratio of foreign-owned firms in sector *j* at time *t*	0.186	0.284	–
$IMPORT_{jt}$	Import ratio of sector *j* at time *t*	0.1202	0.214	+
$HERF_{jt}$	Herfindahl index, proxy for concentration in the market, calculated as the sum of squares of employment share in sector *j*			+/–
$MSCALE_{jt}$	MES of sector *j*, computed as the median employment of sector *j*	32.514	35.164	+/–
$ENTRY_{jt}$	Entry rate in sector *j* at time *t*	0.241	0.082	+
NBR_{rt}	Neighborhood concentration index, measured as the spatial market concentration of provinces surrounding province *r* at time interval *t*	448,445.4	68,421	+/–
$DIVER_{rt}$	Spatial diversity index, measured as the sum of squares of sectoral output shares in province *r* at time *t*	9.198	66.906	–
REL_SIZE_{it}	Relative size of the firm *i* at time *t*, measured as the output of firm *i* to median output in sector *j*	1.157	3.813	–
CAP_INT_{it}	Capital intensity, measured as total value of fixed assets to number of employees of firm *i* at time *t*	140.80	371.97	–
AGE_{it}	Age of firm *i* at time *t* measured as number of years since establishment to time *t*	4.525	4.151	–

[a]Expected effect of the variable on the hazard of exit.
Source: Authors' computations.

structure. Furthermore, due to the close correlation between the growth of SOEs and sectoral growth (which could be used to proxy the growth of market demand), we drop sectoral growth from the estimations to avoid multicollinearity. Table 3 summarizes the variables included in the regression models, as well as the mean,

standard deviation, and expected impact on the exit hazard. A correlation matrix is provided in Appendix 1.

G. Endogeneity

There is some potential endogeneity in the model specification noted above. A first source of endogeneity is unobservable heterogeneity caused by the business cycle, institutional reform, regional and industry factors, and other variables that are not included in the model but that may influence both the survival of domestic firms and foreign entry into the market. A second source of endogeneity is the interdependence or simultaneous causality between survival and some covariates in the model. The entry and exit of firms may be simultaneously determined, as new entrants force less efficient incumbents out of the market (Manjon-Antolin and Arauzo-Carod 2008). The exit of incumbents may also generate a "vacuum" of local input supplies or customers that motivate or allow new actors to enter the market.

In other words, entry and exit can be simultaneous not only because all firms are faced with similar market barriers but also because one can cause the other. In addition, foreign firms may prefer to enter sectors that have high (or low) exit rates, which may be seen as an indication of more (or less) competition. Neglecting these endogeneities may obviously cause spurious estimation results.

Responses to the endogeneity of covariates are hard to find in the survival literature, and even more scarce in studies examining the survival effects of inward FDI. Earlier studies have employed different strategies to handle the problem. Many studies have ignored it (for example Wang 2010; Ferragina, Pittiglio, and Reganati 2009; Iurchenko 2009; and Burke, Görg, and Hanley 2008), some have addressed it by introducing lags or sectoral dummies (Kosova 2010, Görg and Strobl 2003b), while others have used more advanced methods such as instrumental variables (Girma and Görg 2003, Bandick and Görg 2010). Ignoring potential endogeneity or simply using sectoral dummies or lags of potentially endogenous variables may not be sufficient to ensure unbiased estimation. We will therefore use a two-stage instrumental variable model to address this problem.

V. Results and Discussion

A. Descriptive Statistics: Survival of DPFs

As a first step, we investigate the survival of firms using nonparametric methods. Table 4 provides a first glance at the data in survival format. The first column shows the number of intervals, that is, the number of years of survival. The second column is the total number of firms at risk of failure during each interval. It shows that for the first interval 79,852 DPFs were at risk. It should be noted that

Table 4. **Survival of DPFs and Foreign Firms**

Interval	Beg. Total	Deaths	Lost	Survival	Std. Error	95% Confidence Interval	
DPFs							
1	79,852	9,376	9,027	0.8755	0.0012	0.8732	0.8779
2	61,449	5,080	7,347	0.7986	0.0015	0.7956	0.8015
3	49,022	3,434	7,230	0.7382	0.0017	0.7348	0.7415
4	38,358	3,286	6,339	0.6692	0.0019	0.6654	0.6730
5	28,733	6,311	4,998	0.5082	0.0023	0.5037	0.5127
6	17,424	655	5,072	0.4859	0.0024	0.4813	0.4905
7	11,697	0	11,697	0.4560	0.0024	0.4813	0.4905
Foreign							
1	6,256	540	435	0.9106	0.0037	0.9031	0.9175
2	5,281	398	405	0.8392	0.0048	0.8295	0.8484
3	4,478	264	511	0.7867	0.0055	0.7757	0.7973
4	3,703	260	444	0.7280	0.0062	0.7157	0.7399
5	2,999	769	388	0.5284	0.0076	0.5134	0.5432
6	1,842	34	396	0.5175	0.0077	0.5023	0.5324
7	1,412	0	1,412	0.5175	0.0077	0.5023	0.5324
Log-rank test: $= 30.02$; $P = 0.0000$							
Likelihood-ratio test $= 30.6092$; $P = 0.0000$							

Source: Authors' computations.

this is the total of all DPFs established at any time during the period 2001–2008. At the end of the interval, there were 9,376 firms that failed or died as shown in the third column named "Deaths." This is the sum of firms that did not survive after their first year (appearance in the dataset). The fourth column, "Lost," gives the number of firms that were censored or that were out of risk. This indicates that 9,027 firms that were established in the last year of the sample survived until the end of the sample period, i.e., were right-censored. Correspondingly, the data for the seventh interval shows that 11,697 enterprises recorded seven spells of survival. Since they survived through the whole sample period, there were no observations in the "Deaths" column. Moreover, all of them were right-censored, and survived beyond the sample period. Hence, they are all included in the "Lost" column.

The estimation of the survival function and its statistics are presented in the remaining columns. As shown in Jenkins (2005), the rate of survival at interval j is estimated by:

$$S_j = \prod_{k=1}^{f} \frac{\left(N_k - \frac{1}{2}m_k\right) - d_k}{\left(N_k - \frac{1}{2}m_k\right)} \tag{10}$$

where N_k is the number of firms at the start of the interval, m_k is the number of firms censored, and d_k is the number of firms that died. The "Survival" column records the estimated survival rates for all intervals. As shown in the table, only 45% of the

firms remained after 7 years. The table also reveals that the median of the survival duration is approximately 5 years.

The estimated survival rates of foreign firms are somewhat higher than those of DPFs at every interval: foreign firms have a median survival time of around 6 years. The last two rows in the table provide tests for the equality of survival functions. Both the log-rank test and the likelihood test indicate that the differences in survival propensity are statistically significant. This finding contradicts Görg and Strobl's (2003a) results for Irish manufacturing, which suggested that foreign firms seemed to be more footloose than domestic firms. However, it should be noted that most DPFs in Viet Nam during this period were relatively young and small—both of these characteristics raise the likelihood of exit.

Earlier survival studies in Viet Nam have shown somewhat lower survival rates, but these studies focused on firms established before 2001 (Hansen, Rand, and Tarp 2009) and household enterprises (Vijverberg and Haughton 2002). It is likely that the survival rates for firms established during the 1990s were lower because of the less favorable regulatory environment. Moreover, the lower survival rates of household enterprises are partly explained by the fact that they were even smaller than the DPFs established after 2001.

B. Econometric Results

As a first step of the econometric analysis, we have tested whether the assumptions for the proportional hazard model hold. Finding that this is the case (results not reported here but available on request), we proceed to estimate the PH model (equation 4). To handle the possible endogeneity of covariates *HFDI* and *ENTRY*, we complement the base equation (which assumes no endogeneity) with a variant where the potentially endogenous variables are lagged, as well as an estimation using the instrumental variable method (2SCML).

Table 5 shows the results of the model for all DPFs. All specifications are stratified at the 1-digit sector level. This procedure allows for differences in the baseline hazards. This kind of specification is supported by the Wald test presented at the bottom rows of the table. Column (1) of Table 5 is the estimation where *HFDI* and *ENTRY* are assumed to be completely exogenous. Column (2) presents the results of the estimation using the first lags of the endogenous explanatory variables. Column (3) shows the results with the 2SCML correction factors. It can be noted in column (3) that the correction factors in the first stage of the 2SCML estimation are statistically significant at the 1% level, confirming the prior suspicion that the *HFDI* and *ENTRY* variables are endogenous. Column (3) is therefore the preferred estimation equation.

Although the hazard ratio is commonly used to present the hazard function estimation, Table 5 reports the coefficient forms. The reason is that the signs of the coefficients are also the signs of the effects: a negative coefficient means a lower risk

Table 5. **Estimation Results—Impact of FDI**

	Exogenous (1)	**Lag 1 (2)**	**2SCLM (3)**
HFDI	0.253***		0.341***
	(42.16)		(26.16)
*Lag*1.*HFDI*		0.262***	
		(32.03)	
UpFDI	–0.198***	–0.208***	0.469***
	(17.42)	(11.32)	(14.41)
DownFDI	0.228***	0.202***	–0.408***
	(12.77)	(7.59)	(10.26)
ENTRY	0.543***		0.219***
	(16.91)		(5.67)
*Lag*1.*ENTRY*		0.871***	
		(19.77)	
GFDI	–0.009	–0.013	0.256***
	(1.0718)	(1.067)	(17.15)
EXPORT	0.742***	–0.695***	–1.083***
	(9.82)	(5.76)	(8.65)
EXPRATIO	0.00	0.001	–0.004***
	(0.10)	(1.80)	(4.37)
HFDI ∗ *EXPRATIO*	–0.006	–0.004***	–0.005***
	(26.47)	(9.37)	(9.07)
IMPORT	–0.187**	0.040**	–0.314***
	(17.02)	(2.63)	(17.76)
HERF	–0.198***	–0.352***	–0.593***
	(24.55)	(24.77)	(32.74)
MSCALE	–0.383***	–0.444***	–1.218***
	(16.79)	(12.60)	(23.20)
NBR	–0.126***	–0.051***	–2.588***
	(16.93)	(4.71)	(29.52)
DIVER	–0.047***	0.002	–0.060***
	(15.27)	(0.46)	(11.69)
REL_SIZE	–0.175***	–0.134***	0.159***
	(18.23)	(9.62)	(9.20)
CAP_INT	–0.008	0.013	–0.001
	(1.02)	(1.17)	(0.07)
AGE	–0.423***	–0.416***	–0.079**
	(34.30)	(16.33)	(2.88)
Constant	–1.116***	–0.860***	16.96***
	(8.39)	(3.49)	(20.87)
HFDI_correction			0.263***
			(9.68)
ENTRY_correction			–8.878***
			(31.72)
N	306,477	225,796	217,284
Log pseudo likelihood	–56,701.25	–27,722.85	–24,934.99
Wald tests	7,043.85	2,973.58	3,321.32

* = significant at the 10% level, ** = significant at the 5% level, *** = significant at the 1% level.

Note: Cloglog model, dependent variable: dead1 (1 = firm exit, 0 = otherwise). All estimations are in coefficient form rather than as hazard ratios. The *HFDI_correction* and *ENTRY_correction* variables are the error terms from the instrument equations of the 2SCLM procedure. Numbers in parentheses are z-values. Coefficients for regional/sectoral dummies are not shown.

Source: Authors' computations.

of exit. With a few exceptions, the variables have the expected sign in the estimation reported in column (3). However, several variables have the opposite sign in the first two columns of the table. These large differences between the estimations can be explained by the possible biases caused by ignoring endogeneity or inappropriately using lagged variables as instruments.

Focusing first on the variables of interest in column (3), it can be seen that there is a significant relationship between FDI and the exit hazard faced by DPFs. The presence of foreign firms in the same sector (*HFDI*) raises the probability of exit very notably. More specifically, the coefficient $\beta_{HFDI} = 0.341$ suggests that a 1 percentage point increase in *HFDI*, ceteris paribus, will induce an increase in the hazard of exit by $100(e^{0.341} - 1) = 40.6\%$.

This aggregate impact of horizontal FDI on the exit hazard is, as noted earlier, the sum of two effects: the negative competition effect and the potentially positive productivity spillover effect. However, since learning is generally not instantaneous, it is the former that dominates in the short run. The result confirms the static crowding out effect described in Kosova (2010). Louri, Peppas, and Tsionas (2006) and Wang (2010) found similar effects for DPFs in Greece and Canada, respectively, but Görg and Strobl (2003b) and Backer and Sleuwaegen (2003) found the opposite for Ireland and Belgium.

Kosova (2010) distinguishes the static crowding out effect from a dynamic crowding out effect that is related to the output growth of foreign firms in the same sector. The coefficient of the variable *GFDI* in column 3 is positive and significant, which suggests that an increase in foreign output will raise the exit hazard for domestic private firms. Hence, the dynamic crowding out effect also seems to be confirmed.

UpFDI and *DownFDI* reflect the impact of FDI from upstream and downstream sectors. The estimated coefficients show that they are somewhat larger (in absolute terms) than the replacement/competition effect of *HFDI*, but they have the opposite impacts. FDI in upstream sectors raises the exit hazard, but FDI in downstream sectors seems to reduce it. The finding that $\beta DownFDI = -0.408$ means that, ceteris paribus, a given 1 percentage point increase in the share of foreign presence in downstream sectors would lead to a decrease in the hazard rate by $(1 - e^{-0.408}) * 100 = 33.5\%$. By contrast, an increase in foreign presence in upstream sectors by 1 percentage point raises the exit hazard by nearly 60%.

While the positive impact of downstream FDI on the survival of DPFs could possibly be explained by demand creation (which may be particularly strong when foreign investors are export oriented) and the spillovers that come about when foreign firms buy local inputs and provide support for their local suppliers, it is more difficult to explain why upstream FDI seems to strongly reduce the life expectancy of DPFs. One possible channel of influence could be that foreign firms in upstream sectors are likely to crowd out local firms in the same sector, which in turn could harm the domestic firms in downstream sectors. This would be particularly serious if the

foreign firms in upstream sectors use technologies and manufacture intermediate goods that do not match the technologies and input requirements of local firms in later stages of the value chain.

Regarding the other survival determinants, it should be noted that international trade tends to lower exit hazards. The variable *EXPORT* has a large negative coefficient, and a higher export ratio for foreign-owned firms also tends to reduce the exit hazard. These results are as expected. The interaction variable *HFDI * EXPRATIO* also records a negative sign. The interpretation is that the horizontal crowding out effect is weaker in sectors where foreign firms are more export oriented. However, it is somewhat surprising that the variable *IMPORT* also records a significant negative coefficient, given that import competition was hypothesized to raise the competitive pressure on DPFs.

It is possible that the relatively high correlation between imports and exports at the 3-digit level makes it difficult to disentangle the separate effects of these two variables—the fact that the signs of several of the trade-related variables change between the different estimations could indicate collinearity. It can also be hypothesized that a high import ratio is a characteristic of sectors where domestic firms have already learned to manage tougher competition. The DPFs in these sectors can perhaps be described either as firms that have survived import competition for some time or new entrants that are aware of the tough market conditions. To explore the impact of imports in closer detail, it would be interesting to check whether there are any differences between sectors depending on whether their imports consist of final goods or intermediate goods. Unfortunately, lack of data on the use of imports makes it impossible to examine this distinction.

Market concentration, proxied by the Herfindahl index reduces the exit hazard; the more concentrated the market, the lower the probability that domestic firms will have to exit the market. A likely reason for this result is that incumbents have some market power that allows them to respond positively to foreign entry. Since high concentration is often a sign of high entry barriers, it is possible that the number of vulnerable firms—newly established young firms that could easily be squeezed out from the market—is also relatively small. The variable *NBR*, which proxies local concentration, also seems to reduce the exit hazard. This could be an indication of an agglomeration effect. The geographic variable measuring diversity, *DIVER*, also appears to reduce the exit hazard.

The explanation for the negative coefficient estimate of the variable *MSCALE* is similar to that for market concentration. *MSCALE* has a positive effect on survival that is consistent for all three specifications, confirming the argument by Audretsch (1995) that firms in sectors with high minimum scale seem to enjoy higher price-cost margins due to high entry barriers, raising survival rates. At the same time, high minimum scale is likely to mean that exit costs are also high, and that firms will not respond quickly to negative demand shocks or cost increases.

Turning to the variable *ENTRY*, the results indicate that a high entry ratio raises the exit hazard. This finding is consistent with Backer and Sleuwaegen (2003) and Wang (2010) and confirms the replacement hypothesis stating that the entry of a new firm will force inefficient older firms out of the market. In addition, a high entry rate is an indicator of low sunk cost, which suggests not only low entry costs but also low exit costs.

Looking at firm characteristics, there are some interesting points to be noted. Capital intensity does not have any significant impact on firms' survival, which may appear counterintuitive: high capital intensity could be seen as an indication of relatively high barriers to entry and hence high price-cost margins that reduce the exit hazard. However, Viet Nam is a labor-abundant rather than a capital-abundant country. DPFs in general are not likely to have strong competitive advantages related to capital intensive technologies. It is also notable that the relative size of the firm seems to raise the exit hazard, despite the a priori expectation that larger firms would be more resilient. A possible reason is that larger firms may be more vulnerable, e.g., because of higher debt levels, but we do not have access to the financial data needed to explore this further. The variable *AGE* has the expected positive impact on survival in all three estimations.[1]

C. Do SOEs Matter?

As mentioned earlier, the economy of Viet Nam is distinguished by the dominance of SOEs in many sectors. SOEs are not only focused on the provision of public services, but they also hold prominent positions in many other industries. This motivates an analysis of the role of SOEs in determining the survival of DPFs. One obvious reason is that SOEs may influence the survival of DPFs in the same way as FDI does—SOEs can also be assumed to be larger and stronger firms that dominate the smaller and weaker private actors. Moreover, the presence of SOEs may have a conditioning impact on the relation between FDI and DPF survival. Hence, we define four variables—*HSOE, DownSOE, UpSOE*, and *GSOE*—to represent SOEs presence in horizontal, downstream, and upstream sectors, as well as the growth of SOE output.

The expected effects of the SOE variables on DPFs are similar to those of the FDI variables, but not necessarily identical. The reason is that unlike foreign firms, SOEs do not always exhibit higher efficiency or higher productivity than DPFs, and they have fewer unique technological assets that could spill over to local firms (see further Nguyen et al. 2006 and Tran 2013). The backward and forward linkages between DPFs and SOEs may also differ from those between DPFs and

[1] To test the robustness of the results, we have also estimated the hazard function with some parametric models that also have PH properties, including Weibull, Exponential, and Cox models. The results of these tests (available on request) indicate that the findings discussed above are fairly robust to alternative assumptions about the hazard function.

foreign firms. In particular, DPFs may be more likely to select SOEs rather than foreign firms as suppliers or customers due to lower technical requirements and the similarity in business culture. This means that the linkages may be stronger or more extensive, although the potential technological advantage of SOEs—and hence the potential for learning and spillovers—is likely to be weaker.

Furthermore, because SOEs have existed in the market longer than the foreign firms and because they hold substantial market power, the foreign firms may choose different entry strategies and operational strategies in different sectors, depending on the market share of SOEs. This suggests the hypothesis that the survival effects from FDI may vary with the share of SOEs in the sector. This hypothesis is tested by introducing some interaction terms between SOEs and FDI in the empirical model.

In Table 6, column (1) presents the estimation results in which only the presence of SOEs and control variables are included. Column (2) includes both SOEs and FDI. Column (3) focuses on the FDI variables but adds a dummy variable for the quintile of sectors with the highest SOE shares and interacts it with the FDI variables, while column (4) adds a corresponding interaction variable for the quintile of sectors with the lowest SOE shares. These interaction variables are introduced in order to explore how the presence of SOEs influences the impact of FDI on local firms.

The control variables are robust across estimations, but are not included in Table 6 to save space (results are available on request).[2]

The results in column (1) show that SOEs have a significant effect on the survival odds of DPFs. The variable *HSOE* has the expected positive coefficient, but *UpSOE* records a negative coefficient, suggesting that relations with SOEs in upstream industries may benefit local firms. This is in contrast to the results for upstream FDI, which was found to raise the exit hazard. The difference presumably reflects the smaller technology gap between DPFs and SOEs. The coefficient for *DownSOE* is positive but not significantly different from zero, which is also somewhat surprising, given that the coefficient of *DownFDI* in Table 5 was negative and significant.

Another surprising result is that increases in SOE output (*GSOE*) seem to have dynamic crowding in effects on DPFs. The coefficient of *GSOE* in column (1) is –0.208, indicating that the hazard of exit declines by 18.7% for a 1 percentage point increase in the growth of SOEs. This impact becomes even stronger when foreign presence is included in column (2). This result is not easily explained unless the growth of SOE output is highly correlated with overall demand growth. The result could also be connected to the fact that SOEs have lost market shares in several industries and gone through a gradual privatization process during the period under study, which means that there are many sectors where SOEs record negative growth.

[2]One exception is the variable *AGE*, which is insignificant in some of the estimations reported in Table 6.

Table 6. **The Impact of SOEs and FDI**

	SOE (1)	FDI & SOE (2)	High SOE Dummy (3)	Low SOE Dummy (4)
HFDI		0.394***	0.710***	0.753***
		(9.63)	(32.67)	(18.61)
UpFDI		0.715***	0.774***	0.889***
		(16.91)	(20.06)	(19.22)
DownFDI		–0.951***	–0.624***	–0.449***
		(19.97)	(15.02)	(9.62)
HSOE	0.432***	0.381***		
	(24.43)	(11.54)		
UpSOE	–0.168***	–0.258***		
	(7.96)	(9.04)		
DownSOE	–0.060	0.333***		
	(1.663)	(9.67)		
GFDI		0.430***	0.390***	0.456***
		(22.85)	(18.49)	(23.83)
GSOE	–0.207***	–0.354***		
	(13.80)	(23.50)		
*HFDI * SOE_DUM*			0.397***	–0.153***
			(4.64)	(5.77)
*UpFDI * SOE_DUM*			–0.204***	–0.613***
			(3.91)	(11.05)
*DownFDI * SOE_DUM*			1.232***	–0.288*
			(6.00)	(2.55)
SOE_DUM			2.530***	–2.857***
			(4.88)	(7.50)
N	217,284	217,284	217,284	217,284
Log pseudo likelihood	–24,896.03	–24,530.54	–24,619.21	–24,539.41
Wald-test	3,697.232	5,518.01	4,521.25	4,537.00

* = significant at the 10% level, ** = significant at the 5% level, *** = significant at the 1% level.

Note: Cloglog model, dependent variable: dead1 (1 = firm exit, 0 = otherwise). All estimations are in coefficient form, not hazard ratios. Numbers in parentheses are z-values. Coefficients for regional/sectoral dummies are not shown. The High SOE dummy identifies the quintile of 3-digit sectors with the highest SOE shares of output. The Low SOE dummy marks the quintile with the lowest SOE shares. Coefficients for control variables are not shown to save space.

Source: Authors' computations.

Having established that SOEs do have an impact on local firms, it is interesting to examine whether the presence of SOEs may moderate or condition the survival effect of FDI. Column (2) adds the FDI variables to the estimation equation. The signs of the FDI variables remain unchanged, but the absolute size of the estimated coefficients increases: in particular, the vertical impacts of FDI appear to grow stronger. The impact of SOEs is also influenced by the inclusion of the FDI variables. The most notable change is that the coefficient linked to downstream SOEs becomes positive and significant, suggesting that the exit hazard increases if SOEs raise their share among the customers of DPFs. This result is not easily explained, and may be due to the gradual retreat of SOEs from some of the downstream industries. If so,

there could be a link between the coefficient estimates for *DownSOE* and *DownFDI*, although the correlation matrix in Appendix 3 suggests that they are not highly correlated.[3]

To explore the relations between the impacts of FDI and SOEs in somewhat closer detail (in a context where the possible correlation between the FDI and SOE variables is less of a concern), columns (3) and (4) add dummy variables for the 3-digit industries with the highest and the lowest (horizontal) SOE shares. In column (3), the dummy *SOE_DUM* distinguishes the quintile of sectors with the highest SOE shares. The direct effect is an increase in the exit hazard for DPFs, as seen from the positive and significant coefficient for *SOE_DUM*. All three interaction variables combining *SOE_DUM* with *HFDI*, *UpFDI*, and *DownFDI* are also significant.

For *HFDI*, the results suggest that the strong direct effect of horizontal FDI is even stronger in industries with high SOE shares—DPFs that are already pressured by SOEs are particularly vulnerable to further competition from foreign-owned firms. For vertical FDI, the direct effects seem to be smaller or even reversed in sectors with high SOE shares. In particular, it appears that the beneficial effects of downstream FDI are absent in the sectors that are most strongly dominated by SOEs.

In column (4), where the dummy variable identifies the sectors with the lowest SOE shares, the effects are of a different nature. First, the coefficient of *SOE_DUM* is negative and significant, suggesting that the exit hazard is smaller in these sectors. Second, the inclusion of the interaction term reduces the impact of *HFDI*. The direct effect of horizontal FDI is still an increase in the exit hazard, but this effect is somewhat weaker in the sectors with low SOE shares. Third, the effects of vertical FDI are less harmful (upstream FDI) or more beneficial (downstream) in the sectors with low SOE shares.

For the impact of horizontal FDI, the theoretical interpretation of the conditioning role of SOEs appears straightforward. The higher the share of SOEs, the tougher the baseline competition and the stronger the additional negative effect of *HFDI* on the survival odds of DPFs. It is more difficult to make any strong generalizations about how SOEs influence the vertical effects. The results for sectors with high SOE shares are unclear, both theoretically and empirically, and the results probably reflect differences in the capabilities of both DPFs and SOEs across industrial sectors. Further work is clearly needed to better understand these interactions. Yet, the observations that the presence of SOEs has an impact on the exit hazard for DPFs and that they also influence the impact of FDI on DPFs are important and have rarely been made in extant literature.

[3] If the market share of SOEs in downstream industries falls, it is possible that this could be reflected in an increase in the market share of foreign-owned firms (although the SOEs could also be replaced by DPFs). If this shift in market shares has an impact on the exit hazards for DPFs, it would be recorded as opposite effects for the retreating (SOEs) and expanding (FDI) investor groups.

Table 7. **Net Effect, Change in Hazard of Exit 2001–2008 (%)**

		FDI			SOE		
		Low Export	High Export	Avg.	Low Export	High Export	Avg.
Food processing	Horizontal	–5.62	5.92	2.83	7.04	1.39	2.92
	Backward	–2.49	9.37	6.52	0.57	13.87	10.15
	Forward	–2.52	–35.72	–29.38	–0.32	–4.14	–2.61
	Total	**–10.63**	**–20.43**	**–20.03**	**7.29**	**11.12**	**10.46**
Textile, leather, wood products	Horizontal	0.43	–5.50	–4.23	1.78	0.50	0.77
	Backward	–0.31	7.61	5.66	–2.19	5.39	3.52
	Forward	2.50	–1.20	–0.33	–0.79	–1.82	–1.58
	Total	**2.62**	**0.91**	**1.10**	**–1.20**	**4.07**	**2.71**
Metal products, machinery	Horizontal	2.19	–0.40	0.26	–1.30	1.72	0.80
	Backward	2.27	1.41	1.61	–0.62	–3.68	–2.39
	Forward	1.41	–5.08	–2.84	–1.27	–3.21	–2.70
	Total	**5.87**	**–4.07**	**–0.97**	**–3.19**	**–5.17**	**–4.29**
Electricity, energy	Horizontal	–6.91	15.54	3.35	6.08	8.55	6.47
	Backward	–2.13	–1.26	–1.98	–2.71	–2.57	–2.69
	Forward	10.85	14.14	11.26	–0.19	–1.66	–0.50
	Total	**1.81**	**28.42**	**12.63**	**3.18**	**4.32**	**3.28**
Other services	Horizontal	–0.01	–0.56	–0.24	–4.05	6.96	0.33
	Backward	–2.08	3.61	0.40	–2.43	1.21	–0.87
	Forward	–5.79	–7.08	–6.15	–0.31	–3.18	–0.88
	Total	**–7.88**	**–4.03**	**–5.99**	**–6.79**	**4.99**	**–1.42**
All industries	Horizontal	–0.54	–0.13	–0.28	0.48	2.97	1.81
	Backward	–1.48	4.13	1.57	–1.95	5.55	2.03
	Forward	–1.43	–8.25	–4.68	–0.43	–2.90	–1.60
	Total	**–3.45**	**–4.25**	**–3.39**	**–1.90**	**5.62**	**2.24**

Source: Authors' computations.

D. Net Effects of Changes in FDI and SOE Shares

It should be noted that all discussions so far have focused on marginal effects. The results show that effects of foreign presence on the survival of DPFs are remarkably large, they vary depending on whether the foreign firms are in the same or upstream/downstream sectors, and they are influenced by the presence of SOEs. In particular, there seems to be a strong and robust crowding out effect of horizontal FDI and SOE presence. To compute the net effects of changes in FDI during the period under study, the estimated coefficients from the model must be combined with the actual changes in the various forms of FDI and SOEs included in the model.[4]

Table 7 illustrates these net effects based on the coefficients in column (2) of Table 6. The effects from changes in both FDI and SOEs on the survival of DFPs are calculated. To take into account the heterogeneity of DFPs, we present the effects for five major sectors as well as the average effect on the domestic industry.

[4]Although cloglog is a nonlinear model, its PH property allows us to compute the net effects to the hazard ratio.

Moreover, noting the significant impact of international trade on exit hazards, Table 7 also presents separate estimations for the quintiles of 3-digit industries with the lowest and highest export shares in each industry group. All estimations are based on FDI and SOE shares at the 3-digit level. To facilitate an overview of results, the table shows the change in exit hazards between 2001 and 2008, rather than data for individual years. Because of the relatively large changes in FDI and SOE shares between individual years, with increases as well as decreases, there is substantial variation over time and across more disaggregated sectors, which complicates interpretation.

A first point to note is that the estimated net effects on changes in hazard rates are relatively small, considering the large marginal effects found in Table 6. The main reason is that the changes in FDI shares over the whole period have not been very large—both FDI and domestic industry have grown substantially, and a large share of the year-to-year fluctuations disappears when we look at the end points in the dataset.[5] Second, although the average impact of FDI is relatively small—a reduction in the exit hazard by about 3%—there are differences between the broad industry groups, as well as differences between more and less export-oriented subgroups of industries.

Generalizing, it appears that FDI has contributed more to reduce exit hazards in relatively simple industries like food products, while there has been some crowding out of local firms in more advanced industries such as electricity and energy. The effects also seem more beneficial in the more export-oriented industry groups, with the exception of the electricity and energy sector. Third, the average impact of changes in SOE shares is a small increase in the hazard of exit, although there are differences across sectors. There does not seem to be any immediate relationship between the technical complexity of the sector and the net impact of SOEs, nor is there any obvious link to the export orientation of the industry. Although the results confirm that SOEs do have an impact on the exit hazards facing DPFs, it is clear that further work in needed to gain deeper insights into this relationship.

VI. Conclusion

This paper has examined the survival effect of inward FDI on DPFs in Viet Nam. Recent literature suggests that the survival effects of FDI come from different sources that may sometimes have contradictory impacts. Firms that manage to absorb positive technological spillovers will face lower exit hazard thanks to improved productivity and efficiency. Positive effects also come from demand creation

[5] It should be noted that we do not include the dynamic crowding out effect from growth in foreign production in these estimations. The dynamic effect is a short-term phenomenon, and aggregating growth rates over many years yields results that are obviously not realistic.

connected to the presence of foreign firms in downstream sectors. In such cases, domestic firms may gain from increased possibilities to exploit economies of scale. However, the most frequently noted effect in the survival literature is the negative competition effect that occurs as foreign firms take market shares and force local enterprises to reduce output or cut prices in order to maintain their market shares. In either case, less efficient DPFs are likely to be forced to exit the industry.

The paper makes the following four contributions to extant literature. First, we have examined the survival effects from both horizontal and vertical FDI, while most of earlier studies focus on the survival impact of horizontal FDI alone. The results confirm that horizontal FDI is likely to crowd out local firms, but also suggest that the vertical effects are important and that omitting these effects may result in inappropriate conclusions about the overall impact of FDI.

Second, unlike earlier studies that consider domestic firms as a homogenous group, we highlight the role of SOEs for industry dynamics. The presence of SOEs apparently has a direct effect on the survival of DPFs—which is not surprising, considering the significant market shares and market power of SOEs in Viet Nam—but they also seem to have a conditioning impact on the relationship between FDI and the survival of DPFs. These preliminary findings stress the need for further study on the interactions between FDI, SOEs, and DPFs, particularly in transition economies where SOEs still play an important economic role.

Third, we have explicitly tried to manage estimation problems related to the endogeneity of covariates. In particular, we have found indications that both foreign presence and entry ratios may be endogenous. Earlier studies have generally assumed that covariates are exogenous or used lagged variables to try to control for endogeneity.

Fourth, apart from pointing to the partly offsetting effects of horizontal and vertical FDI on the survival of DPFs, we also attempt to calculate the net effect of foreign presence. Although the marginal effects are large and vary by year and industry group, we find a surprisingly small net effect.

What policy conclusions does a finding about increased exit hazards imply? There are two general interpretations of the possible welfare effects of the changes in industrial structure generated by FDI. A first perspective focuses on the vulnerability of DPFs in Viet Nam. The findings show that DPFs can suffer from both a remarkably large short-term crowding out effect and negative longer term effects caused by changes in upstream and downstream sectors. An almost instinctive policy response is to call for measures to strengthen the competitiveness of DPFs, in order to maintain a strong domestic industry sector and a high level of employment in domestic firms.

However, an alternative interpretation is based on an industrial efficiency perspective. How should the structure of domestic industries develop if domestic enterprises are to become more competitive in an increasingly open and internationally oriented market? It seems clear that the strength of the local private sector is not only measured by the number of DPFs in individual industries, but also by

Table 8. **Survival Time and Size (Employment)**

Years of Survival	Relative Size
1	0.87
2	1.00
3	1.12
4	1.27
5	1.47
6	1.71
7	2.03

Note: Size relative to 3-digit industry average.
Source: Authors' computations.

the size, productivity, and competitiveness of these firms. Moreover, flexibility and dynamism are increasingly important characteristics in the internationalized market place. Entrepreneurs need to be able to respond to market signals, moving towards industries and activities where market conditions are favorable and away from sectors where the returns to investment and work effort are lower.

Seen from this perspective, it is not obvious whether an increased exit hazard due to inward FDI is good or bad for domestic industry. In fact, an increased exit rate could even be favorable if it is part of a dynamic restructuring process, where weak firms exit and leave room for more efficient and productive enterprises that are able to grow faster. This suggests that the key questions are "Who are the survivors?" and "Are there enough survivors to maintain a high level of employment?"

A detailed analysis of the survivors lies beyond the objectives of this paper, but Table 8 provides a quick glance at one of the characteristics of surviving firms—size. The table presents the relative size of firms (based on the number of employees) across firms with different survival times. There is a consistent pattern where surviving firms quickly grow larger: the typical DPF that has survived 7 years is more than twice as large as the average firm in its 3-digit industry. This suggests that survivors have an opportunity to grow stronger and larger over time, and that the restructuring process that is triggered by FDI inflows is perhaps not detrimental to the domestic economy as a whole. At the same time, it is appropriate to recognize that more detailed studies of the dynamic effects of FDI are needed to better understand the differences between failing and surviving firms, particularly on whether and how resources used in failing firms are transferred to surviving companies.

A final point relates to the theoretical consequences of the finding that there is a systematic crowding out effect from FDI. As noted earlier, studies of the technology spillovers from FDI have resulted in contradictory findings, with positive as well as negative results reported in the extensive literature (Blomström and Kokko 1998, Görg and Greenaway 2004). One reason could be that the analyses are performed on samples that include both surviving firms and firms that are crowded out because of the competition from FDI. It is possible that the spillover effects estimated in such samples are poor descriptions for both types of firms. The enterprises that are

crowded out per definition do not benefit from any positive technology spillovers, and their inclusion in the sample may also obscure the true impact on surviving firms. This suggest that estimations of spillovers should perhaps be performed in two stages, with a first stage estimating survival and a second stage estimating spillover effects for those firms that are not crowded out by foreign presence.

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Appendix 1: Recent Studies on Survival Effects of FDI

Authors	Countries	Models	Covariates (Firm Variables)	Controls for Endogeneity	Data	Results
Wang (2010)	Canada	AFT	FDI, Export, Import, Entry rate, Industry dummies, Cohort dummies (Size, Ownership dummy, Multi-plant dummy)	No	Manufacturing 1973–1996	(–) Horizontal (+) Backward (+) Forward
Ferragina, Pittiglio, and Reganati (2009)	Italy	Cox	Herfindahl, MES, FDI (Size, Age, Relative labor productivity, Ownership dummy)	No	Manufacturing and services 2005–2007	(+) Services, (?) Manufacturing
Iurchenko (2009)	Ukraine	Cox	FDI, Export, Concentration, Region dummies, Sector dummies, Time dummies (Size, Capital intensity, Wages, Number of subsidiaries, Profitability)	Lag(1) for Herfindahl	Manufacturing 2001–2007	+
Bandick and Görg (2010)	Sweden	Cloglog	Industry dummies, Time dummies (Ownership, Size, Age, Multi-plant dummies, R&D intensity, Exports)	Scoring propensity and IVs	Manufacturing 1993–2002	+ /?
Kosova (2010)	Czech	Lognormal	FDI, Reform dummies, Industry dummies, Region dummies (Foreign capital share, Sales growth, Age, Size, Intangible assets, Technology gap, Solvency)	Dummies for time, region, sector	1994–2001	(–) Short term
Burke, Görg, and Hanley (2008)	UK	Cox	FDI, Concentration, Sectoral growth (Size)	No	Manufacturing 1997–2002	(+) Overall (–) Dynamic and (+) Static industries
Taymaz and Özler (2007)	Turkey	Cox	FDI, Entry rate, Growth, Prices, Imports, Exports, Herfindahl, MES, Time dummies, Industry dummies (Size, Employment growth, K/L, Advertising, Contracted input share, Contracted output share, Interest payments, Profit margin, Bonuses)	No	Manufacturing 1983–2001	(?)

Continued.

Appendix 1: *Continued.*

Authors	Countries	Models	Covariates (Firm Variables)	Controls for Endogeneity	Data	Results
Louri, Peppas, and Tsionas (2006)	Greece	Weibull, Cox, Exponential	FDI, Herfindahl (Inefficiency, Ownership, Age, K/L, Total assets, Leverage, Liquidity, Profit, Debt)	No	Manufacturing 1997–2003	(–)
Görg and Strobl (2003b)	Ireland	Cox	FDI, MES, Herfindahl, Employment growth (Size, Tech level, Age, Foreign dummy)	Sectoral dummies	Manufacturing 1973–1996	(+) All (+) High tech (–) Low tech
Backer and Sleuwaegen (2003)	Belgium	System (OLS) equation	Imports, Foreign entry, Foreign exit (Price cost margin, Sales growth)	No	Manufacturing	(–) Short run, (+) Long run
Alvarez and Görg (2005)	Chile	Probit	FDI, MES, Herfindahl (Size, Age, Productivity, Export dummy, Foreign dummy)	No	Manufacturing 1990–2000	(+) With productivity improvement
Girma and Görg (2003)	UK	Cox	Industry growth, Herfindahl, Region dummies (Age, Size, Ownership, Age at acquisition)	IV method	Electronic and Food industries 1980–1993	(–) Electronics (?) Food
Mata and Portugal (2002)	Portugal	Exponential Hazard	FDI, MES, Employment growth, Entry rate, Concentration, Industry growth (Labor quality, Size, Ownership form)	No	All firms 1983–1991	(?)
Dries and Swinnen (2004)	Poland	Probit	(FDI dummy, Vertical links, Size, Age, Education, Household characteristics)	Yes	Dairy farm data (1996–2000)	(+) Backward effect

AFT = accelerated failure time model, FDI = foreign direct investment, K/L = capital/labor, MES = minimum efficient scale, R&D = research and development, UK = United Kingdom.

Source: Authors' summary of the literature.

Appendix 2: Survival Probabilities of Domestic Firms (%)

Year	OECD[a]	US[b]	UK[c]	UK[d]	Turkey[e]	Viet Nam
1	–	93	99.2	75	83	87
2	–	–	86.0	–	78	79
3	71	–	76.0	–	69	73
4	–	–	69.7	55	60	66
5	–	67	–	–	–	50
10	–	54	–	–	40	–

– = not available, OECD = Organisation for Economic Co-operation and Development, UK = United Kingdom, US = United States.

[a]OECD. 2011. Entry, Exit, and Survival. In *OECD Science, Technology and Industry Scoreboard 2011*. Paris: OECD Publishing; average for firms in the cohort 2004.

[b]Agarwal, Rajshree, and David Audretsch. 2001. Does Entry Size Matter? The Impact of the Life Cycle and Technology on Firm Survival. *Journal of Industrial Economics* 49(1): 21–43; for US in period 1906–1990.

[c]Helmers and Rogers (2010); for UK in the period 2001–2006.

[d]Saridakis, George, Kevin Mole, and David Storey. 2008. New Small Firm Survival in England. *Empirica* 35(1): 25–39; for small sample survey in 1996–2001.

[e]Taymaz, Erol, and Sule Özler. 2007. Foreign Ownership, Competition, and Survival Dynamics. *Review of Industrial Organization* 31(1): 23–42; for the case of Turkey for the period 1983–2001.

Appendix 3: Correlation Matrix for Variables Used in the Model (simple correlations)

		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
1	*HFDI*	1.000																	
2	*UpFDI*	–0.471	1.000																
3	*DownFDI*	–0.366	0.392	1.000															
4	*HSOE*	0.385	0.048	–0.150	1.000														
5	*UpSOE*	–0.392	0.355	0.281	–0.138	1.000													
6	*DownSOE*	–0.185	0.457	0.331	–0.015	0.307	1.000												
7	*GFDI*	–0.144	0.106	0.041	–0.057	0.043	0.015	1.000											
8	*GSOE*	–0.213	0.228	0.165	0.008	0.090	0.088	0.289	1.000										
9	*EXPORT*	0.258	–0.051	–0.032	0.074	–0.185	0.020	–0.044	–0.060	1.000									
10	*IMPORT*	0.450	–0.338	–0.216	0.060	–0.274	–0.080	–0.140	–0.106	0.651	1.000								
11	*EXPRATIO*	0.528	–0.531	–0.206	0.012	–0.373	0.036	–0.120	–0.264	0.012	0.299	1.000							
12	*HERF*	0.213	–0.171	–0.253	–0.172	–0.170	0.015	–0.024	–0.048	0.073	0.160	0.191	1.000						
13	*NBR*	0.181	–0.124	–0.096	0.017	–0.107	–0.050	0.073	0.036	0.041	0.097	0.138	0.114	1.000					
14	*DIVER*	–0.045	0.006	0.034	–0.041	–0.051	0.018	–0.142	–0.042	0.025	0.046	–0.001	0.079	–0.507	1.000				
15	*MSCALE*	0.411	–0.271	–0.071	0.054	–0.258	–0.195	–0.159	–0.091	0.096	0.291	0.314	0.096	0.088	0.106	1.000			
16	*RELSIZE*	–0.130	0.068	0.028	–0.107	0.104	0.028	0.069	0.014	0.020	–0.037	–0.130	–0.012	0.072	0.133	–0.251	1.000		
17	*CAPINT*	–0.003	0.038	0.007	–0.014	0.012	0.060	0.012	0.039	0.024	–0.005	–0.018	0.059	0.090	–0.007	–0.058	0.075	1.000	
18	*AGE*	0.063	–0.028	–0.025	–0.014	–0.073	0.015	–0.013	–0.060	0.256	0.264	0.040	0.028	0.011	0.055	0.025	0.157	0.013	1.000
19	*ENTRY*	–0.295	0.282	0.318	–0.026	0.347	0.223	0.171	0.185	–0.122	–0.240	–0.244	–0.179	–0.012	–0.112	–0.146	0.091	0.026	–0.107

Source: Authors' computations.

Spatial Price Differences and Inequality in the People's Republic of China: Housing Market Evidence

Chao Li and John Gibson*

The large literature on regional inequality in the People's Republic of China (PRC) is hampered by incomplete evidence on price dispersion across space, making it hard to distinguish real and nominal inequality. The two main methods used to calculate spatial deflators have been to price a national basket of goods and services across different regions in the country or else to estimate a food Engel curve and define the deflator as that needed for nominally similar households to have the same food budget shares in all regions. Neither approach is convincing with the data available. Moreover, a focus on tradable goods such as food may be misplaced because of the emerging literature on the rapid convergence of traded goods prices within the PRC that contrasts with earlier claims of fragmented internal markets. In a setting where traded goods prices converge rapidly, the main source of price dispersion across space should come from nontraded items, and especially from housing given the fixity of land. In this paper we use newly available data on dwelling sales in urban PRC to develop spatially-disaggregated indices of house prices which are then used as spatial deflators for both provinces and core urban districts. These new deflators complement existing approaches that have relied more on traded goods prices and are used to re-examine the evidence on the level of regional inequality. Around one-quarter of the apparent spatial inequality disappears once account is taken of cost-of-living differences.

Keywords: housing, inequality, prices, spatial, People's Republic of China
JEL codes: E31, O15, R31

I. Introduction

The large literature on regional inequality in the People's Republic of China (PRC) is hampered by the limited evidence on price dispersion across space, which makes it difficult to distinguish real inequality from nominal inequality. Like statistical agencies in most countries, the PRC's National Bureau of Statistics (NBS) does not publish a spatial price index that allows cost-of-living comparisons over space. Instead, the focus is on the temporal consumer price index (CPI), which is reported

*Chao Li, PhD candidate, Department of Economics, University of Waikato. John Gibson, Professor, Department of Economics, University of Waikato and Motu. We are grateful to Geua Boe-Gibson, Yun Liang, Scott Rozelle, Niny Khor, two anonymous referees, and participants at the *Asian Development Review* conference for assistance and helpful suggestions.

at both the national and the provincial level. There are also separate CPIs for rural and urban areas at both national and provincial levels. These indices allow rates of change in the consumer price level to be compared across different locations but do not allow comparisons of absolute price levels or of the cost of living between locations.

However, there are good reasons to suspect that price levels and the cost of living vary over space. A higher price level is expected in more productive, richer economies (Balassa 1964, Samuelson 1964). The same pattern likely holds within countries because typically productivity growth is stronger in the traded sector than in the nontraded sector. If wages in the traded sector rise with productivity while nontraded sector wages are pegged to those in the traded sector (both sectors compete for workers in the same labor market), then prices of nontraded items will grow faster than productivity and will rise in real terms. The overall price level is an average of traded and nontraded prices so that in the context of regions of the PRC, one can expect a higher overall price level in export-oriented, coastal provinces in which nominal income is higher, such as Guangdong, than in poorer, inland provinces such as Yunnan.

The implications of this pattern are worth emphasizing in the PRC where there is substantial debate about the impacts of economic reform on inequality. A common claim in the literature is that spatial inequality rose in the reform era, especially when policy neglected the rural sector (Fan, Kanbur, and Zhang 2011). This claim has fueled initiatives to help seemingly laggard regions catch up to seemingly advanced regions, including the West China Development Project (Lai 2002), the Northeast China Revitalization Campaign (Zhang 2008), and the Rise of Central China Plan (Lai 2007). Just a subset of these initiatives saw more than one trillion yuan ($180 billion) of state-led infrastructural investment directed to western regions of the country (Yao 2009). But without reliable measures of spatial price differences, it is not clear how much of the reported spatial inequality (and its claimed increase) is simply due to regional price variation and how much reflects differences in real incomes.

In this paper, we use newly available data on dwelling sales in urban PRC to develop spatially-disaggregated indices of house prices which are used as spatial deflators for provinces, urban prefectures, and urban core districts. Since we account for only one source of cost-of-living variation over space, the impacts on inequality that we find when using these deflators should be considered a conservative, lower bound. Our approach contrasts with the two main methods previously used to calculate spatial deflators in the PRC where either a national basket of goods and services has been priced in different regions or a food Engel curve has been estimated and a deflator derived as that which is needed for nominally similar households to have the same food budget shares in all regions. Neither approach is convincing with the data available in the PRC, as we explain below. Moreover, a focus on traded goods such as food may be misplaced because of the emerging literature on the rapid convergence

of prices within the PRC that contrasts with earlier claims of fragmented internal markets.

It is increasingly reasonable to expect integrated goods markets in the PRC, and for goods prices to obey the law of one price (net of transport costs), but the same is not true of housing services. Because of the fixity of land supply, accounting for regional differences in housing service prices is fundamental to the calculation of spatial differences in the cost of living. While other services are also considered nontradable, the long-run supply of their dominant factor of production can spatially adjust to reduce interregional price differences. For example, if haircuts are relatively more expensive in urban areas of the Pearl River Delta, hairdressers might be expected to migrate to that region to increase the supply and reduce the regional price premium. There is no similar migration possibility for land—the presence of abundant land (relative to the population) in western regions and consequently relatively low house prices can do nothing to moderate the high cost of housing in Beijing.

Our focus on housing costs as the main driver of spatial cost-of-living differences is supported by previous studies in other countries. According to Moulton (1995, p. 181): "the cost of shelter is the single most important component of inter-area differences in the cost-of-living." Similarly, Massari, Pittau, and Zelli (2010) find housing prices account for almost 70% of cost-of-living differences between northern and southern Italy. Our approach is perhaps most closely related to Jolliffe (2006) who examines how adjusting for cost-of-living differences between metropolitan and nonmetropolitan areas in the United States (US) causes a complete reversal of the poverty ranking of these areas. In order to measure poverty using spatially deflated data, Jolliffe (2006) uses the fair market rent (FMR) index which consists of just two components: housing expenses (with a weight of 0.44) and all other goods and services (weight of 0.56). This index assumes that cost-of-living variation over space reflects variation in housing prices only and that there is no variation over space in the prices of all other goods and services.[1]

Although our results are most clearly relevant to scholars interested in the PRC, they also may have broader applicability. A growing international literature examines the impact of accounting for spatial price differences, especially those generated from urban housing markets, on apparent trends in nominal outcomes. For example, Moretti (2013) finds a more rapid rise in the cost of living experienced by college graduates compared to high school graduates accounts for one-quarter of the 1980–2000 increase in the nominal college premium in the US. This cost differential occurs because college graduates have increasingly congregated in urban areas with expensive housing (using monthly rent as a proxy for the user cost of housing).[2]

[1] Specifically, the FMR is based on housing prices for the poor, defined as the cost of gross rent (including utilities) at the 40th percentile for standard quality housing.

[2] In a related paper using the same housing cost data, Moretti (2010, p. 4) notes that empirical results are not sensitive to measuring housing costs by using the price of owner-occupied homes instead of using rental costs.

Similarly, Albouy (2012) shows how accounting for the higher real cost of living in US urban areas with more expensive housing provides revealed-preference estimates of the quality of life that are more consistent with popular "livability" rankings and stated preferences. The same effect is present in the Russian Federation, where Berger, Blomquist, and Peter (2008) estimate housing value and wage equations to impute implicit prices for city amenities. In their study, house values and nominal incomes are correlated over space and the implied quality of life rankings generated by the housing and labor markets are consistent with observed internal migration flows. The importance of housing markets for measured inequality and quality of life is emphasized by this literature.

The remainder of the paper is structured as follows. Section II reviews the literature in three areas that help to inform this study: spatial deflation studies, market integration studies, and housing market studies. Section III describes the data that we use to create housing-related spatial deflators for the PRC. One concern with using dwelling prices as an indicator of cost-of-living differences is that dwelling quality may vary systematically across space, so to address this issue we discuss, in section IV, the nature of real estate development in the PRC and provide some empirical evidence on the importance of location effects relative to dwelling characteristics in determining housing prices. Another concern is that dwelling prices may capture more than just the costs of shelter, hence we also contrast our approach with studies that rely on rental costs and describe recent trends in tenure patterns in urban areas of the PRC. The calculation of the deflators is described in section V and the results are contrasted with other spatial deflators for the PRC. The impact of using the deflators when measuring spatial inequality is discussed in section VI, while the conclusions are discussed in section VII.

II. Previous Literature

The approach we use here, of constructing spatially real income by deflating only for housing costs, relies on literature for the PRC that is drawn from three distinct areas: spatial deflation studies, market integration studies, and housing market studies. Our overall goal is to contribute to the literature on spatial inequality in the PRC by examining the impact of using various deflators on estimates of spatial inequality. We reviewed the spatial inequality literature in a recent study (Li and Gibson 2013), where the focus was on the misunderstanding that results from ignoring the fact that for most of the reform era, statistical authorities in the PRC denominated local GDP by the number of people with *hukou* household registration from each place rather than the number of people actually residing in each place (so that measured inequality mechanically increased as the number of non-*hukou* migrants rose). In the current study we use the adjustments to the population denominators created by Li and Gibson (2013) but otherwise do not

address population issues and instead pay attention to the impact of adjusting for spatial cost-of-living differences.

A. Spatial Deflation Studies

The most widely used spatial deflators for the PRC appear to be those of Brandt and Holz (2006).[3] The authors use provincial price data from 1990 to calculate the cost of national rural and urban expenditure baskets (containing 40–60 items) and a population-weighted combined basket. The prices had originally been collected by statistical authorities for the purpose of calculating a temporal index (the CPI) for each province, so that they do not necessarily refer to the same quality of items across provinces. Rural prices were not available for all products consumed in rural areas, so provincial capital city prices were instead used for items constituting just over 40% of the average rural household budget. Since there were no prices for nontraded services, average labor wages in township and village enterprises (TVE) were used as a proxy. Finally, the analysis lacked data on either rent, land prices, or real estate prices, therefore construction costs per square meter of rural household buildings were used in their place with the "quantity" of housing services in the basket set at 0.5625 square meters (m^2)—chosen to give an expenditure that was equivalent to nationwide per-capita rural household living expenditures on housing.

Brandt and Holz (2006) then use the annual rate of change in the CPI for each province to extend the 1990 spatial deflators for each province back to 1984 and forward to 2004. This time series is also used by other researchers studying inequality (e.g., by Sicular et al. 2007, Li and Gibson 2013) since it allows easy updating by just using published data on the annual rate of change in each province's CPI. Despite the simplicity, there are potential problems in using a temporal index to update a spatial index so as to create a panel of deflators. An example of such bias comes from the Russian Federation: Gluschenko (2006) compares a spatial price index calculated for period t using spatial prices for the same period, with an index for period t that is extrapolated from a spatial price index for period t_0 using local CPIs to update prices from t_0 to t. The direct method gives a spatial price index for each province whose range is 44% of the national mean price level, but the indirect method gives a much wider range, of 72%.

The example from the Russian Federation shows that CPI-updated price levels may not adequately proxy for cross-spatial price levels. More generally, it may not be possible to construct panel price indexes that are unbiased across both space and time (Hill 2004). The problem is that bilateral index formulas such as for the Laspeyres index used by Brandt and Holz (2006) are unlikely to give transitive results when extended to a multilateral situation. For example, consider a price index

[3] This paper has 152 Google Scholar citations as of March 2013.

calculated for three regions (Beijing, P_B; other urban areas, P_U; and rural areas, P_R) with base weights that differ in each region. A direct comparison between the rural price level in period t_2 and Beijing prices in period t_0 will not give the same result as constructing an indirect comparison via the third region in an intermediate time period, t_1. That is,

$$P_{R2,B0} \neq P_{R2,U1} \times P_{U1,B0}$$

Instead, transitivity requires use of a multilateral index method, such as the Geary-Khamis (GK) method that underlies the Penn World Table or EKS (Eltetö, Köves, and Szulc) type methods.[4]

Another issue with the deflator formed by Brandt and Holz is the use of a national basket rather than letting consumer responses to relative prices and other differences induce regional variation in the structure of consumption. While sensitivity to consumer responses is a claimed feature of the "no-price" Engel curve method described below (Gong and Meng 2008), it is not required that methods using disaggregated price data ignore variation in the structure of consumption. For example, Deaton and Dupriez (2011) use unit values from household surveys to calculate spatial price differences in two other large countries—Brazil and India—using multilateral Törnqvist indexes that are the geometric average of price relativities between each region and the base region, weighted by the arithmetic average of the budget shares for the two regions. Hence, variation in the structure of consumption, as captured in budget shares for each region, is accounted for by this type of spatial price index. The results for these two countries show a 20% range in average food prices between the cheapest and most expensive regions in India, while in Brazil there is almost no price gradient, reflecting the higher incomes in Brazil and hence greater importance of processed foods which likely have much smaller price margins between regions than do unprocessed foods.[5]

Gong and Meng (2008) use an Engel curve approach to estimate spatial price deflators for each province using data from the Urban Household Income and Expenditure Survey from 1986 to 2001.[6] Their deflator is defined by what is needed for nominally similar households to have the same food budget shares in all regions

[4] These methods compare each country (or region) with an artificially constructed average country (or region). Typically they use the Paasche price index formula to make each of these bilateral comparisons with the artificial country as the base and tend to suffer from substitution bias because the price vector of the base artificial country (region) is not equally representative of the prices faced by all of the countries (regions) in the comparison. EKS methods impose transitivity in the following way: first, they make bilateral comparisons between all possible pairs of countries and then take the nth root of the product of all possible Fisher indices between n countries. Deaton and Dupriez (2011, p. 4) note that multilateral price indexes required for spatial work are typically not consistent with the inflation rates in local CPIs and so need to be calculated regularly, not just once, and updated by the local CPIs.

[5] Relatedly, supermarkets are more important in Brazil (and also in the PRC) than in India, and the growth in the importance of supermarkets assists with spatial convergence in food prices (Reardon et al. 2003).

[6] In contrast to the later work of Almås and Johnsen (2012), Gong and Meng (2008) do not create a panel price index of time–space deflators, and instead the food Engel curves are estimated separately for each year.

following an idea first proposed by Hamilton (2001) for measuring bias in a temporal CPI. These authors find implied regional cost-of-living differences from the Engel curve that are considerably larger than those calculated from pricing a fixed basket using either provincial average prices or household-level unit values. The difference from fixed basket results was most apparent during the mid- to late-1990s when social welfare reforms altered coverage and subsidies for public health, education, and housing. In terms of inequality, when no adjustment was made for spatial price differences, Gong and Meng (2008) find that the mid-1990s saw the most significant increase in regional income inequality, but after using the deflator derived from their Engel curve results, they find regional income inequality to actually increase the most in the late 1980s.

Almås and Johnsen (2012) use a similar Engel curve approach with data from just 2 years (1995 and 2002) for rural areas in 19 (of 31) provinces and urban areas in 11 provinces. Rather than estimating a spatial cost-of-living index year by year, they attempt to make incomes comparable over both time and space using a single set of Engel curve estimates. Based on this procedure, these authors claim that the CPI understates price changes in rural areas and overstates them in urban areas: the deflator derived from the Engel curve suggests a 44% rise in the rural cost of living from 1995 to 2002 and zero change in the urban cost of living compared to CPI increases of 8% and 11%, respectively. The use of this Engel curve deflator closes the rural–urban gap in terms of price levels, with the rural cost of living rising from 60% of the urban level in 1995 to 87% of the urban level by 2002. Thus, the real income figures calculated with their deflator show a greater rise in inequality and a more modest fall in poverty than is implied by making no spatial adjustment and using the CPI for temporal deflation.

The studies that use a food Engel curve to back out regional differences in the cost of living (or more generally the bias in any spatial or temporal deflator) are one strand in a broad literature that relies on observable proxies for well-being to calculate implicit compensation for people living in different circumstances (such as family size and structure, or location). For example, Timmins (2006) uses internal migration data from Brazil under the logic that moves reveal preferences over locations that differ in terms of nominal incomes and the cost of living and can thereby reveal spatial differences in the cost of living. Lanjouw and Ravallion (1995) use child anthropometric indicators (stunting and wasting) in addition to food share to indicate well-being when anchoring their calculation of allowances for household size economies (effectively, the inverse of the compensation needed by people living in smaller households to be as well off as those in larger ones at the same per capita consumption). Subjective data on self-rated welfare can also be used. Krueger and Siskind (1998) and Gibson, Stillman, and Le (2008) use survey questions that compare feelings of being better-off in the present or the past to adjust for possible biases in the CPI, and the same method could be used to make spatial comparisons.

The problem with all of these approaches is that it is simply an assertion that the welfare indicator—whether food budget shares, anthropometrics, and so forth—does indeed identify people who are equally well off. At least since Nicholson (1976), a long literature has argued that food share is not a good indicator of well-being. Consider the example of using food share to calculate the exact amount of money needed for parents to maintain their consumption while providing for a child: Since child consumption is concentrated more on food than is adult consumption, the food share would be higher even if exact compensation had been given, and this higher food share would wrongly indicate the need for further (over)compensation.

In the context of the food Engel curve estimates for the PRC, there is a substantial difference between provinces and between urban and rural areas in the proportion of household members who are children. The data from the latest wave of the China Health and Nutrition Survey (CHNS) show 0–15 year old children comprise just 3% of the average household in urban areas of Liaoning province but comprise 16% of the average rural household in Guangxi. Food shares will thus be higher in Guangxi even if there were no differences in the cost of living, but the Engel method will not necessarily recognize this.[7] Consequently there are reasons to doubt the reliability of spatial deflators produced by this method.

B. Market Integration Studies

Many authors consider the PRC an example of a developing country with segmented markets and much less integration than developed countries (Gong and Meng 2008, Xu 2002). In the early reform period, this description may have been apt since economic interaction between provinces had been minimized during the planned economy era, making the PRC more like a cluster of independent economies rather than a large, spatially integrated economy.

But the surprising claim of some influential studies is that market integration declined even more during the reform period. According to Young (2000, p. 1128)

> (T)wenty years of economic reform . . . resulted in a fragmented internal market with fiefdoms controlled by local officials whose economic and political ties to protected industry resemble those of the Latin American economies of past decades.

The claimed reason for the seemingly perverse fragmentation of the internal market while the PRC opened up internationally is that devolution of powers saw

[7] Adding demographic variables to the Engel curve regression may not help since there is no reason for these effects to operate as just intercept-shifters. The literature using food Engel curves to study bias in temporal deflators is more credible since it typically restricts attention to a particular household type (say, two adults with two children). The change in household structure over a decade or so is much less than the differences over space, yet all of the regional differences are rolled into a catch-all term that is assumed to be due to just cost-of-living differences.

local government revenue linked to local industry protection, leading to interregional trade wars. Apparent confirmation comes from Poncet (2005) who examined "border effects" between provinces by comparing volumes of intraprovincial and interprovincial trade. The trade-reducing impact of provincial borders appeared to increase between 1992 and 1997, from which the study concluded that the domestic economy was fragmented and that "rather than a single market, (the PRC) appears as a collection of separate regional economies protected by barriers" (Poncet 2005, p. 426).

A critical reappraisal shows that the evidence from Young (2000) is not robust and that the PRC is comparable to the US in terms of being a relatively integrated, large economy (Holz 2009). For example, Young showed a rise in the (natural logarithm of the) interprovincial standard deviation of (the natural log of) prices of various consumer and agricultural goods, which was taken as evidence of trade barriers segmenting markets. But this calculation was neither robust to inflation nor to the growth in product variety in the reform period. Once Holz (2009) accounts for these factors there is no trend in interprovincial price dispersion, and the range of variation matches that in intercity data for products in the US. Similarly, Young found a convergence in the output structure of each province during the reform period, taken as evidence of provinces duplicating each other's industries rather than allowing regional specialization. The degree of convergence in the composition of value-added across US states in the same period was approximately the same as for provinces in the PRC, but there were no claims of rising interstate trade barriers in the US at that time.

In keeping with the reappraisal by Holz (2009), a number of more recent studies find the PRC to be a relatively well integrated market economy. Fan and Wei (2006) apply panel unit root tests to data on monthly prices for a group of 93 industrial products, agricultural goods, other consumer goods, and services in 36 major PRC cities, finding that prices do converge to the law of one price. Similarly, Ma, Oxley, and Gibson (2009) use spot energy prices in 35 major cities to test for convergence with their panel unit root tests indicating that the energy market is integrated in the PRC. Huang, Rozelle, and Chang (2004) examine prices for rice, maize, and soybeans from almost 50 locations in 15 provinces on the eve of the PRC's accession to the World Trade Organization. These authors find most market pairs to be integrated (and this integration to extend down to village level) and market integration to be substantially higher than even 5 years earlier.[8] A longer term perspective on grain prices found that on the eve of the industrial revolution, market integration in the PRC was as high as it was in most of the advanced areas of western Europe (Keller and Shiue 2007a), while contemporary markets are even

[8]Rising integration is also apparent in the labor market. Since 1997, urban wages in the PRC's interior provinces have risen at a faster rate than in coastal regions—although the absolute wage gap continues to grow (Li et al. 2012).

more integrated. Keller and Shiue (2007b, p. 107) conclude that for the PRC "in the late twentieth century local and national prices essentially move one-to-one." Thus, it is mainly the central planning era that deviated from the pattern of the PRC being a normal, relatively integrated, large economy.

Another way to examine market integration is to test how long it takes prices to converge following idiosyncratic shocks. For example, Parsley and Wei (1996) find convergence rates to purchasing power parity of 5 quarters for tradable goods and 15 quarters for services, for a sample of 48 cities in the US. When the comparable approach is used in the PRC, convergence rates appear to be much faster. Lan and Sylwester (2010) study the prices of 44 products in 36 PRC cities and estimate the half-life of divergences from the law of one price averages just 2.4 months. This is approximately twice the speed of adjustment found in the US, leading these authors to conclude: "(O)ur findings suggest that prices within [the People's Republic of] China converge to relative parity extremely quickly" (Lan and Sylwester 2010, p. 231).

A recent review of product, labor, and capital market integration in the PRC summarizes the evidence as showing: "(P)roduct markets became more integrated over time, as regional trade increased and product prices were increasingly similar throughout the country" (Chen et al. 2011, p. 73). Given this similarity over space of the prices of tradable goods, the focus of many of the previous spatial deflation studies summarized above may be misplaced. In an environment where traded goods prices converge rapidly, the main source of price dispersion across space should come from the nontraded components of consumption and especially from housing, given the fixity of land. We therefore briefly review the literature on spatial variation in house prices before turning to the data that we use to develop housing-related deflators.

C. Housing Market Studies

In the planned economy era, government agencies such as work units provided all urban housing. Rents were low and the dwelling one was allocated depended on administrative criteria such as job rank (Bian et al. 1997). Housing reform was launched in 1988 with privatization and creation of an urban housing market as the aim (State Council 1988). Thereafter, commodity houses built by private developers could be bought on the housing market (Huang and Clark 2002). For the first decade of reform, a dual track system developed with large numbers of commodity houses bought by work units and then distributed to workers at discounted prices (Huang 2003). In 1998, the State Council abolished the old housing system completely, and thereafter any provision of subsidized housing by work units was strictly banned (State Council 1998, Huang 2003). Since then, the urban housing system has become totally market oriented.

In contrast to the urban sector, rural houses were self-funded, self-built, and self-renovated by residents, and remain so until now (Liu 2010). The right to use

rural residential land (*nongcun zhaijidi shiyongquan*) is evenly distributed and free of charge for village collective members. Land is collectively owned by the village and the occupant is not allowed to mortgage or trade the land, although transfers within the village collective community are permitted. The occupant may build new houses or renovate old houses with their own funds for all kinds of needs such as marriage, tourism (*nongjiale*, akin to a motel, for urban tourists to taste rural life), family workshop, and handicraft production (Liu 2010). Thus, the rural housing system enables rural residents to satisfy their housing needs at much lower cost than is incurred by urban residents in the current era. Though rural self-built houses are generally large and cheap, they are poor in quality relative to urban housing in terms of housing attributes such as the energy source for cooking, bath facilities, and individual toilets (Logan, Fang, and Zhang 2009).

The reforms have led to a large literature on urban housing in the PRC, with early studies on determinants of home ownership (Huang 2003, Pan 2004). But after the full marketization of urban housing in 1998, the focus shifted to affordability due to the sharp increases in house prices. For example, the Shanghai Housing Price Index (SHHPI) of the China Real Estate Index System (CREIS) rose by 63% within 2 years from January 2001 (Hui and Shen 2006). Liu, Reed, and Wu (2008) document poor housing affordability in Beijing during the 2000s using the house price to income ratio (PIR) and the home affordability index (HAI). The PIR is defined as the ratio of the average market value of a typical dwelling to the average annual household income and the HAI measures the ability of a household with an average income to pay back a mortgage on a typical home. In a more comprehensive study, Xiang and Long (2007) calculate PIR and HAI indices for 34 major cities and find Beijing, Shanghai, Shenyang, Xiamen, and Haikou to have poor housing affordability, while the inland cities of Hohhot, Changsha, Chongqing, and Urumqi have relatively better housing affordability.

In addition to affordability, the other focus of recent literature on the urban housing market is price determination. Zhang and Tian (2010) study sales of new dwellings in 35 major cities between 1995 and 2006, finding stable long-run intercity price relativities, which implies that the urban housing market in the PRC is segmented and that specific local economic characteristics matter. Deng, Gyourko, and Wu (2012) examine land auctions for 35 major cities from 2003 to 2011 to construct a model of land supply and also for use in a hedonic model of dwelling prices, finding that house prices are driven by the land market rather than by construction costs. Zheng, Kahn, and Liu (2009) estimate a hedonic house price regression for 35 major cities and find significant location effects in determining prices. Wu, Deng, and Liu (2012) use a similar model but examine the role of intracity locational factors (e.g., distance to city center). Overall, this research indicates the importance of location in determining dwelling prices in urban PRC, with the most plausible source of inter-area variation coming from land prices.

III. Data

For our main analysis, we use administrative data on the average selling price for new residential dwellings that real estate developers are required to report to the NBS. Specifically, every transaction for new housing sales is meant to be reported (both monthly and annually, directly to the NBS through an electronic portal). These are the most commonly used data for studies of the PRC urban housing market (Zheng, Kahn, and Liu 2009). Since most of the housing market is new construction rather than repeat sales (Deng, Gyourko, and Wu 2012), an index derived from prices of new units is broadly representative. The average selling price is given for each province in the *China Real Estate Statistics Yearbook* (NBS 2011a), while for urban prefectures the statistics are found in the *China Statistical Yearbook for Regional Economy* (NBS 2011c). For urban core districts (which are more consistently urban than the prefecture they belong to), the numbers are reported for 2009 (but not 2010) in the *China Urban Life and Price Yearbook* (NBS 2010).[9]

We obtain data on average GDP for every province, every urban prefecture, and every urban core district from the *China Statistical Yearbook for Regional Economy* (NBS 2011c) and the *China City Statistical Yearbook* (NBS 2011b). These same two sources provide information on the value of total urban real estate investments on residential assets (IRA). The data on the resident population, which are needed for correct calculation of per capita values (rather than using the misleading registered population figures), are year-end 2010 figures for provinces from NBS (2011c) and are 1st November 2010 figures for prefectures and districts as reported in the county-level tabulations of the 2010 Census of Population (NBS 2012).

In addition to these data provided by the NBS, we gathered our own data on sales prices and attributes of new apartment units from www.Soufun.com, which is the largest real estate listing site in the PRC. In conjunction with the CREIS, Soufun.com co-publish the *China Real Estate Statistical Yearbook*. For the primary data collection, we only considered the dominant type of urban residence which is a private apartment in a complex. We did not consider subsidized public rental housing, economically affordable housing, and high-grade apartments and villas, which are just minor components of the urban housing system. According to the *China Real Estate Yearbook 2011*, of 8.82 million new urban housing units sold in 2010, just 2.5% were high-grade apartments or villas and 3.7% were economically affordable housing. The other 94% were standard private apartments, and so our primary data collection concentrated on this dominant form of urban housing.

[9] Subsequent editions of the *China Urban Life and Price Yearbook* after 2010 do not report house price data for urban core districts, hence we use the 2009 values as reported in 2010.

IV. The PRC Urban Housing Market and Price Determinants

If dwelling quality varies systematically over space, then it may interfere with using published average new dwelling selling prices as an indicator of standardized housing costs for urban areas. However, real estate development in the PRC is organized such that systematic quality differences between cities are unlikely, since many apartment complexes in different cities are developed by the same national-level real estate development companies (sometimes even using the same names for their complexes in each city). While each complex may have dozens of multistory towers, each containing more than 50 individual housing units, within a complex there are only a few (typically less than 10) floor plans available and the selling price in terms of yuan per square meter varies little across the individual units. But there is considerable variation in selling price between complexes in different areas, including between different districts of the same city. For example, Beijing has 16 city districts, and complexes in different Beijing districts may have prices that vary by up to CNY30,000 ($4,800) per square meter. This variation is consistent with the finding of Deng, Gyourko, and Wu (2012) that variation in new dwelling prices is driven by the land market.

In order to verify if dwelling quality varies systematically over space, we gathered data in February 2013 on sales prices for 150 new apartments in three cities. Each city is from a different level of the administrative hierarchy: (i) Beijing is a municipality-level city with an equivalent status to a province; (ii) Nanjing is the capital of Jiangsu province and is one of 15 subprovincial cities, which have much greater autonomy and higher status than prefecture-level cities; while (iii) Changsha is a prefecture-level city and the capital of Hunan province. The data collection was restricted to these three cities because advertisements from most of the 323 cities in Soufun.com lack data on key attributes (both unit and complex characteristics). The majority of advertisements list only the average selling price of all units in a complex, but for the three selected cities, the unique price (per square meter) for every apartment in a complex is consistently listed. Furthermore, the advertisements always list the complex opening date, completion date, and the proportion of units sold to date (the sales ratio) only for these three cities, while for other cities these data are missing. Previous research has found that these factors play a significant role in determining new apartment prices because they represent changing pricing behavior of the real estate developer at different stages to completion of an apartment complex (Wu, Deng, and Liu 2012). We sampled prices from 3 to 5 complexes for each of the 13 districts of Beijing, 5 to 8 complexes from each of the nine districts of Nanjing, and 5 to 12 complexes from each of the five districts of Changsha.

The data used for the hedonic apartment price regression are described in Appendix A. For some characteristics, apartments in Nanjing and Changsha appear to have more desirable qualities than those in Beijing, with more green space and

a higher proportion of the complex area being green space (despite the complexes in Nanjing and Changsha rising higher, on average, than those in Beijing). Also, the listings for Changsha are for slightly newer complexes than for Beijing, as seen from the fewer months elapsed since the complex was opened for sale and the greater number of months to completion of the complex. On the other hand, the apartments in Beijing in the sample are larger than those in Changsha, which is likely to be a desirable characteristic showing up in higher prices even when we concentrate on the price per square meter. The apartment complexes from Beijing also have a higher car park ratio (the number of car parks per dwelling)—note that these are rented or sold separately, while most observations for Nanjing and Changsha leave this attribute blank so it is unclear if car parking is bundled with the price of the apartment in those cities. Overall, there is no clear sign that Beijing apartments have better quality relative to those in the other two cities. For example, the new trend in the real estate market in urban PRC of developers selling decorated new houses rather than unfinished ones is just as apparent in all three cities.

The results of the hedonic house price regressions are shown in Table 1. The dependent variable is the logarithm of the price (in thousands of yuan) per square meter so that the relative difference in prices is not directly shown by the regression coefficients on the dummy variables for each city. Instead, the coefficients must be transformed into percentage differences using $percentage\_difference = (e^{\hat{\beta}} - 1) \times 100$, which shows that the price per square meter is 84% higher in Nanjing than in Changsha, and 256% higher in Beijing without controlling for any attributes of the apartment (first column of Table 1). The results in the second column of the table use the attributes of each apartment but do not consider the location. Despite having 15 characteristics that are potentially related to selling prices, these explain slightly less of the variation in prices than just using location dummy variables.

When the apartment characteristics are put together, the hedonic regression explains 84% of price variation, and after controlling for all of the characteristics of the particular apartment and its complex, the relative price differences are fairly similar to what they were without the controls. Specifically, the (conditional) price per square meter is 105% higher in Nanjing than in Changsha and 229% higher in Beijing. While the price premium is slightly smaller for Beijing than when using the raw data, it is somewhat larger for Nanjing and this reflects the fact that, at least for these three cities, there is no systematic quality gradient whereby apartments in cities with higher priced real estate have more desirable attributes of either the unit or the apartment complex. In the absence of the sort of apartment-specific data that we used in the regression, we proceed to use raw data on average selling prices for all cities and we treat the spatial variation in these raw prices as mainly reflecting the fixity of land supply rather than systematic variation in dwelling quality.

Table 1. **Effect of Location and Dwelling Characteristics on House Prices in Urban PRC**

	Natural log (price of apartment unit in CNY1,000 /m^2)		
	(1)	(2)	(3)
Beijing = 1, otherwise = 0	1.270		1.190
	(15.28)***		(13.85)***
Nanjing = 1, otherwise = 0	0.610		0.716
	(7.34)***		(9.59)***
Unit characteristics			
Apartment area (m^2)		0.005	0.002
		(4.75)***	(2.60)**
Number of bedrooms		–0.432	–0.191
		(5.80)***	(3.82)***
Number of bathrooms		0.348	0.283
		(4.38)***	(5.50)***
Number of living rooms		0.030	0.127
		(0.29)	(1.89)*
Decorated = 1, otherwise = 0		0.337	0.256
		(3.93)***	(4.71)***
Level (floor) in complex		–0.011	–0.005
		(1.44)	(0.97)
Complex characteristics			
Land area (1,000 m^2)		–0.002	–0.001
		(2.13)**	(1.62)
Total number of floors		–0.002	0.003
		(0.34)	(0.83)
Floor area ratio		–0.104	0.072
		(2.57)**	(2.51)**
Green area (1,000 m^2)		0.005	0.003
		(2.07)**	(1.56)
Green area / total area		–3.035	–0.466
		(4.18)***	(0.95)
Car park ratio		0.006	0.018
		(0.06)	(0.29)
Months after opening		–0.008	–0.012
		(1.09)	(2.48)**
Months to completion		–0.017	–0.019
		(2.79)***	(4.79)***
Sale ratio		0.043	0.060
		(0.39)	(0.70)
Constant	1.945	4.039	1.584
	(33.08)***	(11.04)***	(5.48)***
R-squared	0.61	0.59	0.84

* = significant at 10%, ** = significant at 5%, *** = significant at 1%, m2 = square meter.
Note: Absolute value of t statistics in parentheses for regressions where $N = 150$. The omitted location is Changsha.
Source: Authors' computations from data in housing sample collected by authors in February 2013 from www.Soufun.com.

A. Rental Equivalence Approach

Before turning to the evidence on average selling prices of new dwellings, we discuss an alternative approach to forming standardized housing costs—the

rental equivalence method. In some Organisation for Economic Co-operation and Development (OECD) countries, temporal price indices for the services provided by owner-occupied dwellings are based on the imputed value of shelter for owners that are calculated as equivalent to what they forgo by not renting out their homes. In the case of the CPI for the US, this measure was adopted in 1983 in place of the previous measure based on house prices, since it was argued that prices did not accurately reflect the costs of shelter since they also include the use of a house as an asset. There is no guarantee that the rental equivalence method produces lower costs than do price methods, and indeed in the US between 1983 and 2007, the monthly principal and interest payment needed to purchase a median-priced existing home increased by only one-half as much as the increase in shelter prices indicated by the rental equivalence method.[10] But to maintain consistency with temporal deflators, many spatial cost-of-living studies in the US rely on rents rather than on house sales prices (e.g., Moretti 2013).

Despite the arguments for the rental equivalence approach, three reasons lie behind our decision to use the selling prices of new dwellings. First, we note that in some OECD countries (e.g., New Zealand) the price of housing services for owner-occupiers in the CPI is based on new housing sales, with the value of the net increase in the stock of owner-occupied housing during the reference period providing the expenditure weights. These components reflect the change in the price of housing acquired by the owner-occupier segment of the household sector, which is analogous to the approach that we use below and is particularly applicable to the situation in urban areas of the PRC since so much of the market is supplied by new housing rather than resale of existing dwellings.[11] Second, observed rents in urban areas of the PRC may not be an appropriate basis for pricing the rental equivalence of owner-occupied dwellings because of the low share of rented dwellings (Ahmad 2008). Finally, in contrast to the situation for prices of new dwellings, there are no comprehensive statistics on rents reported by the NBS on a spatially disaggregated basis so that any attempt to implement the rental equivalence approach would be limited in scope and so could not inform national-level estimates of inequality. Moreover, when considering variation in house prices and rents over space, the same fundamental driver—land prices—affects both, whereas for the temporal variation studied by much of the literature, factors such as interest rates may create a wedge between house prices and rents.

[10] These figures come from the Bureau of Labor Statistics online publication "Common Misconceptions about the Consumer Price Index: Questions and Answers" available at: http://www.bls.gov/cpi/cpiqa.htm

[11] Even urban dwellings built as recently as the 1979–1999 period are being dismantled to make way for new development because they do not meet the standards of modern urban affluence in the PRC (since they are either too small or lack desirable facilities). See "Most Homes to be Demolished in 20 Years," *China Daily*, 7 August 2010. Available at: http://www.chinadaily.com.cn/china/2010-08/07/content_11113982.htm

Table 2. **Number of Urban Households of Various Tenure Types (millions)**

Dwelling Tenure Type	Census 2000 (long form)			Census 2010 (long form)		
	Highly Urbanized	Very Highly Urbanized	Total	Highly Urbanized	Very Highly Urbanized	Total
Self-built	9.3	9.0	18.3	12.1	5.9	18.0
	(40.2%)	(18.8%)	(25.8%)	(28.2%)	(9.9%)	(17.5%)
Purchased	8.1	23.0	31.0	17.8	33.2	51.0
	(35.0%)	(48.0%)	(43.7%)	(41.5%)	(55.4%)	(49.6%)
Rented	4.7	13.4	18.2	11.1	17.9	29.0
	(20.4%)	(28.1%)	(25.6%)	(25.8%)	(29.9%)	(28.2%)
Total Households	23.1	47.8	70.9	42.8	60.0	102.8

Note: "Highly urbanized" refers to counties or districts where more than 70%, but less than 90%, of the population are urban residents. "Very highly urbanized" refers to counties or districts where 90% or more of the population are urban residents. There are 583 counties or districts in the 2000 census in these categories and 596 in the 2010 census. The column total number of households includes "other tenure types" which are not reported in the table.

Source: NBS (2003, 2012).

B. Trends in Urban Tenure

To help put our choice of using selling prices rather than rents in context, we describe here the trends in urban tenure based on data from the "long form" population census (answered by 10% of the population, which we gross up to total population counts). The available data are reported at county or district level so we categorize according to the urban population as a percentage of the county or district population and restrict attention to the most urbanized counties and districts (being 70% or more urban), distinguishing "highly urbanized" with 70%–90% urban from "very highly urbanized" with $\geq$ 90% urbanized. In total, there were 71 million urban households in 2000 and 103 million in 2010 under these definitions.

The first trend is that the share of urban households living in self-built accommodation has fallen considerably, from over one-quarter of the total in 2000 to just one-sixth by 2010 (Table 2). This trend is most apparent in very highly urbanized areas, where households in self-built dwellings declined by 3 million over 10 years and are now under 10% of the total (down from 19% in 2000). This pattern most likely stems from rising land values—for example, Wu, Gyourko, and Deng (2012) calculate that real, constant quality land values in Beijing rose by 800% from 2003 to 2010. Under such land price pressures, self-built dwellings are likely to be undercapitalized in the sense of being too small and having inadequate facilities relative to a new dwelling that would be appropriate for such land values. The flip side of the falling share of self-built dwellings is a rising share of purchased dwellings, which are the majority form of tenure (Table 2). Moreover, the rate of new construction, of approximately 8 million new standard private apartments each year, is equivalent to about one-sixth of the existing stock of purchased urban dwellings. The preponderance of new stock in the owner-occupied portfolio means that our focus on the price of new apartments is appropriate.

The final tenure category in Table 2 is renters, who have also seen a rise in numbers, although only half as large as the increase in the number of purchasers. However, what is not shown in Table 2 is that that rental sector in urban areas of the PRC is quite different from the owner-occupied sector, mainly housing poor rural–urban migrant workers (Wu 2012) and youth (Zhu 2013, Ouyang, 2011) in dwellings that are older and of lower quality than the dwellings that are being purchased. For example, we gathered data on apartment rentals in Beijing from Soufun.com and found the listed dwellings to have an average age of 10 years, which is much older than the dwellings that were for sale.

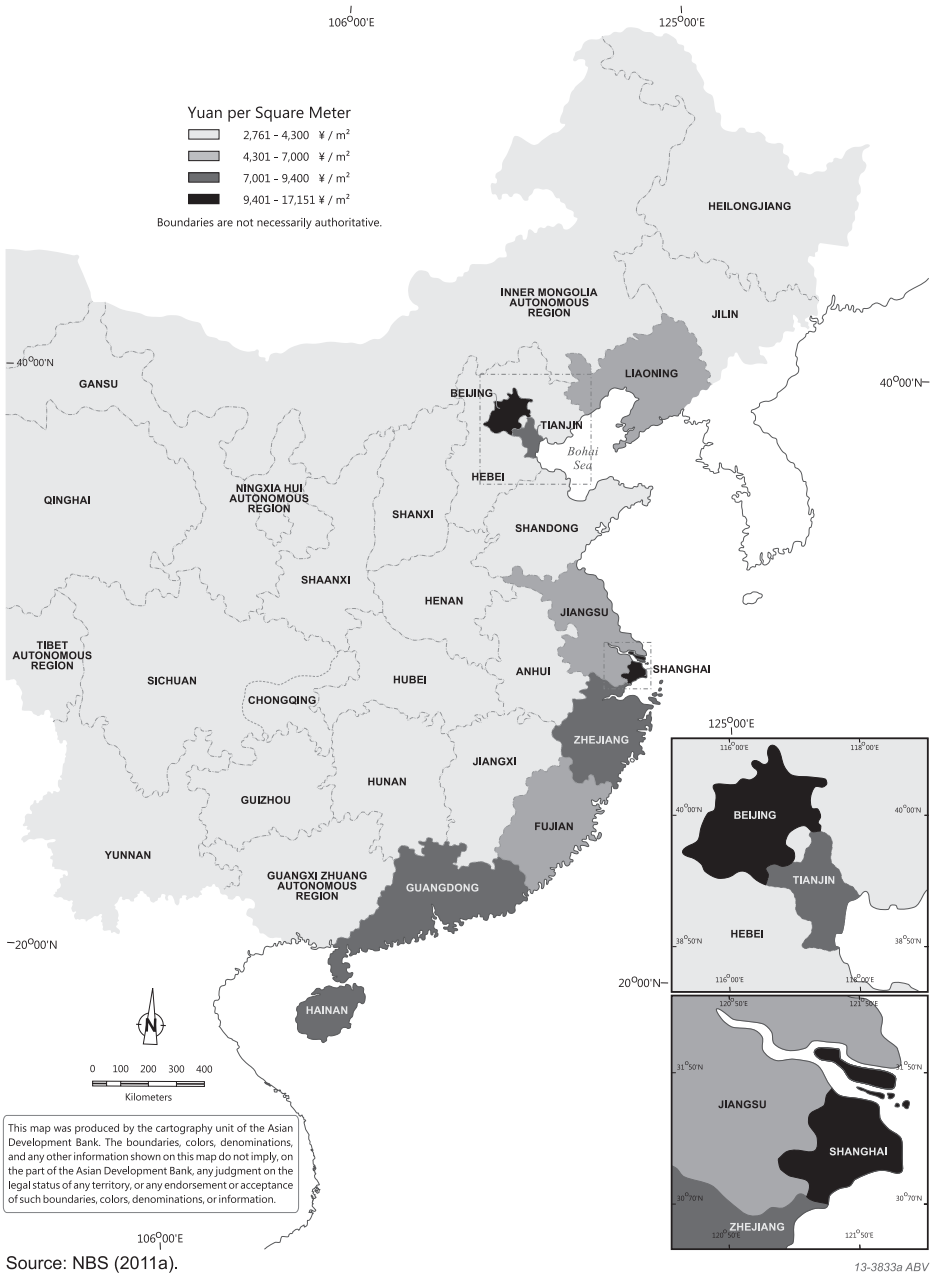
V. Housing Prices and Estimated Deflators

Since there is tentative evidence that purchased new apartment quality does not vary systematically between cities, we go ahead and use data from the *China Real Estate Statistics Yearbook* (NBS 2011a), *China Statistical Yearbook for Regional Economy* (NBS 2011c), and the *China Urban Life and Price Yearbook* (NBS 2010) on the average selling price in 2010 (provinces and urban prefectures) and 2009 (urban core districts) of new residential dwellings. We note that these data are for the urban sector, and our expectation is that these prices vary over space most especially because of intercity land price variation. For this reason we do not consider rural housing since rural residential land use rights are not determined by market forces and also because the data available for rural households are just the construction costs (building materials) which we consider to be traded goods and therefore less likely to vary over space than do urban house prices. The distinction between the urban and rural housing sectors is clearly seen in the way that the statistical system reports the relevant data—rural household expenditure on new dwelling construction is defined as consumption expenditure in the *China Rural Statistical Yearbook 2011* (NBS 2011d) while urban household expenditure on house purchases is defined as a separate category apart from consumption expenditure in the *China Urban Life and Price Yearbook 2010* (NBS 2010).

The average prices for new urban housing in 2010 are displayed in Figure 1, at provincial scale. The highest prices are found in Beijing (CNY17,150 per square meter) and Shanghai (CNY14,290 per square meter). The next highest category of prices (CNY7,001–CNY9,400 per square meter) are only one-half as expensive as those in Beijing, and are found in Tianjin, Zhejiang, Guangdong, and Hainan. In general, the highest prices are found in a continuous belt of provinces along the coast between Jiangsu and Hainan and in the Gulf of Bohai. All of the remaining provinces fall into the lowest price category, which includes all interior provinces plus the coastal province of Shandong.

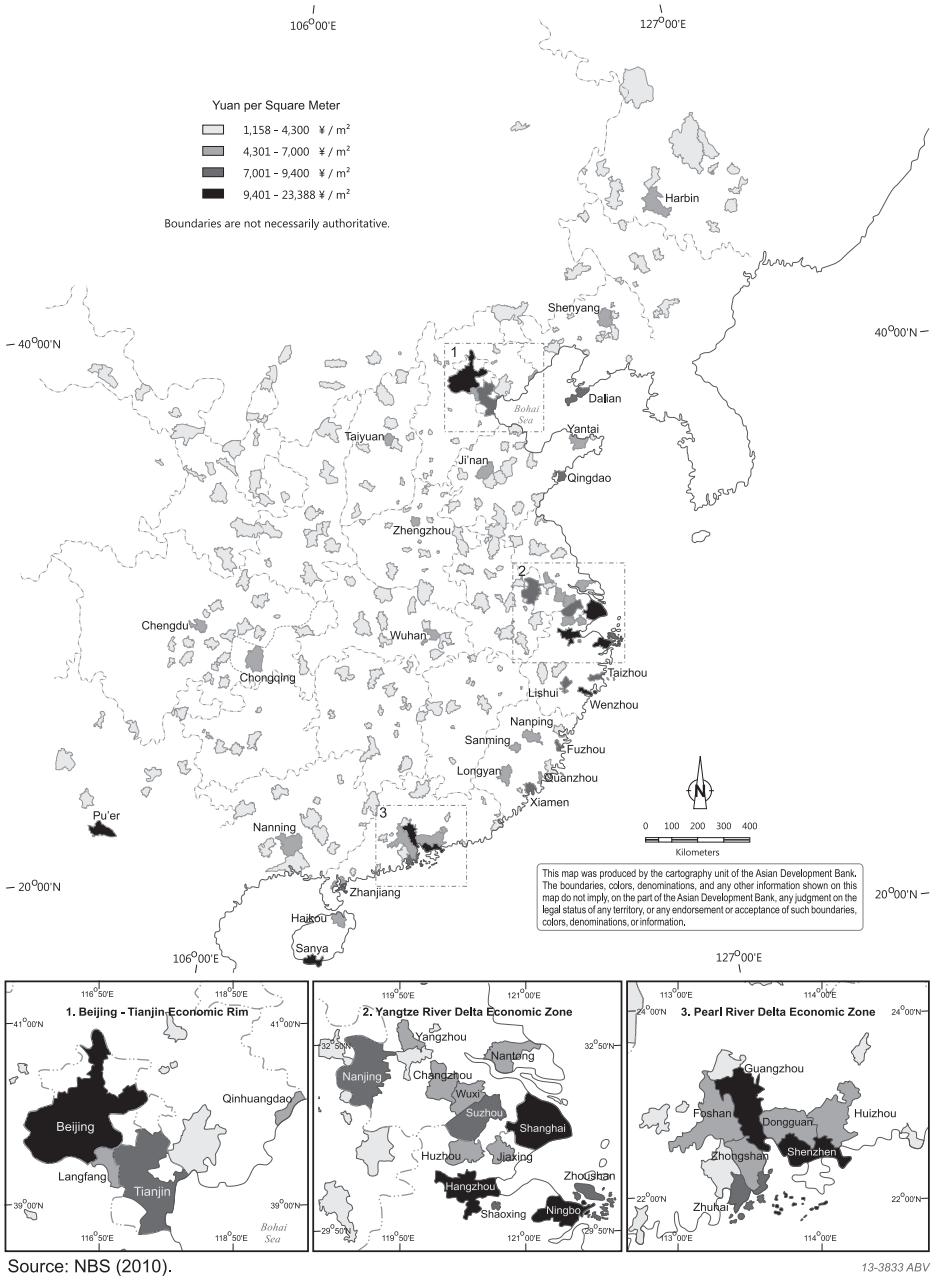
There is considerable heterogeneity within provinces since many of them are as large and as populous as independent countries. Therefore, Figure 2 provides

Figure 1. **Provincial Average Prices for New Urban Housing, 2010**



Source: NBS (2011a).

Figure 2. **Average Prices for New Housing in Urban Core Districts, 2009**



Source: NBS (2010).

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a finer-scale view of urban house prices, reporting the average value in 2009 for each of the 288 core urban districts. These core districts lie within prefecture-level and subprovincial cities, but are more consistently urban than the full area of the prefecture, which often includes rural counties. In order to concentrate on the region where most core urban districts are located, the map truncates Xinjiang, Tibet, and Qinghai in western PRC. This region contains only two urban districts—Karamay and Urumqi (both in Xinjiang). It is apparent that there are a number of cities in interior provinces such as Chengdu, Harbin, Ji'nan, Taiyuan, and Wuhan with much higher prices than revealed by the provincial average. Pu'er in Yunnan even falls into the highest price category shared by cities such as Guangzhou, Hangzhou, and Shenzhen, in addition to Beijing and Shanghai. Conversely, it is also apparent that there are cities in the coastal provinces with much lower prices than some cities in the interior. Consequently, the variation in the cost of living will be more accurately portrayed at subprovincial levels.

In order to measure cost-of-living differences over space, we calculate a Törnqvist price index for each province (and also for each urban prefecture and urban core district):

$$T = \exp\left[\sum_{j=1}^{J}\left(\frac{s_{kj}+s_{ij}}{2}\right)\ln\left(\frac{P_{ij}}{P_{kj}}\right)\right]$$

where s_{ij} is the average share that item j has in consumption in region i, and s_{kj} is the average budget share in region k, which is the base region, while P_{ij} and P_{kj} are the prices of item j in region i and in the base region. The Törnqvist index uses the arithmetic average of the budget shares in the base region and in region i to weight the logarithm of the price relativities between those two regions. These weighted price relativities are then summed over all J items that comprise the budget.

Our working assumption is that only house price variation contributes to cost-of-living differences, so as to form a lower bound for the impact of deflation on spatial inequality. Since it is assumed that prices do not vary spatially for all other components of the budget, the index formula reduces to the log house price relativity between Beijing (base region) and region i, weighted by the average importance of housing in Beijing and region i. There are no micro data on household budget shares on housing that can be disaggregated to subprovincial levels so we instead use national and regional accounts data. Spatially disaggregated annual investments in urban residential assets are published by the NBS, and since the urban housing market is dominated by new housing stock rather than repeat sales (Deng, Gyourko, and Wu 2012), this annual investment should be a good proxy for the component of regional income set aside for housing provision. However, one further adjustment is needed because of the famously low share of final consumption in GDP for the PRC, which varies across provinces because of differing intensities of net exports.

Table 3. **Residential Investment, Final Consumption Expenditure, Average New House Prices, and Deflation Indices at Province Level, 2010**

Province	FIRA	FCON	HP	Tornqvist	DINXB&H
Beijing	0.11	0.56	17,151	1.00	1.00
Tianjin	0.06	0.38	7,940	1.15	1.19
Hebei	0.09	0.41	3,442	1.40	1.52
Shanxi	0.05	0.44	3,338	1.29	1.28
Inner Mongolia	0.07	0.39	2,983	1.39	1.40
Liaoning	0.13	0.40	4,303	1.43	1.40
Jilin	0.08	0.41	3,495	1.36	1.42
Heilongjiang	0.06	0.53	3,492	1.28	1.38
Shanghai	0.07	0.55	14,290	1.03	0.99
Jiangsu	0.08	0.42	5,592	1.24	1.30
Zhejiang	0.07	0.46	9,332	1.11	1.34
Anhui	0.13	0.50	3,899	1.40	1.45
Fujian	0.07	0.43	6,077	1.21	1.37
Jiangxi	0.06	0.47	2,959	1.33	1.43
Shandong	0.06	0.39	3,809	1.30	1.42
Henan	0.07	0.44	2,856	1.37	1.50
Hubei	0.07	0.46	3,506	1.32	1.35
Hunan	0.07	0.47	3,014	1.35	1.24
Guangdong	0.06	0.47	7,004	1.16	1.12
Guangxi	0.09	0.51	3,382	1.35	1.35
Hainan	0.20	0.46	8,800	1.23	1.09
Chongqing	0.14	0.48	4,040	1.42	1.64
Sichuan	0.09	0.50	3,985	1.32	1.42
Guizhou	0.07	0.63	3,142	1.30	1.27
Yunnan	0.09	0.59	2,893	1.36	1.20
Tibet	0.01	0.64	2,761	1.21	1.22
Shannxi	0.09	0.45	3,668	1.36	1.26
Gansu	0.05	0.59	2,938	1.28	1.29
Qinghai	0.06	0.53	2,894	1.32	1.17
Ningxia	0.11	0.49	3,107	1.43	1.31
Xinjiang	0.05	0.53	2,872	1.30	1.29

DINXB&H = deflation index of Brandt and Holz (2006) updated to 2010, FCON = final consumption expenditure as a fraction of GDP, FIRA = investments in urban residential assets as a fraction of GDP, HP = average selling price of urban commercial new house units (yuan per square meter), Tornqvist = deflation index used by authors.

Sources: Authors' computations from data in NBS (2011a and 2011c).

We therefore use the ratio of annual investments in urban residential assets to final consumption expenditure as our proxy for the budget shares in the Törnqvist formula.[12]

Table 3 contains the provincial Törnqvist indexes calculated under these assumptions along with the input data used. The base region is Beijing and the index values are interpreted as the factor by which nominal GDP per capita in region *i* has to be multiplied to translate it into Beijing prices. On average, GDP per capita in

[12]The share of final consumption expenditure in GDP is not available for prefectures and urban districts, so we use the share for the province that the prefecture or district is part of, as an approximation.

provinces outside of Beijing has to be raised by 30% to make it comparable to GDP per capita at Beijing prices. The deflator ranges from 1.03 for Shanghai—whose residents face housing prices almost as high as in Beijing—to 1.42 for Chongqing and 1.43 for Liaoning. It is notable that the lowest average housing prices do not always give the lowest calculated price index because the importance of housing also matters. For example, house prices are low in Gansu but the inflation factor is lower than average because of the relatively low importance of provision for residential housing in regional income.

The last column of Table 3 reports the deflator from Brandt and Holz (2006) using the national basket, which is updated to 2010 using movements in each province's CPI. The Brandt and Holz deflator is more variable than the Törnqvist index, with an unweighted coefficient of variation across provinces more than one-third higher than for the Törnqvist index. This pattern is consistent with Gluschenko (2006), who found that calculating a spatial deflator just once and updating it with the local CPIs can overstate the spatial variation in prices. Nevertheless, the overall level of adjustment needed to put GDP outside of Beijing into Beijing prices is quite similar, with an average inflation factor of 32%. The cross-province patterns of the deflators are also quite similar, with a Pearson correlation coefficient of 0.71 and a rank-correlation of 0.63.

VI. Impacts of Deflation on Spatial Inequality

Our overall goal in carrying out the analysis reported here is to examine how much difference is made to estimates of spatial inequality in the PRC when using deflators derived just from variation in housing costs. The results are summarized in Table 4, which reports three measures of inequality—the Gini coefficient, the Theil index, and the weighted coefficient of variation (CoV)—for three levels of geography (province, urban prefecture, and the urban core districts within urban prefectures).[13] The nominal values that are deflated are GDP per resident in 2010, which takes into account the various corrections to both GDP statistics and population denominators that are summarized in Li and Gibson (2013). We restrict attention to 2010 because of the need for census data to provide correct counts of the resident population (rather than the *hukou*-registered population) for subprovincial spatial units.

If no account is taken of spatial variation in the cost of living, the level of spatial inequality is overstated by up to 35% (for interprovincial analysis, using the

[13] The Theil index is: $T_w = \sum_{j=1}^{m} (p_j/P)(y_{wj}/\mu)\ln(y_{wj}/\mu)$ where $m = 31$ provinces (or 288 prefectures or urban core districts), p_j is the population of the j^{th} province (or prefecture or district), P is overall population, y_{wj} is GDP *per capita* of the j^{th} province (or prefecture or district), and μ is the overall population-weighted mean of GDP *per capita* for all provinces. The (weighted) coefficient of variation is: $CoV = \sqrt{\sum_{j=1}^{m} (p_j/P)(y_{wj} - \mu)^2}/\mu$. The Gini coefficient is: $G = \left(\sum_{i=1}^{m}\sum_{j=1}^{m} p_i p_j \left|y_{wi} - y_{wj}\right|\right)/2\sum p_i^2\mu$.

Table 4. **Interregional Inequality in GDP per Capita with and without Spatial Deflation**

	THEIL	THEIL(D)	CoV	CoV(D)	GINI	GINI(D)
Province	0.08323	0.06147	0.42332	0.35521	0.22672	0.19790
Prefecture	0.17442	0.14105	0.62303	0.55361	0.33106	0.29865
Districts	0.11026	0.08512	0.46545	0.40836	0.26059	0.22552

CoV = population weighted coefficient of variation, (D) = inequality measure on GDP per resident with spatial housing cost deflation, GDP = gross domestic product, GINI = Gini coefficient, THEIL = Theil index.
Note: Results are for 31 provinces, 288 prefectures, and 288 prefecture-merged districts (prefecture urban cores).
Source: Authors' computations from data in NBS (2010; 2011a, b, and c; 2012).

Theil index). This is two-thirds larger than the impact of spatial deflation found by Li and Gibson (2013) who use the deflator from Brandt and Holz (2006), updated to 2010 with the rise in each province's CPI. Since the current analysis assumes that prices for all goods other than housing are set on perfectly integrated markets, it should provide a lower bound to the impact of spatial deflation if a "full" deflator was used which considered all components of consumption.

The lowest proportionate overstatement from not deflating comes when studying urban prefectures. This most likely reflects the fact that these spatial units have the highest apparent level of inequality amongst the various levels of disaggregation presented in Table 4, due to their heterogeneity. An urban prefecture may contain rural counties and this lack of a consistently defined urbanity gives higher apparent inequality between these "urban" units, and so correcting for spatial price differences has less impact. The more defensible level of subprovincial analysis is the urban core district within an urban prefecture, since this excludes rural counties. At this level of geography, spatial inequality is overstated by 14% (using the weighted coefficient of variation) to 30% (using the Theil index) if differences in the urban cost of living are not taken into account.

VII. Conclusions

In this paper we use newly available data on dwelling sales in urban PRC to develop spatially-disaggregated indices of house prices which are used as spatial deflators for provinces, urban prefectures, and urban core districts. Since we account for only one source of cost-of-living variation over space, the impacts on inequality that we find when using these deflators should be considered a conservative, lower bound. Previous approaches to forming spatial deflators for the PRC have focused more on traded goods prices, but our interpretation of the recent evidence is that traded goods prices adjust quickly to parity levels and so are unlikely to cause long-run cost-of-living differences between areas. In contrast, the fixity of land makes housing the most likely source of price dispersion across space.

It would be ideal to generate regional components of house prices that hedonically adjust for all components of dwelling quality, but such data are not available

beyond a limited number of cities. Nevertheless, our limited analysis suggests that systematic variation in the quality of new dwellings between cities is unlikely, making the published data on the average price of newly constructed urban dwellings a potentially useful source of information on spatial cost-of-living differences. When we use this information to adjust nominal GDP per resident we find that around one-quarter of the apparent spatial inequality disappears once account is taken of cost-of-living differences. Since there are good theoretical reasons for expecting a higher price level in nominally richer areas, our results provide a caveat to concerns about the degree of spatial inequality experienced in the PRC.

Our results are consistent with literature from other countries which finds that apparent patterns in nominal outcomes may weaken or reverse once account is taken of spatial price differences emanating from urban housing markets. The current research may help compare the spatially deflated level of real inequality in the PRC to that in other countries, but we believe that any altered inferences due to the deflation we propose are most relevant to temporal comparisons. The legacy of central planning and the *hukou* registration system meant that urbanization and urban housing development in the PRC were much less advanced at the beginning of the reform era than would be expected. Consequently, the spatial cost-of-living differentials now being caused by the urban housing market (reflecting the fixity of land) are likely to have grown from a very low base, making interpretation of trends in nominal inequality in the PRC atypically sensitive to assumptions about spatial and temporal differences in the cost of living.

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Appendix A: Comparison of House Prices and Attributes (means)

	Beijing	Nanjing	Changsha
Unit Price			
(CNY1,000 /construction m^2)	29.93	13.93***	7.13***
Unit Characteristics			
Area (m^2)	152.69	131.60	115.69***
Number of bedrooms	2.64	2.78	2.74
Number of bathrooms	1.76	1.40**	1.54
Number of living rooms	1.88	1.94	1.94
Decorated = 1, otherwise = 0	0.36	0.26	0.32
Level (floor) in complex	3.86	7.56***	7.18***
Complex Characteristics			
Land area (1,000 m^2)	149.10	198.65	198.76
Total number of floors	13.74	20.24***	25.78***
Floor area ratio	2.42	2.28	3.51***
Green area (1,000 m^2)	47.73	84.55	87.15**
Green ratio	0.32	0.39***	0.42***
Car park ratio	1.08	0.88**	0.97
Months after opening	15.14	12.42	11.98**
Months to completion	1.94	4.72	5.50**
Sales ratio	0.23	0.78***	0.45***
Observations	50	50	50

* = significant at 10%, ** = significant at 5%, and *** = significant at 1% for testing difference in mean compared with Beijing, CNY = yuan.

Sources: Housing sample collected by authors in February 2013 from www.Soufun.com

Effects of Monetary Policy Shocks on the Exchange Rate in the Republic of Korea: Capital Flows in Stock and Bond Markets

Soyoung Kim*

Several studies have suggested that the prediction of standard theory on the effects of monetary policy on the exchange rate might not be applicable to or in the case of the Republic of Korea because participation of foreign investors is weak in the bond market but strong in the stock market. The current study examines the effects of monetary policy shocks on the exchange rate in the Republic of Korea by using structural vector autoregression models with sign restrictions. To determine the channels by which monetary policy shocks affect the exchange rate, I investigate the effects on various components of capital flows. The main empirical findings are as follows. First, a contractionary monetary policy shock, which increases the interest rate, appreciates the Korean won significantly in the short run as predicted by most theories. Second, contractionary monetary policy shocks increase capital inflows into the bond market consistent with the prediction of the uncovered interest parity condition. This seems to be the main channel by which contractionary monetary shocks appreciate the won. Finally, foreign investors tend to withdraw money from the domestic stock market in response to a monetary tightening, resulting in a decrease in capital inflows.

Keywords: monetary policy shocks, vector autoregression, sign restrictions, exchange rate, capital flows
JEL codes: F31, F32, F33, F36

I. Introduction

Since the 1990s, many emerging Asian economies have liberalized capital accounts and opened financial markets to foreign investors. Foreign investors to date own a huge amount of stocks in emerging markets. Thus, international capital flows into domestic stock markets have become important sources of exchange rate instability as evidenced by the recent global financial crisis. However, foreign ownership in some emerging Asian bond markets, for example, the Republic of Korea, is still negligible.

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In advanced countries, foreign investors actively participate in bond markets. In some countries, they even participate in bond markets more actively than they do in stock markets. For example, a huge amount of United States (US) debt securities (e.g., Treasury bills) is owned by foreigners. In contrast, foreign investor ownership is limited in the US stock market. Such a difference in foreign investor participation in stock and bond markets may generate an important difference in the transmission of structural shocks in emerging and advanced countries. The current study investigates one interesting aspect of structural shock transmission, that is, the effects of monetary policy shocks on the exchange rate of an emerging Asian country.

Most international monetary and macro models predict that a contractionary monetary policy shock, which increases the interest rate, leads to domestic currency appreciation, other things being equal. Traditional Mundell–Fleming–Dornbusch models and recent New Open Economy macroeconomic models incorporate a version of the interest parity condition. This implies that, other things being equal, an increase in the domestic interest rate leads to domestic currency appreciation because the expected return on domestic bonds (denominated in domestic currency) becomes relatively higher than the expected return on foreign bonds (denominated in foreign currency). Therefore, the relative demand between domestic and foreign currency increases net capital inflows into the domestic bond market and appreciates the domestic currency against the foreign currency.

A number of studies have empirically tested such a standard prediction for advanced countries (Eichenbaum and Evans 1995, Kim and Roubini 2000, Kim 2003, Faust and Rogers 2003, Kim 2005, Scholl and Uhlig 2009). These studies found that a contractionary monetary policy shock leads to domestic currency appreciation, a finding consistent with standard theory, although the shape of exchange rate responses does not perfectly match the standard theoretical predictions implied by the uncovered interest parity condition.

However, if the domestic bond market is not fully developed and foreign investors do not actively participate in the domestic bond market of emerging countries, the standard channel through the domestic bond market may not function well. An increase in the domestic interest rate may also lead to a depreciation of the domestic currency through the stock market channel. Foreign investors may withdraw money from the domestic stock market as contractionary monetary shocks have adverse effects on the domestic economy. Capital outflows in the stock market may lead to exchange rate depreciation. Several studies (Kim and Ryou 2001, Lee and Ryou 2006) examined a reduced form or a simple timing relation between the interest rate and the exchange rate in the Republic of Korea. The results suggest that a rise in the interest rate corresponds to Korean won depreciation or that interest rate appreciation does not significantly affect the exchange rate.[1]

[1] Several studies (Radelet and Sachs 1998; Stiglitz 1999; Wade 1998; Dekle, Hsiao, and Wang 2002; Ohno, Shirono, and Sisli 1999) suggested that a high interest rate policy increases the interest burden of highly leveraged

To clarify this issue, this study examines the effects of monetary policy shocks on the exchange rate in the Republic of Korea. Although previous studies have examined the reduced form or simple timing relation between the interest rate and exchange rate, a reduced from or a simple timing relation between the interest rate and exchange rate can be generated from structural shocks other than monetary policy shocks. The current study employs a structural vector autoregression (VAR) model with sign restrictions developed by Uhlig (2005) to identify exogenous shocks on monetary policy and examine the effects of identified shocks on the exchange rate. This study also examines the effects of monetary policy shocks on various components of capital flows, such as capital inflows (liabilities) and outflows (assets) in stock and bond markets, to determine the mechanism by which monetary policy shocks affect the exchange rate.[2]

The rest of the paper is organized as follows. Section II explains the empirical methodology. Section III provides the empirical results on the effects of monetary policy shocks on exchange rate. Section IV discusses the detailed transmission mechanism by examining the effects of monetary policy shocks on various components of capital flows. Section V concludes and presents a summary.

II. Empirical Method and Data

A. Structural Vector Autoregression Models with Sign Restrictions

To identify exogenous monetary policy shocks and examine the effects of the identified shocks on the exchange rate, structural vector autoregression (VAR) models with sign restrictions (Uhlig 2005) are used. Past studies on the effects of monetary policy have frequently used structural VAR models, which are effective in identifying exogenous monetary policy shocks. By imposing proper sign restrictions, several puzzling responses (e.g., liquidity and price puzzles) can be eliminated.[3] Given that puzzling responses are often regarded as failures in identifying proper monetary policy shocks, the identification strategy with sign restrictions is appealing. The methodology of the structural VAR model with sign restrictions is briefly described below.

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firms and raises bankruptcy rates, thus worsening the economic environment and bringing about further depreciation during a financial crisis.

[2] See Kim (2013b) for more general results on emerging countries.

[3] An exogenous monetary expansion (contraction) is supposed to increase (decrease) monetary aggregates and price levels and decrease (increase) interest rates. However, in a model that uses innovations in broad monetary aggregates as monetary policy shocks, both monetary aggregates and interest rates increase. This phenomenon is called the "liquidity puzzle." On the other hand, in a model that uses innovations in interest rates as monetary policy shocks, both interest rates and price levels increase. This phenomenon is called the "price puzzle." These puzzles are often regarded as indications that exogenous shocks to monetary policy are not properly identified in the model (Kim 2013a).

A reduced form of the VAR model is considered:

$$Y_t = B(L)Y_{t-1} + C(L)X_t + u_t \tag{1}$$

where Y_t is an $l \times 1$ vector of endogenous variables, X_t is an $m \times 1$ vector of exogenous variables, u_t is a $l \times 1$ residual vector, $E(u_t) = 0$, $E(u_t u_t') = \Sigma$, and $B(L)$ and $C(L)$ are $l \times l$ and $l \times m$ matrix polynomials in lag operator L.

In previous studies, reduced-form residuals and elements of u_t are the linear combinations of structural shocks:

$$u_t = Av_t \tag{2}$$

where A is an $l \times l$ matrix, v_t is an $l \times 1$ vector of structural shocks, $E(v_t) = 0$, and $E(v_t v_t') = 1$. Previous studies often recovered orthogonal structural shocks from reduced-form residuals by determining A. For example, the recursive identification strategy developed by Sims (1980) recovers A as a lower triangular matrix by applying Cholesky decomposition on Σ.

Uhlig (2005) has identified structural shocks by imposing sign restrictions on impulse responses. The study has identified only one structural shock in particular, that is, monetary policy shocks, which amounts to identifying a single column $a \in R^m$ of the matrix A. Uhlig (2005) defines the impulse vector as follows.

Definition 1. The vector $a \in R^m$ is called an impulse vector if matrix A exists; thus, $AA' = \Sigma$ and a is a column of A.

Uhlig (2005) shows that any impulse vector a can be characterized by $a = \tilde{A}\alpha$, where $\tilde{A}\tilde{A}' = \Sigma$ is a Cholesky decomposition of Σ, and α is an l-dimensional vector of unit length. Thereafter, the vector impulse response $r_a(k)$ for a is expressed by the following: $r_a(k) = \sum_{j=1}^{l} \alpha_j r_j(k)$, where $r_j(k) \in R^l$ is the vector response at horizon k to the jth shock in a Cholesky decomposition of Σ. A list of inequality restrictions on the entries of the vector impulse response $r_a(k)$ at various horizons k is then imposed.

Following the pure sign restriction approach by Uhlig (2005), a Bayesian prior for the VAR parameters (B, Σ) and an independent uniform prior for α are assumed. Only the draws that satisfy the inequality restrictions are retained in the simulation exercise. The probability bands are calculated based on 10,000 such draws.

B. Empirical Model and Data

The estimation period is relatively short. Hence, only five endogenous variables are included in the baseline VAR model: the call rate (CR), the monetary base (MB), the consumer price index (CPI), industrial production (IP), and the

won–dollar exchange rate (ERA). The first four variables are key macro/monetary variables. These are included to identify monetary policy shocks. The last variable is the focus of this study.

Two variables are also included as exogenous variables in the baseline model: the federal funds rate (FFR) and a variable representing worldwide risk conditions. The FFR is included to control for US monetary policy, which is likely to affect the exchange rate. A variable representing worldwide risk conditions is included because it is also likely to affect the won–dollar exchange rate and capital inflows into and outflows from the Republic of Korea. Given that assets in emerging countries like the Republic of Korea are riskier than assets in the US, changes in worldwide risks are likely to affect the relative price of assets in emerging versus advanced countries (or the risk premium of the won relative to the dollar) and consequently capital flows and exchange rates. For example, an increase in uncertainty or credit risk in the world economy may prompt international investors to purchase assets in advanced countries, which are relatively safer, by selling assets in emerging countries, which are relatively riskier.

The FFR and worldwide risk conditions are assumed to be exogenous to domestic variables because the variables of a small open economy such as the Republic of Korea are not likely to affect US or worldwide variables. These exogenous variables are also not restricted in terms of their contemporaneous effect on endogenous variables in the model (Equation (1)).

The following sign restrictions on impulse responses are imposed to identify contractionary monetary policy shocks: (1) increased call rate, (2) decreased monetary base, and (3) decreased CPI. By imposing these restrictions, liquidity and price puzzles are avoided. Thus, the impulse responses of these basic macro variables to monetary policy shocks are consistent with conventional wisdom on the effects of monetary policy. The sign restrictions are imposed on the impulse responses for the first 12 months after a shock.[4]

Thereafter, various components of financial accounts (capital flows) are added one by one to infer the transmission mechanism in the baseline model. Given that the sign restrictions imposed in the baseline model can identify monetary policy shocks, no additional restrictions are imposed in the extended models.

Monthly data are used. A constant term and six lags are assumed. The estimation period is from January 1999 to June 2012. The sample starts from 1999 because monetary policy operating procedures in the Republic of Korea have changed substantially, with the capital account almost fully liberalized after the Asian financial crisis (Kim and Park 2006, Kim and Yang 2012). The CBOE DJIA Volatility Index (VIX) is used as the variable to represent worldwide risk. The difference between

[4]Following Scholl and Uhlig (2009), the restrictions are imposed for 12 months. The main results are still qualitatively similar even when the restrictions are imposed under different durations. Several results are reported in Section IV.C.

Table 1. **Participation Rate of Foreign Investors in Bond and Stock Markets in the Republic of Korea**

	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011
Bonds	0.09	0.11	0.29	0.48	0.46	0.59	4.45	4.33	5.57	6.64	6.90
Stocks	32.80	36.30	41.20	40.50	37.20	35.10	32.70	27.20	30.50	31.20	30.60

Sources: Bank of Korea, Financial Supervisory Service, Korea Exchange.

the US Baa corporate bond yield (Moody's seasoned Baa corporate bond yield) and the 10-year US Treasury constant maturity rate, representing credit risks, is also used in the extended experiment. Korean data are obtained from the web page of the Bank of Korea. US data are obtained from the Macro Database of the Federal Reserve Bank of St. Louis.

Table 1 shows the participation rate of foreign investors in the stock and bond markets of the Republic of Korea in 2000. Foreign investors owned a substantial fraction of stocks (ranging from 27% to 42%) but only a small fraction of bonds (less than 10%). Before 2007, foreign investors owned less than 1% of bonds.

III. Empirical Results: Effects on Exchange Rate

A. Baseline Model

Figure 1 shows the impulse responses of the macro variables to monetary policy shocks in the baseline model with 68% probability bands for a 4-year horizon.[5] The name of each variable is denoted at the top of each graph. By imposing the sign restrictions, the price and liquidity puzzles are avoided. In response to monetary policy shocks, the interest rate increases and the monetary base and price level decrease. These are likely responses under exogenous monetary contraction. Industrial production declines over time; the peak response is found to occur approximately 15 months to 20 months after the shock.

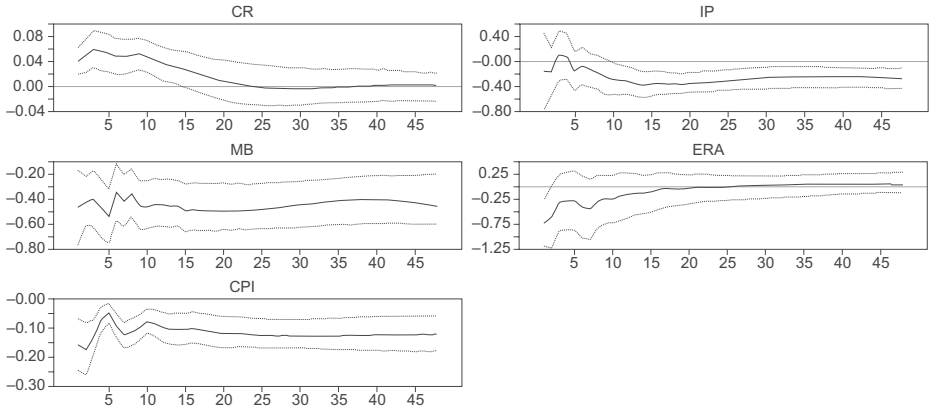
The domestic currency appreciates on impact and then depreciates back to the initial level approximately 20 months after the shock. The probability bands do not include the zero response in the short run. The standard theory suggests that, other things being equal, an exogenous monetary contraction, which increases the interest rate, will appreciate the domestic currency in the short-run. The empirical result matches the prediction.[6]

I then examine the nature and the size of monetary shocks and exchange rate responses more carefully. The interest rate increases by 0.04 percentage point on

[5]The number of estimated parameters is relatively large compared to the data point, hence a 68% probability band (or one-standard error band) is used for inference.

[6]The domestic currency depreciates back to the initial level in the long run. The long run effect is therefore close to zero. This finding is puzzling because the monetary base and price levels decline in the long run. A brief explanation is provided in Section IV.

Figure 1. **Impulse Responses to Monetary Policy Shocks—Baseline Model**



CPI = consumer price index, CR = call rate, ERA = won–dollar exchange rate, IP = industrial production, MB = monetary base.

Note: This figure shows the impulse responses to monetary policy shocks for a 4-year horizon in the baseline model. The solid line is the median response, while dotted lines are 68% probability bands. The name of each responding variable is denoted at the top of each graph.

Source: Author's computations.

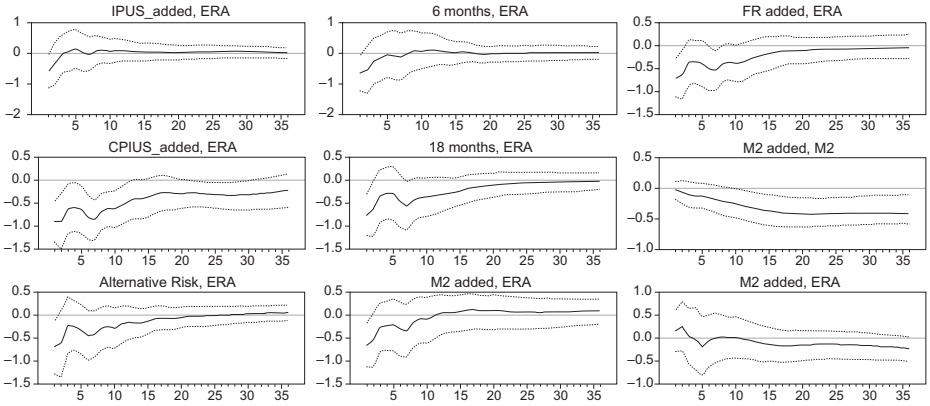
impact and stays 0.04–0.06 percentage point above the initial level for one year. The interest rate decreases back to the initial level in 20 months or so. In response to such monetary policy shocks, the exchange rate appreciates about 0.75% on impact, which is the maximum effect. The domestic currency depreciates back to the initial level in about 20 months or so.

B. Extended Experiments

To check the robustness of the results, several alternative specifications are investigated. First, industrial production and CPI of the US are added one by one as exogenous variables in the model ("CPI_US added" and "IP_US added" in Figure 2). Basic economic conditions in the US such as economic activity and the price level (represented by industrial production and CPI) may be important to explain exchange rate movements. Second, the difference between the US Baa corporate bond yield and the 10-year US Treasury constant maturity rate, which proxies for credit risk, is used instead of VIX ("VIX" in Figure 2).[7] Third, the duration of the sign restrictions is changed to 6 and 18 months instead of 12 months ("6 months" and "18 months" in Figure 2). Fourth, M2 is added as an additional endogenous variable to check whether a broad monetary aggregate decreases in response to identified monetary policy shocks as expected in an actual exogenous monetary contraction ("M2_added" in Figure 2). Finally, foreign exchange reserves

[7]The correlation between the two variables for the sample period is 0.8.

Figure 2. **Impulse Responses to Monetary Policy Shocks—Extended Models**



6 months = sign restrictions imposed for 6 months, 18 months = sign restrictions imposed for 18 months, Alternative Risk = the difference between the US Baa corporate bond yield and the 10-year US Treasury constant maturity rate used instead of VIX, CPIUS_added = US consumer price index added to model, ERA = won–dollar exchange rate, FR = foreign exchange reserves, FR_added = foreign exchange reserves added to model, IPUS_added = US industrial production added to model, M2 = broad money measure, M2_added = broad money measure added to model.

Note: This figure shows the impulse responses to monetary policy shocks for a 3-year horizon in the extended model. The solid line is the median response, while dotted lines are 68% probability bands. The name of each model and each responding variable is denoted at the top of each graph.

Source: Author's computations.

(FR) is added as an additional endogenous variable to infer the foreign exchange intervention ("FR_added" in Figure 2).

The results are reported in Figure 2. The results of all models are not too different from those of the baseline model. The domestic currency appreciation is found as predicted by the standard theory predicts. The domestic currency appreciates on impact and then depreciates to the initial level over time. The error bands do not include zero responses in the short run. In the model where M2 is added, M2 decreases over time in response to a contractionary monetary policy shock. Such responses of M2 are expected in exogenous monetary contraction, a result that further supports the validity of the empirical model. Finally, foreign exchange reserves do not change significantly, which may imply that foreign exchange intervention is not clearly found after monetary policy shocks. This suggests that we do not need to pay too much attention to foreign exchange intervention when we examine the effects of Korean monetary policy shocks on exchange rate.

IV. Detailed Transmission Mechanism: Effects on Components of Capital Flows

A. Theory

To explore further the detailed channels through which monetary policy shocks affect the exchange rate, I examine the effects of these shocks on the various

components of capital flows. Theoretically, an exogenous monetary contraction that increases domestic interest rate affects the portfolio choice of foreign and domestic investors in the following ways. First, domestic bonds become more attractive because they offer higher interest rates. Second, domestic stocks become less attractive because monetary contraction is likely to have an adverse effect on the domestic economy and a substitution from domestic stocks to domestic bonds may transpire considering that domestic bonds offer better returns. Third, given that foreign bonds offer relatively less returns than domestic bonds, the former becomes less attractive. Fourth, the sale of domestic stocks (with a negative expectation on the domestic economy after monetary contraction) may lead to the purchase of foreign bonds. Fifth, foreign stocks become less attractive because a substitution from foreign stocks to domestic bonds may occur. Sixth, the sale of domestic stocks may lead to the purchase of foreign stocks.

These changes in portfolio choices are likely to affect various components of capital flows and exchange rates. First, foreign investors are likely to purchase domestic bonds because they offer higher returns, thus resulting in an increase in capital inflows for the bond market and appreciation of the domestic currency. Second, foreign investors are likely to sell domestic stocks because they become less attractive under a negative perspective on the domestic economy after monetary contraction. This condition is likely to result in a decrease in net capital inflow in the stock market and the depreciation of the domestic currency. Third, domestic investors are likely to sell foreign bonds to purchase domestic bonds, thus leading to a decrease in net capital outflow for the bond market and domestic currency appreciation. Fourth, domestic investors may purchase foreign bonds if they wish to substitute domestic stocks with foreign bonds, thus leading to an increase in net capital outflow for the bond market and depreciation of the domestic currency. Fifth, domestic investors may sell foreign stocks to purchase domestic bonds that offer higher returns, thus resulting in a decrease in net capital outflow for the stock market and appreciation of the domestic currency. Sixth, domestic investors may purchase foreign stocks if they would like to substitute domestic stocks with foreign stocks, thus leading to an increase in net capital outflow for stock market and depreciation of the domestic currency.[8]

The standard theory, which is based on a relation like the interest parity condition, suggests that the first and the third channels should work for domestic and foreign bond markets. Moreover, the domestic currency appreciates through the first, third, and fifth channels but depreciates through the second, fourth, and sixth channels. If foreign investors do not actively participate in the domestic bond market, the first channel may not work. If domestic investors are restricted from

[8]Foreign investors' buying and selling strategy may have an impact on domestic investor sentiment, which may amplify the effects of foreign investors' actions. On the other hand, domestic currency appreciation following monetary contraction worsens the trade balance and decreases output, which may make the negative effect on stock price stronger.

Table 2. **Basic Properties of the Components of Capital Flows**

	Mean	Standard Deviation
Bond, Asset (Net Outflows)	0.35	1.57
Bond, Liability (Net Inflows)	1.43	3.16
Stock, Asset (Net Outflows)	0.62	1.80
Stock, Liability (Net Inflows)	0.68	3.37
Loan, Asset (Net Outflows)	0.23	2.80
Loan, Liability (Net Inflows)	−0.06	5.50

Note: This table shows the mean and standard deviation of various types of capital flows (monthly frequency). Each variable is expressed as a percent of trend gross domestic product.
Sources: Bank of Korea

investing in foreign bond and stock markets, the last four channels may not work. If only the second channel works, then the domestic currency depreciates.

B. Empirical Results: Baseline Model

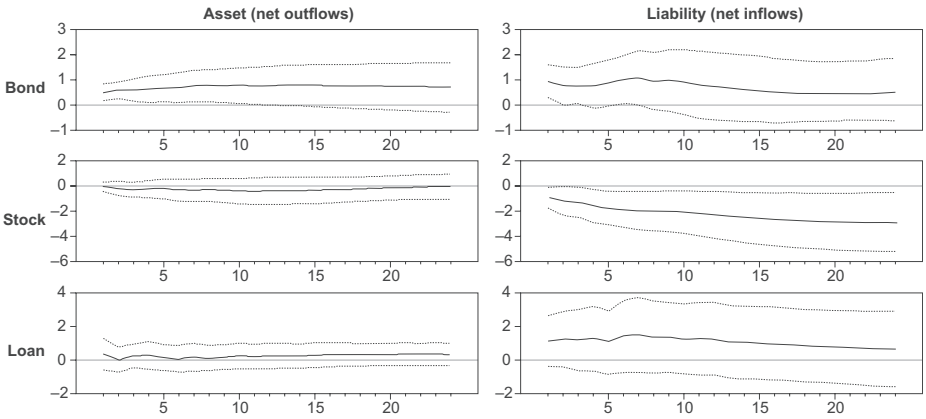
To explore whether each channel works, the effects of monetary policy shocks on various components of capital flows are examined by extending the baseline model (Section II.B). Capital flows are divided into two large categories: net capital inflows (net changes in liability flows) and net capital outflows (net changes in asset flows). Three types of net capital inflows and outflows are considered, namely, stocks, bonds, and loans. Although theoretical predictions on capital flows in loan markets are not discussed in Section IV.A, capital flows in loan markets are examined empirically because capital flows in loan and bond markets are likely to behave similarly in theory.

Each variable is stated in US dollar terms, divided by trend US dollar GDP for normalization, and then multiplied by 100. Therefore, each variable is expressed as a percentage of trend GDP.[9] Table 2 shows the mean and standard deviation of each component. For each component, net inflows (liability) are more volatile than net outflows. The volatilities of stock and bond flows do not differ much, but the volatility of loan flows exceed the volatility of stock and bond flows.

Figure 3 shows the (cumulative) impulse responses of various components of capital flows for a 2-year horizon. Cumulative responses are reported to infer whether capital flows increase up to a certain time horizon after the shock. The graphs in the first and the second columns show the components of net capital outflows and inflows, respectively. The graphs in the first, second, and third rows show the capital flows in bonds, stocks, and loan markets, respectively. To aid comparison of the sizes of inflows and outflows for each market, scales of the graphs in the same row are identical by construction.

[9] A linear trend is used, but the results are similar when other trend types (e.g., a quadratic trend, the trend obtained using the HP filter) are used. GDP is calculated in monthly terms to be consistent with monthly capital flows.

Figure 3. **Impulse Responses of Components of Capital Flows—Baseline Model**



Note: This figure shows the (cumulative) impulse responses of various components of capital flows to monetary policy shocks for a 2-year horizon. The solid line is the median response while dotted lines are 68% probability bands. The graphs in the first and the second columns show the components of net capital outflows and inflows, respectively. The graphs in the first, second, and third rows show the capital flows in bonds, stocks, and loan markets, respectively.
Source: Author's computations.

First, capital inflows decrease sharply for the stock market in response to a contractionary monetary policy shock. The probability bands do not include zero responses. This finding implies that the second channel works strongly; that is, foreign investors withdraw money from the domestic stock market after domestic monetary tightening. However, capital outflows for the stock market do not change significantly.

Second, capital inflows for the bond market increase, a result consistent with the first channel. The short-run response is also significant. Coinciding with the prediction of standard theory, foreign investors purchase domestic bonds because of better returns. Capital inflows for the loan market also increase, although probability bands include the zero response.

Third, capital outflows for the bond market also increase significantly consistent with the fourth channel. This result suggests that domestic investors purchase foreign bonds after selling domestic stocks. In contrast, capital outflows for the loan market do not change significantly. Also note that the increase in capital inflows for bond and loan markets exceed that of capital outflows, indicating net capital inflows.

The standard bond market channel is therefore functional. Some of the past studies have questioned this channel because of the relatively weak participation of foreign investors in the Korean bond market. However, the empirical results show that foreigners invest more in domestic bonds after domestic monetary contraction.[10]

[10]The significant effect through the bond market may be related to the rising trend of foreign participation in the bond market from 0.48% in 2004 to 6.9% in 2011, as shown in Table 1. The increase in foreign participation may

On the contrary, the other side of the standard bond market channel does not seem to work well. Empirical results show that domestic investors purchase foreign bonds after a domestic monetary contraction. Domestic investors may end up purchasing foreign bonds after selling domestic stocks with negative prospects after monetary contraction. However, the increase in capital inflows for the bond market seems to be larger than that of capital outflows in the short run. In addition, capital inflows for the loan market increase (although not significantly), whereas capital outflows do not change substantially. These results suggest that capital flows in bond and loan markets lead to domestic currency appreciation in the short run.

Finally, similar to the claim of some past studies, foreign investors sell domestic stocks after monetary contraction. This finding is probably due to the negative perspective on the domestic economy after monetary contraction. This condition may lead to domestic currency depreciation, as suggested by previous studies. However, the increase in capital inflows for bond and loan markets is relatively stronger than the decrease in capital inflows for the stock market. Overall, the empirical result shows that the domestic currency appreciates.[11]

C. Extended Experiments

To check the robustness of the results even further, effects of monetary policy shocks on the components of capital flows are examined based on the alternative models discussed in Section III.C. Similar to the baseline model, each component of capital flows is added to each alternative model without imposing any further restrictions. The results are reported in Figure 4. Each column of figures shows the results from each model, and each row of figures shows the results for each component of capital flows. To facilitate the comparison, the graphs in the first two, next two, and last two rows have the same scale.

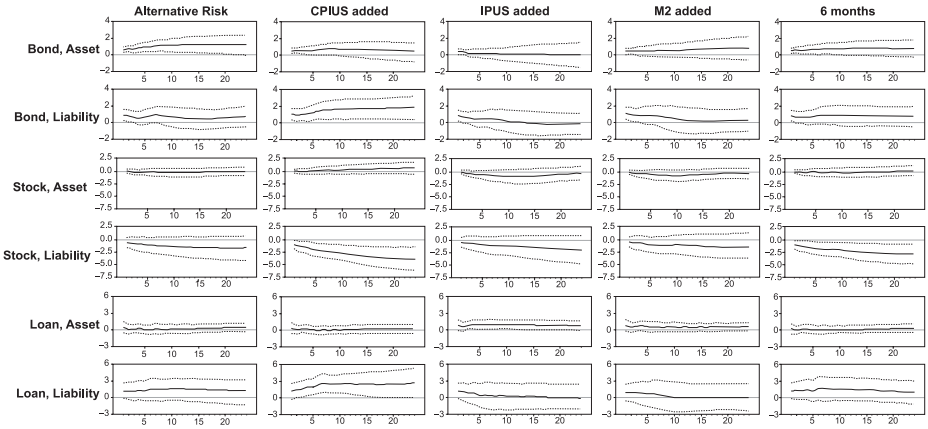
First, the short-term increase in capital inflows for the bond market is significant in all models. Capital outflows for the bond market also increase significantly; however, the increase in outflows tends to be smaller than the rise in inflows, particularly in the short run. These empirical findings on capital flows in the bond market are robust across different models.

Capital outflows for the stock market tend to decrease in all models. However, the decrease is not significant in some models. Changes in capital inflows for the stock market are relatively small compared with the changes in capital outflows. Capital inflows for the loan market increase in most models, significantly so in some

be related to various regional efforts on developing bond markets (e.g., the Asian Bond Market Initiative and Asian Bond Funds). Refer to Kim and Yang (2011) for Asian financial cooperation.

[11] Note that the decrease in capital inflows to the stock market and the increase in capital outflows to the bond market are more persistent than the increase in capital inflows to bond and loan markets. This may explain why the domestic currency does not appreciate in the long term despite the long-term decrease in price levels and monetary base.

Figure 4. **Impulse Responses of the Components of Capital Flows—Extended Experiments**



6 months = sign restrictions imposed for 6 months, Alternative Risk = the difference between the US Baa corporate bond yield and the 10-year US Treasury constant maturity rate used instead of VIX, CPIUS_added = US consumer price index added to model, IPUS_added = US industrial production added to model, M2_added = broad money measure added to model.

Note: This figure shows the (cumulative) impulse responses of various components of capital flows to monetary policy shocks for a 2-year horizon in the extended models. The solid line is the median response while dotted lines are 68% probability bands. Each column of figures shows the results from each model, and each row of figures shows the results for each component of capital flows.

Source: Author's computations.

models. Capital outflows for the loan market also increase in some cases. However, the rise is insignificant and relatively small in most cases. To summarize, similar to the baseline model, capital inflows to the loan market tend to increase, whereas capital inflows for the stock market tend to decrease. Nevertheless, these results are not significant in some cases.

V. Conclusion

In contrast to the condition in advanced countries, foreign investors do not actively participate in the domestic bond market in the Republic of Korea. However, foreign investors actively participate in the domestic stock market. Based on this phenomenon, some past studies argue that a monetary contraction may depreciate the domestic currency when the standard bond market channel does not work. In addition, foreign investors may withdraw money from the domestic stock market after a monetary contraction. Against this backdrop, this study examined the effects of monetary policy shocks on the exchange rate in the Republic of Korea using structural VAR models with sign restrictions.

Empirical results show that a monetary contraction appreciates the exchange rate in the short run as predicted by standard theory. To explore the channels by

which monetary policy shocks affect exchange rates, the effects of these shocks on various components of capital flows were examined. Consistent with standard theory such as the uncovered interest parity condition, the study found a significant increase in capital inflows to the bond market. This seems to be the main channel by which monetary contraction appreciates the domestic currency. However, this result contradicts the argument that the bond market channel may not work because of weak participation of foreign investors in the domestic bond market. Empirical results also show that capital inflows tend to decrease in the stock market, which is similar to the claim of some past studies. However, the bond market channel seems to dominate the stock market channel because domestic currency appreciates under monetary contraction.

Since the Asian financial crisis in 1997, several policy makers and researchers have argued that emerging Asian economies suffer from the lack of strong monetary transmission mechanisms. They suggest that investors do not respond strongly to monetary expansion and the resulting decline in interest rates because firms have become conservative after experiencing the devastating financial crisis.

Even if the interest rate or investment channel does not work, the empirical results suggest that an alternative channel for monetary policy, that is, the exchange rate channel, is likely to work. Many Asian countries, including the Republic of Korea, adopted a regime with a more flexible exchange rate and a more open capital account. Under such an environment, the interest rate channel becomes weaker but the exchange rate channel becomes stronger.[12]

The policy environment of emerging economies will undergo rapid changes in the future. Emerging economies are likely to be more financially interconnected with the rest of the world as foreign investors participate more actively in the bond market and as domestic investors vigorously pursue outside opportunities. This environment will lead to another challenge for policy makers of emerging economies. Policy makers should be aware of the changes in policy environments and should have a clear understanding of the effects of changes in policies.

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[12] However, the exchange rate channel may not work for countries with a rigid exchange rate regime, while changes in credit availability following monetary policy may be important for countries where short-term and long-term interest rates are not closely tied together.

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Inclusive Growth: When May We Expect It? When May We Not?

Kunal Sen*

Episodes of economic growth that lead to reductions in poverty and inequality are relatively rare in developing countries. In this paper, we examine the institutional foundations of such growth episodes. We argue that the institutional factors that lead to accelerations in economic growth will be different from those that lead to growth maintenance and avoidance of growth decline, and that the institutional preconditions for growth accelerations suggest that these growth episodes may not be inclusive. We present empirical evidence drawn from descriptive and cross-country econometric analyses that support these theoretical propositions.

Keywords: inclusive growth, institutions, growth accelerations, growth maintenance, poverty, inequality
JEL codes: E02, O40, P48

I. Introduction

It is now well recognized that sustained economic growth that is inclusive will ensure poverty reduction as well as a reduction in inequality. While many developing countries have witnessed rapid economic growth in the recent decades, relatively few of these countries have been able to ensure that the economic growth process has been inclusive of the poor (Ali and Zhuang 2007; Klasen 2004, 2010). In contrast to the large literature on the determinants of economic growth, there is relatively little understanding on the preconditions for inclusive growth, by which we mean economic growth that leads to reductions in poverty and/or inequality.

Much of the literature on inclusive growth has tended to focus on the factors such as lower inflation, greater gender equality, and creation of productive employment for the poor as determinants of inclusive growth (IPC 2007). However, such factors are merely the proximate determinants of inclusive growth and cannot be regarded as the deep determinants of inclusive growth. In the theoretical literature on the determinants of economic growth, institutions are defined as "the rules of the game or more formally, the humanly devised constraints that shape human interaction" (North 1990, p. 3). They are now widely regarded as the fundamental cause

*Professor of Development Economics at IDPM, and Associate Director at the Brooks World Poverty Institute (BWPI), University of Manchester, United Kingdom. I thank the co-managing editor, Maria Socorro Bautista, and the two referees for their helpful comments, as well as the discussants in the *Asian Development Review* workshop held in Manila in March 2013.

of economic growth—the fundamental cause being "the factors potentially affecting why societies make different technology and accumulation choices" (Acemoglu 2009, p. 20).

As the recent literature on the causes of growth makes clear, better regulations and laws provide firms with incentives to invest in productive activities and to develop new goods and production technologies. This leads to greater factor accumulation and technological change (Hall and Jones 1999; Acemoglu, Johnson, and Robinson 2005). However, whether well-functioning institutions can also be regarded as key determinants of inclusive growth is an issue that has received little attention in the academic and policy literature. It is not obvious that the growth process engendered by better institutions will be inclusive of the poor, and whether institutional improvements such as better protection of the rights of private investors will be poverty and inequality reducing. For example, Kraay (2006) finds that improvements in the quality of institutions can lead to greater pro-poor growth, while Amendola, Easaw, and Savoia (2013) show that stronger property rights can exacerbate inequality. Therefore, the inclusiveness of growth processes that are caused by better quality institutions are a matter of conceptual and empirical debate.

Parallel to the developments in our theoretical understanding of the causes of growth, there has been a realization in recent years that the emphasis in the previous growth empirics literature on long-run growth or levels of income (such as in the report of the Commission for Growth and Development 2008) is not compatible with the "stylized facts" of economic growth (Pritchett 2000). As Jones and Olken (2008, p. 582) point out, "almost all countries in the world have experienced rapid growth lasting a decade or longer, during which they converge towards income levels in the United States." Conversely, nearly all countries have experienced periods of abysmal growth. Circumstances or policies that produce 10 years of rapid economic growth appear easily reversed, often leaving countries no better off than they were prior to the expansion.

Long-run growth averages within countries mask distinct periods of success and failure, and while the growth process of all "developed" economies is well characterized by a single growth rate and a "business cycle" around that trend (at least until the recent crises), this is not true of most countries in the world (Kar et al. 2013). Massive and discrete changes in growth are common in developing countries, and most developing countries experience distinct growth episodes: growth accelerations and decelerations or collapses (Jerzmanowski 2006). The recent empirical literature has highlighted the need to differentiate between different phases of growth in a particular country—that is, our understanding of the causes of growth needs to take into account the fact that the causes of growth accelerations may well be different from the factors that maintain growth, once it has ignited in the country (Rodrik 2005).

In this paper, we provide a conceptual framework for understanding the causes of inclusive growth, drawing from both the theoretical literature on institutions and

the empirical literature on growth phases. We argue that the institutional preconditions of early-stage growth accelerations (that is, when a country has begun to witness positive growth after a prolonged period of stagnation or collapse) may be such that inclusivity of growth may not be particularly evident. During a growth acceleration, formal institutions are either non-existent or function ineffectively. Informal institutions, repeated bilateral relations between politicians and bureaucrats on one hand and investors on the other, solve the credible commitment problem that can lead to investment and growth. However, these informal institutions by their nature are not inclusive and favor certain firms and households over others. As a consequence, growth accelerations are unlikely to be inclusive.

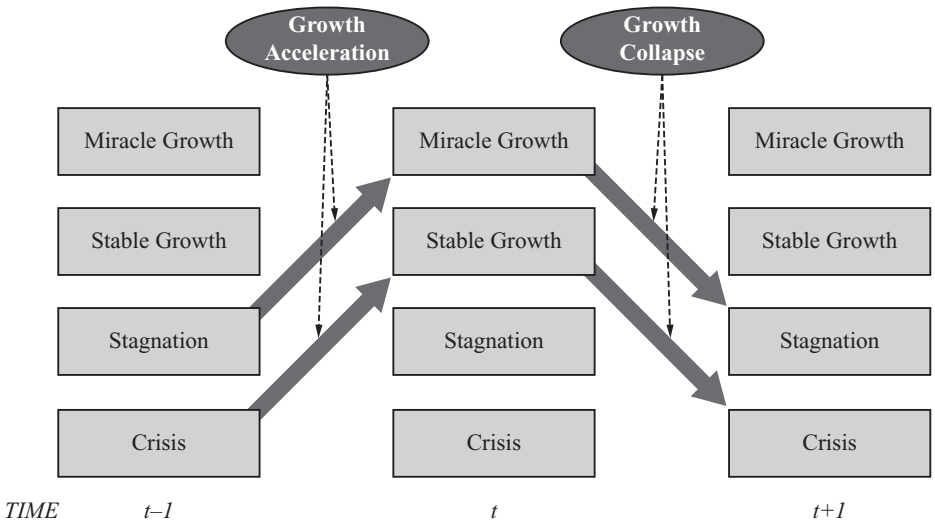
On the other hand, inclusive growth is more likely to result when a country is in growth maintenance phase—that is, when growth rates are positive for some time—especially if inclusive formal economic and political institutions are to emerge in the country. The emergence of inclusive formal institutions not only leads to a greater likelihood that growth will be maintained, but will also ensure that economic growth is broad-based (Acemoglu and Robinson 2012). We then present empirical evidence that supports our main theoretical propositions using a panel of 42 countries for which we have data on poverty, inequality, and institutional quality for the period 1984–2010.

The rest of the paper is as follows. Section II conceptualizes economic growth as transitions between growth phases, differentiating between the determinants of growth accelerations and of growth maintenance. Section III draws from the recent theoretical literature on institutions and economic growth to obtain testable hypotheses on the institutional preconditions of inclusive growth across growth phases. Section IV examines the behavior of poverty and inequality across different growth phases to see whether the outcomes of inclusive growth differ across these phases. Section V provides a descriptive analysis of the relationship between institutions and the inclusiveness of growth across growth phases. Section VI tests our main hypotheses using econometric analysis. Section VII concludes.

II. Understanding Economic Growth as Transitions in Growth Phases

As the recent empirical literature on economic growth shows, economic growth in many developing countries involves discrete and quantitatively massive transitions between periods of high growth, periods of negative growth, and periods of stagnation. To fix our ideas on transition paths around growth regimes, we provide a simple sketch of these transition paths in Figure 1. Using a rough and ready way to demarcate growth regimes, we classify growth regimes into four categories: (i) a growth regime which we call "miracle growth," where the average increase in per capita income is 5% per annum or more; (ii) a growth regime which we call "stable growth," where the average increase in per capita income is between 0% and 5% per

Figure 1. **Transition Paths between Growth Phases**



Source: Author's representation.

annum; (iii) a growth regime which we call "stagnant growth," where the average increase in per capita income is around 0% per annum; and (iv) a growth regime which we call "growth crisis," where the average change in per capita income is negative.

Figure 1 makes clear that a complete characterization of the growth process in any particular country needs an understanding of the factors that lead to growth acceleration (that is, the transition from stagnation or crisis to stable growth or miracle growth) as well as the factors that lead to the avoidance of growth collapses and the maintenance of positive growth (that is, the ability of the country to stay in stable growth or miracle growth in consecutive periods). It is not obvious that the factors leading to growth acceleration will lead to growth maintenance as well. As Rodrik (2005, p. 3) argues:

> Igniting economic growth and sustaining it are somewhat different enterprises. The former generally requires a limited range of (often unconventional) reforms that need not overly tax the institutional capacity of the economy. The latter challenge is in many ways harder, as it requires constructing a sound institutional underpinning to maintain productive dynamism and endow the economy with resilience to shocks over the longer term.

Once we view economic growth as transitions between the above growth phases and, in particular, the transitions from crisis/stagnant growth to stable/miracle

growth, the key questions that need to be asked relate to: (i) the institutional determinants of growth acceleration and how they differ from the institutional determinants of growth maintenance; and (ii) how poverty and inequality behave during growth accelerations and growth maintenance. We turn to these two issues in the next two sections.

III. The Institutional Determinants of Inclusive Growth

A recent set of papers has tried to go beyond the proximate determinants of economic growth such as macroeconomic stability and trade openness to study the fundamental causes of economic growth across countries, and in particular, the importance of economic and political institutions (most notably, Acemoglu, Johnson, and Robinson 2001). These papers have mostly focused on the institutional determinants of economic growth and not on whether these institutional factors may affect the inclusiveness of growth. Nor have they distinguished between different phases of economic growth—in particular, between growth acceleration and growth maintenance. In this section, we review the literature on institutions and growth and explore the possible implications of this literature for our understanding of the institutional determinants of inclusive growth. In particular, we ask whether the nature of growth with respect to its inclusiveness will be different during early stage growth accelerations compared to phases when growth has already ignited and is being maintained.

Acemoglu and Robinson (2008, 2012) have provided an influential theoretical argument on why institutions can be seen as fundamental causes of economic growth. In their theory, economic growth may accelerate initially under extractive economic institutions such as insecure property rights and regulations that limit entry to markets and extractive political institutions that concentrate power in the hands of a few with limited checks and balances. However, it is unlikely for economic growth to be maintained and be broad-based without the emergence of inclusive economic and political institutions. Inclusive economic institutions feature secure property rights for the majority of the population (such as smallholder farmers and small firms), law and order, markets that are open to relative free entry of new businesses, state support for markets (in the form of public goods provision, regulation, and enforcement of contracts), and access to education and opportunity for the great majority of citizens. Inclusive political institutions allow broad participation of the citizens of the country, uphold the rule of law, and place constraints and checks on politicians along with the rule of law.

Once inclusive economic and political institutions emerge, economic growth may be maintained for a long time. However, political and economic elites may not have a strong incentive not to change extractive institutions if they personally benefit from the presence of these institutions. In contrast, inclusive and political

institutions will be more likely to prevail once they emerge. This is true because with the emergence of such institutions (e.g., democratization and secure property rights for the majority of the population), strong economic performance will be the likely result, reinforcing the welfare-enhancing effects of these institutions. The persistence of extractive institutions may explain why developing countries see boom–bust growth, as these institutions are not likely to lead to long-run sustained growth. Moreover, extractive institutions by their very nature are not conducive to inclusive growth processes.

Sen (2013) argues that the institutional determinants of early-stage growth accelerations may be different from the institutional determinants of growth maintenance. When a country has witnessed a prolonged period of stagnation or crisis previous to an acceleration in economic growth, formal institutions are either not present or, when present, are weakly enforced. Informal institutions—bilateral repeated relationships between politicians and investors where the politician protects the investor from expropriation of his profits in return for rents—may help solve the credible commitment problem in the absence of strong formal institutions and may get economic growth started (Grief 1993). A similar point is made by Pritchett and Werker (2013), who argue that a move from "disordered deals" to "ordered deals" that are available to investors—where deals are personalized relationships between economic agents, and the move from disordered to ordered deals is a shift from unpredictable to more predictable relationships—are both necessary and sufficient for economic growth to accelerate, and by De Dios and Ducanes (2013) who argue that different types of institutions may be necessary at different levels of economic development for a particular country.[1]

However, for economic growth to be maintained, formal institutions need to emerge and/or be properly enforced. Also, personalized relationships between politicians and investors should not be confined to a few investors, but be made available to a wider set of investors. Pritchett and Werker (2013) call this a move from "closed" to "open" deals.

An important implication of the earlier discussion is that the growth processes associated with informal institutions or "ordered deals" are unlikely to be inclusive, as the very nature of these institutions and deals implies that they are exclusionary, possibly leaving out large parts of the productive poor. Thus, it can be hypothesized that the process of growth acceleration will not be necessarily inclusive, and that more inclusive processes of growth may have to wait until the economy enters a growth maintenance regime. Whether economic growth is inclusive in the growth maintenance phase will depend on the emergence of inclusive economic and political institutions—the greater the degree of inclusivity of these institutions, the more inclusive economic growth will be.

[1] For a similar argument in the context of economic development, see North, Wallis, and Weingast (2009).

The rather sparse literature on the stylized facts of pro-poor growth also helps us to understand the nature of poverty dynamics around growth traverses. While the positive relationship between economic growth and poverty reduction (if measured using the headcount ratio) is clearly seen in the cross-country data (Ravallion 2012), poverty reduction is more likely to occur when economic growth occurs in long spells rather than in short spells. This suggests that a move from a stagnant/crisis growth regime to a stable/miracle growth regime is not enough for poverty reduction unless the country stays in the stable/miracle growth regime for some time—that is, if the country avoids a growth decline in the initial period following a growth acceleration.

There is now an emerging quantitative literature on the determinants of growth accelerations and why some countries maintain high growth while other countries witness growth collapses. With respect to growth accelerations, Hausmann, Pritchett, and Rodrik (2005, p. 328) find that standard growth determinants such as major changes in economic policies, institutional arrangements, political circumstances, or external conditions "do a very poor job of predicting the turning points." They argue that growth accelerations are caused predominantly by idiosyncratic and often small-scale changes.

Pritchett (2000) points out that slow-moving determinants of growth, such as improvements in the quality of institutions, or time-constant factors, such as geography, are less likely to explain the frequent shifts from one growth phase to another that we observe in many developing countries. Jones and Olken (2008) show that changes in institutional quality are not associated with either growth accelerations or declines, where institutional quality is measured by a lower level of corruption and better rule of law.

On the other hand, Berg, Ostry, and Zettelmeyer (2012) find that growth duration (that is, the avoidance of growth collapses) is positively related to the presence of democratic political institutions in the country, along with the degree of equality of income distribution. Jerzmanowski (2006) finds that better institutional quality improves the possibility that a country will remain in a stable or miracle growth phase and will be less likely to suffer a growth collapse. Finally, using cross-country panel data, Sen (2013) shows that the institutional determinants of growth accelerations are different from those of growth maintenance and that improvements in formal institutions are unable to explain growth accelerations. These findings suggest that growth accelerations may occur in countries that have weak institutions, but at the same time, weak institutions may limit the sustainability or maintenance of economic growth.

However, these studies do not look at the relationship between institutional quality and the inclusivity of growth accelerations and growth maintenance. We examine this relationship later in the paper. But first, we examine whether poverty and inequality—the two critical dimensions of inclusive growth—differ across the growth acceleration and growth maintenance phase.

IV. Patterns in Poverty and Inequality in Growth Phase Transitions

In this section, we examine the behavior of inclusive growth during growth acceleration and during growth maintenance phases. We take inclusive growth to be a process of growth that leads to lower poverty and/or less inequality.[2] As Klasen (2010) notes, there are conflicting definitions of inclusive growth, with some definitions using a process-oriented approach and others using an outcome-oriented approach (e.g., Ali and Son 2007, McKinley 2010).[3] Our definition is outcome oriented and mostly guided by the availability of comparable data. The aim is to examine how inclusive growth has been for as many countries as possible for the longest sample period. Since we are interested in understanding the relationship between institutions and inclusive growth across different growth phase transitions within countries, we are similarly constrained by the availability of reliable time-series data on institutions.

For the poverty and inequality data, we use the *World Development Indicators* from the World Bank. We draw data on purchasing power parity GDP from the Penn World Tables. These are available for a subset of the countries from 1982 in most cases. Time-series data on institutions are obtained from the International Country Risk Guide (ICRG) database widely used in the institutions and growth literature.[4] The first year of the data is 1984 and the last is 2010.

We first need to identify years comprising growth breaks for our sample of countries for which we have comparable poverty and inequality data. We use the years of growth breaks that are provided in Kar et al. (2013), who use a combination of a statistical procedure and a filter-based approach to identify the breaks. We provide details of this methodology in Appendix 1. The years of growth breaks for the countries for which we have poverty and inequality data before and after the break are presented in Appendix 2.

For the poverty measure, we use the income share of the bottom 20% of the income distribution and the headcount ratio (in percent of the population) at $1.25 a day. For the inequality measure, we use the ratio of income share of the bottom 10% to the income share of the top 10% and the Gini coefficient. For the

[2] In our econometric analysis, we will use a measure of inclusive growth, which is the average of poverty and inequality. This would imply that an episode of economic growth that reduces poverty but increases inequality will not be considered as an episode of inclusive growth.

[3] Some definitions of inclusive growth take it to be an increase in the equality of opportunity (e.g., Ali and Zhuang 2007, Sugden 2012). However, in our view, greater equality of opportunity is a determinant of inclusive growth and a consequence of greater inclusiveness of institutions but is not inclusive growth per se. It should also be noted that we do not include nonmaterial dimensions of inclusive growth such as expanded education and health in our measure (e.g., Sugden 2012, Rauniyar and Kanbur 2010), as the institutional determinants of inclusive growth mostly relate to the material dimensions of inclusive development.

[4] A vast number of studies have used these measures in testing for the effects of institutions on economic growth. Prominent among them are Knack and Keefer (1995); Hall and Jones (1999); Acemoglu, Johnson, and Robinson (2001); Glaeser et al. (2004); and Rodrik, Subramanian, and Trebbi (2004).

institutional quality measures, we use the degree of corruption, rule of law,[5] and democratic accountability. The rule of law captures to some degree the inclusiveness of economic institutions—a higher prevalence of law and order would imply that more households and firms would be able to engage in economic transactions (Dixit 2009), while democratic accountability captures the inclusivity of political institutions. The degree of corruption captures the overall institutional environment in the country in question.

We now examine the trends in the poverty and inequality measures for a sample of countries for which we have identified the year of growth acceleration and the subsequent period of growth maintenance. To study the behavior of poverty and inequality before and after growth acceleration and during growth maintenance, we can only use the countries listed in Appendix 2 for which we have at least one set of observations on poverty and inequality before the year of growth acceleration and another set of observations during or after the growth acceleration.[6] We have 24 countries that match this criterion.[7] A positive feature of the countries in our sample is that they are drawn from all regions of the developing world and can be said to provide a representative picture of inclusive growth patterns during growth accelerations.

Table 1 presents the annual percentage change in the income share of the bottom 20% of the income distribution (column 1), the headcount ratio at $1.25 a day (column 2), the ratio of top to bottom 10% of the income distribution (column 3), and the Gini coefficient (column 4) for the 24 sample countries during their growth acceleration phase. We also compute three summary statistics that capture the responsiveness of poverty and inequality to the change in the growth rate during the growth acceleration phase.[8] The first statistic—the percentage change in the income share of the bottom 20% (as in column 1) to the change in the growth rate—measures the response of income shares of the bottom 20% of the income distribution to a growth acceleration (column 5). The second statistic—the percentage change in the headcount ratio (as in column 2) to the change in the growth rate—measures the response of poverty to a growth acceleration (column 6). The third statistic—the percentage change in the Gini coefficient (as in column 4) to the change in the growth rate—measures the response of inequality to a growth acceleration (column 7).

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[5] The rule of law measure in ICRG comprises two subcomponents: the law subcomponent, which is a measure of the strength and impartiality of the legal system, and the order subcomponent, which is an assessment of the popular observance of the law.

[6] For poverty and inequality data before the growth acceleration year, we choose the year for the data which is at least 3 years before the year of acceleration, and for the poverty and inequality data during and after the growth acceleration, we choose the year which is not more than 8 years after the growth acceleration year. It should be noted that our choice of years for the data is constrained by the lack of available data for the countries in our sample.

[7] The years for the pre-acceleration poverty and inequality data are given in Appendix 3a.

[8] We obtain the pre-break and post-break growth rates from Kar et al. (2013). These are provided in column 8 of Table 1. The growth rates are ordinary least squares growth rates, where GDP per capita is regressed on a linear time trend and a constant.

Table 1. **Behavior of Poverty and Inequality during Growth Accelerations (GA)**

	Annual Change							
Countries	**Income Share, Bottom 20% (1)**	**HCR, $1.25 a day (– means decrease) (2)**	**Ratio of Income Share, Top 10% to Bottom 10% (3)**	**Gini (4)**	**Income Share of Bottom 20% Response to GA (5)**	**HCR Response to GA (6)**	**Gini Response to GA (7)**	**Change in Growth Rate during GA (8)**
Bangladesh	–2.6	–3.6	6.2	4.8	–1.14	–1.55	2.68	2.3
Brazil	2.2	–3.1	–3.3	–0.1	1.48	–2.06	–2.21	1.5
PRC	–1.4	–0.1	3.7	2.9	–0.70	–0.05	1.83	2.0
Colombia	0.2	3.2	0.1	0.5	0.08	0.08	0.04	3.2
Costa Rica	0.2	3.2	0.1	0.5	–2.12	1.00	0.04	3.8
Dominican Republic	–8.1	–2.8	18.5	6.1	0.26	–0.75	4.86	3.6
Ecuador	0.9	–32.0	2.8	0.6	–4.55	–8.88	0.77	2.7
Guatemala	–12.5	–14.5	19.3	1.1	–3.20	–3.72	4.96	3.9
India	0.5	–1.4	–0.8	–0.6	0.31	–0.90	–0.52	1.5
Iran	3.2	–2.1	–5.6	–2.1	0.32	–0.21	–0.55	10.2
Jordan	–3.9	n/a	8.6	3.7	–0.53	n/a	1.18	7.3
Madagascar	0.8	–3.2	3.0	2.0	0.31	–1.20	1.12	2.7
Malaysia	2.2	–9.9	–4.1	–1.1	0.37	–1.68	–0.70	5.9
Mexico	6.2	–23.3	–11.6	2.0	1.94	–7.29	–3.62	3.2
Morocco	–0.1	12.7	0.5	0.1	–0.02	3.84	0.14	3.3
Nicaragua	5.0	–7.6	–8.4	–2.2	0.68	–1.03	–1.14	7.4
Nigeria	–6.8	2.3	11.1	2.5	–0.82	0.28	1.33	8.3
Panama	10.6	–7.6	–31.7	–0.4	2.16	–1.56	–6.47	4.9
Paraguay	3.8	3.5	–3.5	–0.2	0.96	0.87	–0.88	4.0
Peru	–0.1	1.6	–0.2	–0.2	–0.01	0.24	–0.03	6.9
Poland	–1.0	–24.1	0.7	0.9	–0.17	–3.95	0.11	6.1
Romania	–2.3	60.6	5.5	3.8	–0.28	7.48	0.68	8.1
Tanzania	–0.2	1.9	0.2	0.3	–0.04	0.48	0.04	4.0
Zambia	10.8	–1.7	–14.1	–1.8	1.52	–0.24	–1.98	7.1
Average	**–0.2**	**–1.3**	**0.6**	**1.1**	**–0.13**	**–0.56**	**0.34**	**4.7**

GDP = gross domestic product, HCR = headcount ratio, n/a = not available, PRC = People's Republic of China.
Notes:
(i) Annual change is equal to log change/T where T is the number of years between the 2 years before and after the acceleration.
(ii) Income share of bottom 20% response to GA is column (1) as a ratio of column (8).
(iii) HCR response to GA is column (2) as a ratio of column (8).
(iv) Gini response to GA is column (4) as a ratio of column (8).
(v) Change in growth rate during GA is the difference between the pre- and post-break growth rate of GDP per capita obtained from Kar et al. (2013).
Sources: World Bank (2013); Kar et al. (2013); author's calculations.

We find that for several of the countries, the response of the income shares of the bottom 20% to a growth acceleration was such that the bottom 20% were *worse off* during the growth acceleration: for example, Bangladesh, the People's Republic of China (PRC), Dominican Republic, Guatemala, Jordan, Nigeria, Poland, and

Romania. In contrast, there is more evidence of a decrease in headcount poverty during a growth acceleration: 15 out of the 23 countries (Jordan does not have data on the headcount poverty ratio) witnessed a decline in headcount poverty during a growth acceleration. However, when we observe the behavior of inequality as captured by the Gini, we find that several countries witnessed an increase in inequality in the growth acceleration—a total of 14 countries.

If we were to use the headcount ratio as the metric of poverty and the Gini coefficient as the metric of inequality, only 8 out of 24 countries—one-third of the sample—witnessed both a fall in poverty and inequality during growth accelerations. On average, the share of the bottom 20% and the headcount ratio fell by 0.2% and 1.3% per annum, respectively, and the Gini coefficient increased by 1.1% per annum for our sample of countries. This implies that if we use the income share of the bottom 20% of the income distribution as the metric of poverty and the Gini coefficient as the metric of inequality, the average country exhibited a *worsening* of inclusive growth during a growth acceleration phase.

Table 2 presents the annual percentage change in the income share of the bottom 20%, the headcount ratio at $1.25 a day, the ratio of top to bottom 10%, and the Gini coefficient, as well as the three summary statistics as in Table 1, for the same set of countries during their growth maintenance phase.[9] In this case, the three summary statistics provide the response of the income share of the bottom 20%, the headcount ratio, and the Gini coefficient to the magnitude of growth during the growth maintenance phase (column 8 in Table 2).[10] Here, we find many more countries witnessing an increase in the income share going to their bottom 20% (14 countries) as well as a fall in the $1.25 a day headcount ratio (19 countries). Several countries also witness a fall in inequality, whether measured by the top 10% to bottom 10% shares or by the Gini coefficient (13 countries in the first case, 14 countries in the second case).

There is evidence of a higher degree of inclusive growth in the growth maintenance phase—the income share of the bottom 20% increases by 0.7% per annum on average, the headcount ratio declines, on average, by 6.2% per annum and the Gini falls, on average, by 0.2% per annum. Twelve countries show a decline in both headcount poverty and inequality (as measured by the Gini) during the growth maintenance phase. Whichever metric one uses for poverty (the headcount ratio or

[9] For this exercise, the year of the poverty and inequality data is the last year in the growth maintenance phase for which data was available. The years are provided in Appendix 3b.

[10] We obtain the data on the magnitude of growth during the growth maintenance phase for each country from Kar et al. (2013). The magnitude of growth is calculated by taking the difference between the actual growth rate and the predicted growth rate if growth had not accelerated, and timing this with the duration of the growth phase. Predicted growth rate is obtained by running a separate prediction regression for each growth transition and predicting a country's growth rate on the basis of its previous growth and its level of per capita income. See Kar et al. (2013) for further details and justification of the manner growth magnitudes are calculated. It should be noted from Table 2 that the PRC witnesses the largest magnitude of growth followed by Malaysia, Iran, and India. This is a product of high growth rates during growth maintenance (relative to past growth) and the long duration of the growth phase in these countries.

Table 2. **Behavior of Poverty and Inequality during Growth Maintenance (GM)**

	Annual Change							
Countries	**Income Share, Bottom 20% (1)**	**HCR, $1.25 a day (– means decrease) (2)**	**Ratio of Income Share, Top 10% to Bottom 10% (3)**	**Gini (4)**	**Income Share of Bottom 20% Response to GM (5)**	**HCR Response to GM (6)**	**Gini Response to GM (7)**	**Magnitude of Growth during GM (8)**
Bangladesh	0.2	–2.4	–0.4	–0.3	0.02	–0.22	–0.03	11
Brazil	3.1	–7.7	–5.0	–1.4	1.56	–3.87	–0.68	2
PRC	–3.2	–10.0	6.1	1.5	–0.03	–0.09	0.01	114
Colombia	5.3	–12.5	–13.9	–1.2	0.66	–1.57	–0.15	8
Costa Rica	0.4	–6.3	–0.4	0.5	0.04	–0.63	0.05	10
Dominican Republic	0.6	–5.2	–1.8	–0.4	0.02	–0.16	–0.01	32
Ecuador	6.1	–15.0	–10.4	–1.8	0.68	–1.66	–0.20	9
Guatemala	2	–5.9	–2.7	–0.4	0.67	–1.97	–0.12	3
India	–0.4	–1.4	1.1	0.7	–0.01	–0.03	0.01	51
Iran	1.3	–6.1	–2.3	–0.8	0.02	–0.11	–0.01	55
Jordan	1.3	–16.5	–2.5	–1.1	0.1	–1.27	–0.08	13
Madagascar	–2.6	3.6	–0.1	–1.4	–2.56	3.62	–1.37	1
Malaysia	–0.3	n/a	0.2	–0.1	–0.01	0	0	63
Mexico	–1.6	–6.5	3.7	–0.3	–1.57	–6.53	–0.29	1
Morocco	0	–12.3	1.3	0.4	0	–0.88	0.03	14
Nicaragua	2.2	–0.7	–4.5	–1.6	0.22	–0.07	–0.16	10
Nigeria	0.5	0.5	–0.1	0.5	0.27	0.26	0.23	2
Panama	3.7	–6.5	–5.9	–1.1	0.12	–0.22	–0.04	30
Paraguay	3.4	–9.9	–5.4	–1.0	0.11	–0.33	–0.03	30
Peru	–1.4	–6.1	2.3	0.4	–0.05	–0.23	0.02	26
Poland	–1.0	3	2.6	1.5	–0.02	0.07	0.03	46
Romania	–0.4	–16.6	0.8	0.4	–0.01	–0.46	0.01	36
Tanzania	–1.1	–3.1	2.4	1.2	–0.07	–0.20	0.07	16
Zambia	–1.6	1	2.1	0.9	–0.10	0.06	0.06	16
Average	**0.7**	**–6.2**	**–1.4**	**–0.2**	**0.01**	**–0.69**	**–0.11**	**25**

HCR = headcount ratio, n/a = not available, PRC = People's Republic of China.

Notes:

(i) Annual change is equal to log change/T where T is number of years between the 2 years before and after growth maintenance. The years for each country are provided in Appendix 3b.

(ii) Income share of bottom 20% response to GM is column (1) as a ratio of column (8).

(iii) HCR response to GM is column (2) as a ratio of column (8).

(iv) Gini response to GM is column (4) as a ratio of column (8).

(v) Magnitude of growth during GM (column [8]) is obtained from Kar et al. (2013), and is the product of the difference between actual and predicted growth if the GM had not occurred and the duration of the GM phase (in years). The figures in col. (8) are in percentages.

Sources: World Bank (2013); Kar et al. (2013); author's calculations.

the income share of the bottom 20%) and using the Gini as the metric for inequality, the average country exhibits inclusive growth during the growth maintenance phase.

V. The Relationship between Institutions and the Inclusiveness of Growth

In this section, we provide a descriptive analysis of the relationship between institutional quality and the inclusiveness of growth as a precursor to the econometric analysis. Our core hypotheses are as follows: (i) that there will not be a significant reduction in poverty and inequality during a growth acceleration, while there may be a more significant reduction of poverty and inequality during a growth maintenance episode; and (ii) that the responsiveness of poverty and inequality will be greater, especially in a growth maintenance phase, if there is an improvement in institutional quality.

We explore these hypotheses in this section using simple bivariate scatter plots of the relationship of poverty and inequality response to growth acceleration (GA) and growth maintenance (GM) and changes in our measures of institutional quality during GA and GM.[11] In Figures 2 and 3, we plot the relationship between headcount poverty and inequality (Gini) responses to GA and changes in corruption, rule of law, and democratic accountability during a GA phase. In Figures 4 and 5, we plot the relationship between headcount poverty and inequality (Gini) responses to GM and changes in corruption, rule of law, and democratic accountability during a GM phase.

From Figure 2, we find that the relationship between poverty response to GA and changes in corruption, rule of law, and democratic accountability during a GA is either flat or even positive. In the case of inequality response to GA, we find that its relationship with changes in corruption, rule of law, and democratic accountability during a GA is flat or weakly negative (Figure 3). Thus, improvements in institutional quality, wherever they have occurred during GA, do not seem to be strongly associated with declines in poverty and inequality during a GA phase.

In contrast, when we observe the relationship between the responses of poverty and inequality to GM and institutional change during GM (Figures 4 and 5, respectively), we see that there is a strong negative relationship between poverty response and improvements in democratic accountability (Figure 4c), and between inequality response to a GM and decreases in the degree of corruption (Figure 5a) and improvements in the rule of law (Figure 5b).[12]

In sum, there is suggestive evidence that the behavior of poverty and inequality is different during growth acceleration and growth maintenance and that improvements in institutional quality are more likely to be associated with declines in poverty and inequality during growth maintenance. In the next section, we investigate these relationships more systematically using multivariate regression methods.

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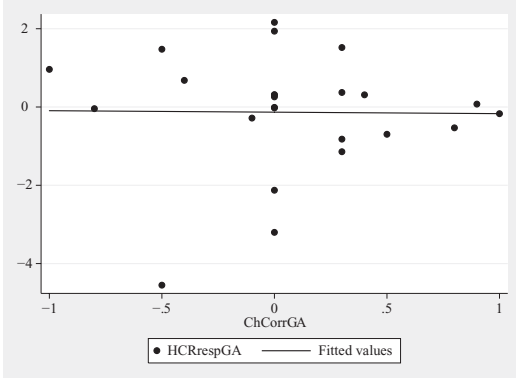
[11] As we have annual data on institutional quality from ICRG (unlike the poverty and inequality data), we first take 5-year averages of corruption, rule of law, and democratic accountability before and after the growth break. We then take the difference between the pre-break and post-break 5-year averages.

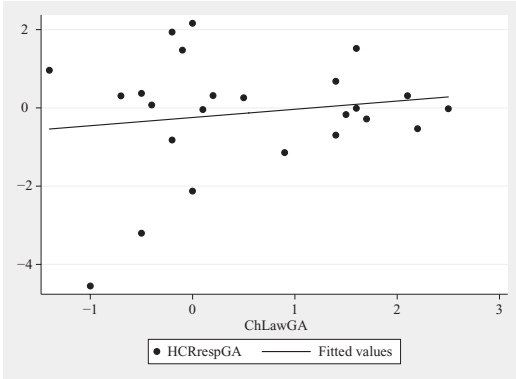
[12] However, the positive relationship between inequality and democratic accountability in GM is counterintuitive.

Figure 2. **Responsiveness of Poverty (Headcount Ratio, HCR) to Institutional Change during Growth Acceleration (GA)**

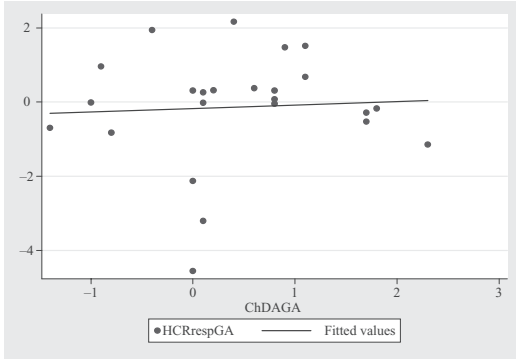
2a. Corruption



2b. Rule of Law



2c. Democratic Accountability



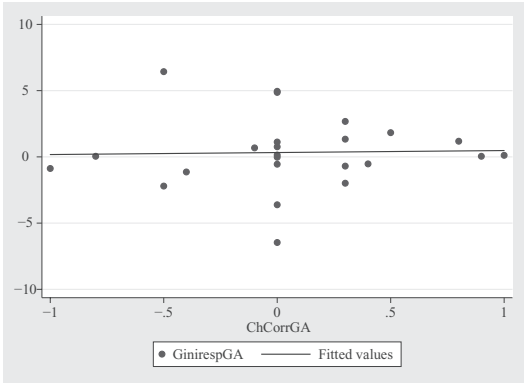
ChCorrGA = change in corruption measure during GA, ChDAGA = change in democratic accountability measure during GA, ChLawGA = change in rule of law measure during GA, HCRrespGA = HCR response to GA.

Note: Fitted values are obtained from ordinary least squares regression of HCRrespGA to changes in institutional variables.

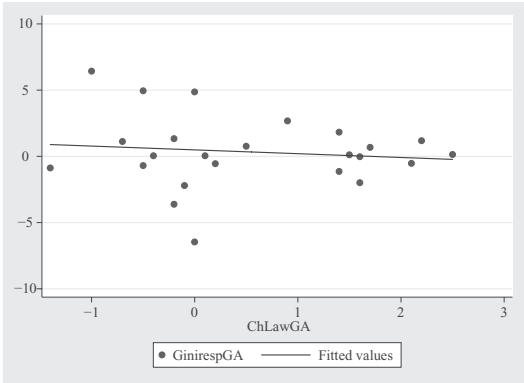
Source: Author's calculations.

Figure 3. **Responsiveness of Inequality (Gini) to Institutional Change during Growth Acceleration (GA)**

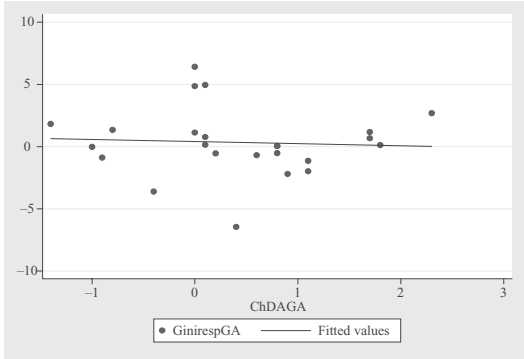
3a. Corruption



3b. Rule of Law



3c. Democratic Accountability



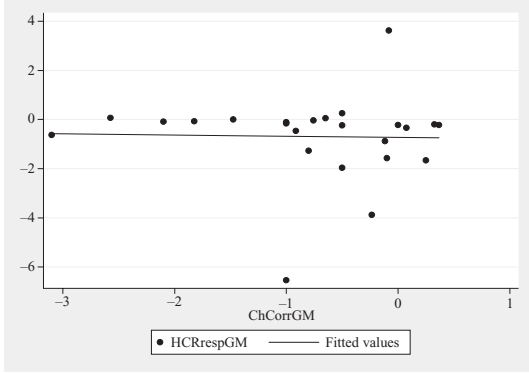
ChCorrGA = change in corruption measure during GA; ChLawGA = change in rule of law measure during GA, ChDAGA = change in democratic accountability measure during GA, GiniespGA = Gini response to GA.

Note: Fitted values are obtained from ordinary least squares regression of GiniespGA to changes in institutional variables.

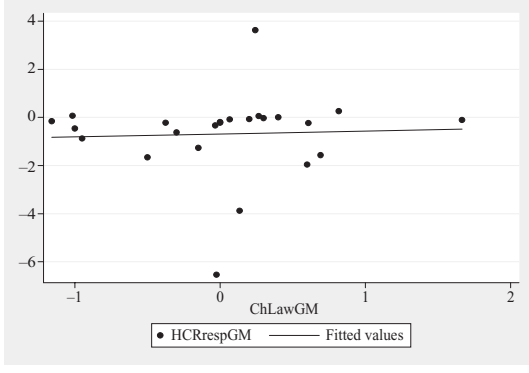
Source: Author's calculations.

Figure 4. **Responsiveness of Poverty (Headcount Ratio) to Institutional Change during Growth Maintenance (GM)**

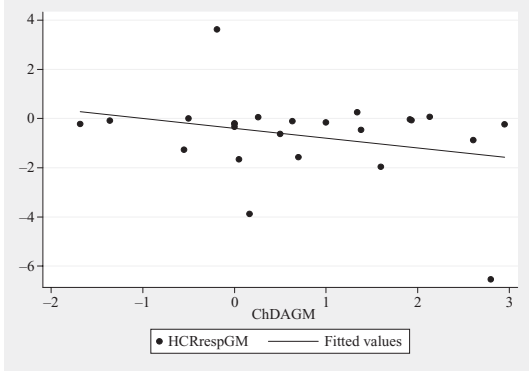
4a. Corruption



4b. Rule of Law



4c. Democratic Accountability



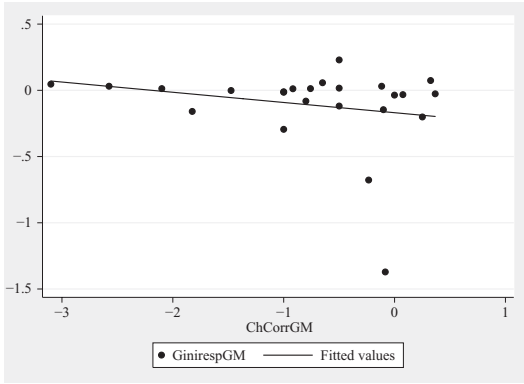
ChCorrGM = change in corruption measure during GM, ChDAGM = change in democratic accountability measure during GM, ChLawGM = change in rule of law measure during GM, HCRrespGM = HCR response to GM.

Note: Fitted values are obtained from ordinary least squares regression of HCRrespGM to changes in institutional variables.

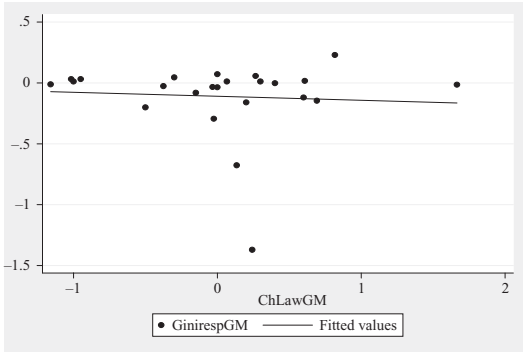
Source: Author's calculations.

Figure 5. **Responsiveness of Inequality (Gini) to Institutional Change during Growth Maintenance (GM)**

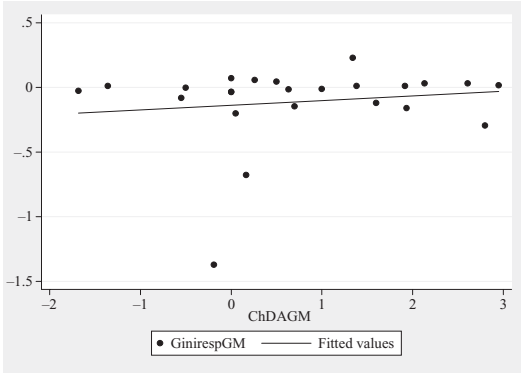
5a. Corruption



5b. Rule of Law



5c. Democratic Accountability



ChCorrGM = change in corruption measure during GM, ChDAGM = change in democratic accountability measure during GM, ChLawGM = change in rule of law measure during GM, GiniespGM = Gini response to GM.

Note: Fitted values are obtained from ordinary least squares regression of GiniespGM to changes in institutional variables.

Source: Author's calculations.

VI. Econometric Analysis and Results

In this section, we undertake econometric analysis to assess the validity of our main hypotheses. Reiterating our main theoretical propositions, we expect that inclusiveness of growth will be less evident during growth accelerations; that growth maintenance phases will be more likely to lead to inclusive growth; and that the extent of inclusive growth during growth maintenance will be positively associated with improvement in the inclusivity of institutions, though less so in the case of growth acceleration.

To test these hypotheses, we first construct a composite variable for inclusive growth (*POVINQ*) which is the sum of the headcount ratio and the Gini. The idea behind this simple measure is that it captures both the poverty and inequality dimensions of inclusive growth. Using this measure, a growth episode can be termed inclusive if both poverty and inequality are falling, or if the fall in poverty is greater than the increase in inequality where both variables are moving in different directions (and vice versa). We then run regressions of the following form:

$$POVINQ_{it} = A_1 + A_2 * GA_{it} + A_3 * GM_{it} + A_4 * GA_{it} * INST_{it} + A_5 GM_{it} * INST_{it} + A_6 * INST_{it} + A_7 Z_{it} + error_{it} \quad (1)$$

where *POVINQ* is our measure of inclusive growth, subscript *i* denotes the country, and subscript *t* denotes time.

We use panel data for countries where we have at least three observations of poverty data (one before the acceleration, one after the acceleration, and one much later during growth maintenance) and where we have data on institutions from 1984 to 2010 (we exclude all advanced market economies). There are 42 countries in all including countries that did not see a growth acceleration during 1984–2010 and countries that had growth maintenance for all the years for which we have poverty and inequality data.

The variable *GA* captures the growth acceleration phase and is a dummy variable that takes the value 1 for the year when growth acceleration occurred, 0 otherwise. The variable *GM* captures the growth maintenance phase and is a dummy variable that takes the value 1 for all intervening years between a growth acceleration and a growth decline. If there had been no growth decline following growth acceleration, the dummy remains equal to 1 for the rest of the period of analysis. The variable *INST* is a measure of institutional quality. As in the previous section, we use 3 different measures: corruption (higher values denote less corruption), rule of law, and democratic accountability. We also interact GA and GM with our different institutional quality variables—this captured by the interaction variables $GA * INST$ and $GM * INST$. Finally, we let Z be a vector of control variables. Our unit of time is one year, and the panel is unbalanced due to the lack of data on poverty and inequality for some country-years.

Based on our hypotheses, we would expect that A_2 to be either statistically insignificant or negative and significant, while A_3 would be negative and significant. Moreover, if A_2 and A_3 turn out to be both negative, then A_3 would likely be greater in magnitude than A_2. We expect A_4 to be statistically insignificant and A_5 to be negative and significant, as we expect that improvements in institutional quality would not affect inclusive growth in a growth acceleration phase, but would do so in a growth maintenance phase. The direct effect of the institutional quality variable on inclusive growth can be expected to be positive, so the coefficient A_6 would be negative and statistically significant.

We use three controls that are standard in the growth/poverty/inequality empirics literature: (i) the ratio of government consumption spending to GDP (*GOVTCONS*); (ii) the openness of the economy, as measured by the ratio of total exports and imports of goods and services to GDP (*OPEN*); and (iii) a dummy for oil-exporting economies (*OILEXP*). Government social expenditures (e.g., on education and health) may lead to more inclusive growth. However, reliable panel data on government social expenditure is not available, and we are confined to using government consumption expenditures as a proxy (Iradian 2005). Greater openness may lead to more inclusive growth, though this has been debated (Winters, McCulloch, and McKay 2004). Oil-exporting countries would have a higher share of revenue from natural resources, and this may allow them to spend more on the social sector, leading to more inclusive growth. However, this may also bias growth away from labor-intensive sectors, which may lead to less inclusive growth (Sachs and Warner 1999).[13]

We present our results in Table 3. In the first column, we regress *POVINQ* on GA and GM, along with the control variables—*GOVTCONS*, *OPEN*, and *OILEXP*. We use ordinary least squares estimation, with standard errors corrected for country-level clustering.[14] We find the growth maintenance phase to be associated with declines in inclusive growth, but not the growth acceleration phase.

In the second column, we include the corruption variable (*CORR*) and its interaction with *GA* and *GM*. While corruption does not have a direct effect on inclusive growth (in that a lower degree of corruption does not lead to faster declines in poverty/inequality), we find that countries that had seen declines in the degree of corruption in their growth maintenance phase also witnessed declines in poverty/inequality. That is, the coefficient on the interaction term ($GM * CORR$) is negative and statistically significant. In contrast, a decline in the degree of corruption

[13] High levels of literacy are also expected to lead to more inclusive growth. However, the commonly used Barro–Lee data is only available quinquennially, and using this data reduces the number of observations in our panel data considerably from around 350 observations to less than 70.

[14] It should be noted that we cannot include country and year fixed effects. In the case of the former, the GM variable will be washed out for countries where growth acceleration occurred at an early stage. In the case of the latter, the GA variable will be washed out, as many countries experienced growth accelerations simultaneously.

Table 3. **Regression Results**

Variables	**(1)**	**(2)**	**(3)**	**(4)**
Constant	69.04***	71.72***	86.32***	71.08***
	(19.55)	(4.99)	(4.89)	(5.50)
GA	1.70	–0.70	1.75	–1.40
	(4.20)	(1.10)	(1.18)	(1.21)
GM	–3.65**	–4.53***	–5.10***	–5.67**
	(2.10)	(1.33)	(1.39)	(1.41)
CORR		–1.24		
		(0.85)		
GA ∗ *CORR*		0.85		
		(4.91)		
GM ∗ *CORR*		–3.11*		
		(1.89)		
LAW			–6.17***	
			(0.90)	
GA ∗ *LAW*			0.90	
			(4.65)	
GM ∗ *LAW*			–3.21**	
			(1.63)	
DA				–0.26
				(1.25)
GA ∗ *DA*				3.02
				(4.54)
GM ∗ *DA*				–3.86**
				(1.47)
GOVTCONS	–0.51*	–0.61*	–0.63*	–0.62*
	(0.24)	(0.21)	(0.22)	(0.21)
OPEN	0.01	0.04	0.03	0.04
	(0.02)	(0.03)	(0.03)	(0.03)
OILEXP	2.06	2.08	2.16	2.24
	(3.62)	(3.65)	(4.11)	(3.87)
R square	0.22	0.31	0.32	0.28
Number of observations	350	350	350	350

*** = significant at the 1% level,** = significant at the 5% level,* = significant at the 10% level, CORR = corruption, DA = democratic accountability, GA = growth acceleration, GM = growth maintenance, GOVTCONS = government consumption, LAW = rule of law, OPEN = openness, OILEXP = oil exporter.

Note: The dependent variable is the composite measure of inclusive growth *POVINQ*, the sum of the headcount ratio and the Gini coefficient. Figures in parentheses are t-ratios, with robust standard errors corrected for country-level clustering.

Source: Author's estimates.

in the growth acceleration phase does not seem to be associated with a reduction in poverty/inequality. The coefficient on the interaction term (*GA* ∗ *CORR*) is not statistically significant.

In column 3, we include the rule of law (*LAW*) in the regression, by itself and in interaction with *GA* and *GM*. Interestingly, we see that improvements in the rule of law has a direct negative effect on poverty and inequality, as the coefficient on *LAW* is negative and statistically significant. The coefficient on the interaction term

for *GA* and *LAW* is not statistically significant, but the coefficient on the interaction term for *GM* and *LAW* is negative and significant.

Finally, in column 4, we include democratic accountability (*DA*), by itself and in interaction with *GA* and *GM*, and obtain similar results as for corruption and the rule of law. Our results suggest that poverty/inequality reduction is more likely to occur during the growth maintenance phase. In addition, improvements in institutional quality in this phase—whether in the form of lower corruption, greater rule of law, or greater democratic accountability—are likely to contribute to further reduction in poverty/inequality. In contrast, growth acceleration appears unlikely to lead to poverty/inequality reduction, independent of institutional quality improvements.[15]

VII. Conclusions

The achievement of inclusive growth in the developing world is a significant challenge for policy makers in international development. What are the fundamental causes of inclusive growth, and when may we expect to witness inclusive growth during a growth experience of a particular country? This paper examines the institutional preconditions and argues that the inclusiveness of growth varies across growth phases within countries. It derives some possible testable hypotheses from the recent literature on institutions and provides empirical evidence that support the hypotheses.

When may we expect inclusive growth? It is most likely to be witnessed when economic growth for a particular country has accelerated and the country is in growth maintenance phase and when inclusive institutions have emerged. When may we not expect inclusive growth? It is unlikely to be witnessed at the onset of economic growth, especially if the acceleration in economic growth has been caused by informal or extractive institutions. Our findings imply that from a policy perspective, in countries that have not yet witnessed a growth acceleration or where growth is on a decline, it is arguably more important to get growth started, as the inclusivity of growth may have to come later. Once growth has accelerated, it is important to facilitate the emergence of inclusive institutions as the greater the inclusivity of institutions, the more likely that economic growth will be inclusive.

[15] We also do further robustness tests of our results. First, we use different timings of growth accelerations as in Berg, Ostry, and Zettelmeyer (2012) and Jones and Olken (2008). Secondly, we construct a different growth acceleration variable, where the dummy takes the value 1 for the year of the growth acceleration and the 2 years following it. Our results do not change with these changes in the construction of the growth acceleration and maintenance variables. Finally, we calculate *POVINQ* with different weighting given to poverty and inequality with no change in our results.

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Appendix 1: Identifying Breaks in Growth Rates

The empirical literature that studied growth phase transitions can be classified under two distinct approaches: the "filter-based" approach (Hausmann, Pritchett, and Rodrik 2005) and the "statistical break test-based" approach. The former approach identifies growth breaks on the basis of subjectively defined rules, while the latter approach uses estimation and testing procedures that identify growth breaks in terms of statistically significant changes in (average) growth rates. Some studies use a combination of the two approaches.

The contributions that have adopted the statistical approach have mostly used the Bai–Perron methodology (1998) which locates and tests for multiple growth breaks within a time series. In this method, an algorithm first searches all possible sets of breaks (up to a maximum number of breaks) and determines for each number of breaks the set that produces the maximum goodness of fit. The statistical tests then determine whether the improved fit produced by allowing an additional break is sufficiently large, given what may be expected by chance (Jones and Olken 2008). Starting with a null of no breaks, sequential tests of k versus $k+1$ breaks allow one to determine the appropriate number of breaks. Bai and Perron (1998) determine critical values for tests of various sizes and employ a trimming parameter, expressed as a percentage of the number of observations, which constrains the minimum distance between two breaks. Examples of the statistical-based method to identify growth breaks are Jones and Olken (2008) and Kerekes (2012).

Both filter-based and statistical-based methods have their limitations. The simple rules that are used in filter-based methods are often ad hoc and downplay the inherent volatility in income data for developing countries. This leads to an identification of a breakpoint in the per capita income series when there actually may be none. A key limitation of the statistical-based method which uses the Bai–Perron tests for structural breaks is that the latter has low power, leading to rejection of structural breaks even when they are "true" breaks.

In order to address the limitations of the filter-based and statistical-based methods, Kar et al. (2013) combine both these methods in a manner that retains the strength of both methods, while attempting to compensate for the weaknesses of each. In order to capture a larger number of "true" breaks than may be provided by the application of the Bai–Perron method, they propose a two-step method that first uses the Bai–Perron estimation technique to identify potential breaks and then uses a "filter" to confirm the genuine breaks.

The first step entails using the Bai–Perron technique to estimate the best "potential" breaks for 125 countries (all countries with population of over a threshold of seven hundred thousand and based on data availability on purchasing power parity GDP per capita since at least 1970 in the Penn World Tables, version 7.1). Kar et al. (2013) assume a minimum distance of 8 years between 2 breaks to minimize the possibility of conflating business cycles with breaks in growth rates (see also Berg, Ostry, and Zettelmeyer 2012). They also assume that countries with 40 years of

data can have a maximum of 2 breaks, countries with 50 years of data can have a maximum of 3 breaks, and countries with 60 years of data can have a maximum of four breaks.

Once the "potential" breaks have been estimated, the second step of the methodology uses the following filter in order to confirm the genuine breaks: (i) if an up break follows another up break or a down break follows another down break, then a 1% change would classify the break as a genuine break; (ii) if an up break follows a down break or a down break follows an up break, then a 3% change would classify the break as a genuine break; and (iii) in case of the first break, since it is not known whether it follows an up break or a down break, a 2% change would classify as a genuine break.

Using this methodology, Kar et al. (2013) find a total of 318 breaks (both up breaks and down breaks) from a group of 125 countries. The identification of breaks in economic growth allows them to identify the years when a particular country is witnessing a growth acceleration or growth deceleration, and if the country has witnessed a growth acceleration previously, how long the country is in a growth maintenance phase—the period between the year of growth acceleration (an up break) and the year of growth deceleration (a down break).

Appendix 2: Years of Growth Breaks

Country	Year of Growth Break
Bangladesh	1998
Brazil	2002
China, People's Republic of	1991
Colombia	1994
Costa Rica	1991
Dominican Republic	1991
Ecuador	1999
Guatemala	1988
India	1993
Iran	1988
Jordan	1991
Madasgascar	2002
Malaysia	1987 (down break in 1996)
Mexico	1989
Morocco	1995
Nicaragua	1995
Nigeria	1987
Panama	2002
Paraguay	2002
Peru	1992
Poland	1991
Romania	1994
Tanzania	2000
Zambia	1994

Note: Breaks only for the period 1984–2010.
Source: Kar et al. (2013).

Appendix 3a: Year for which Poverty and Inequality Data are Obtained from *World Development Indicators* prior to Growth Acceleration

Country	Year of Growth Break
Bangladesh	1992
Brazil	1997
China, People's Republic of	1987 (1993)
Colombia	1999
Costa Rica	1986
Dominican Republic	1988
Ecuador	1995
Guatemala	1987
India	1987 (1994)
Iran	1986 (1990)
Jordan	1987 (1992)
Madasgascar	1999 (2005)
Malaysia	1984
Mexico	1984 (Ginni – 1992)
Morocco	1991 (1999)
Nicaragua	1993 (1998)
Nigeria	1986 (1992)
Panama	1998
Paraguay	1999
Peru	1986 (1994)
Poland	1987 (1992)
Romania	1989
Tanzania	1992
Zambia	1993 (1996)

Note: Years in parentheses are years in which data on Gini is available.

Appendix 3b: Year for which Poverty and Inequality Data are Obtained from *World Development Indicators* for the Latest Year of Growth Maintenance

Country	Year of Growth Break
Bangladesh	2010
Brazil	2009
China, People's Republic of	2005
Colombia	2010
Costa Rica	2009
Dominican Republic	2009
Ecuador	2010
Guatemala	2007
India	2006
Iran	2006
Jordan	2011
Madasgascar	2009
Malaysia	2009
Mexico	2008
Morocco	2007
Nicaragua	2005
Nigeria	2010
Panama	1998
Paraguay	2010
Peru	2009
Poland	2009
Romania	2009
Tanzania	2007
Zambia	2006

Globalization and the Quality of Asian and Non-Asian Jobs

Robert J. Flanagan and Niny Khor*

This paper assesses the impact of international trade and investment flows on the evolution of working conditions and labor rights in Asian and non-Asian countries in the late 20th and early 21st centuries. Labor conditions improved as globalization increased during this period. We find that real per capita income growth remains a powerful source of improved labor conditions, and the effect of trade on working conditions is mainly indirect through its impact on per capita gross domestic product (GDP). We find no evidence that eliminating trade barriers degrades labor conditions. We do find evidence that persistent differences in labor conditions between Asia and the rest of the world can be explained by differences in growth and international trade. Finally, we find no evidence that countries with poor labor conditions attract disproportionate flows of foreign direct investment (FDI). Instead, FDI flows seem mainly influenced by considerations of market size, investment risks, and the share of trade in GDP. After holding those influences constant, Asia receives a comparatively small share of world FDI inflows.

Keywords: globalization, labor conditions, foreign direct investments, Asia
JEL codes: F21, F66, J81

I. Introduction

Efforts to reduce barriers to international trade and investment frequently encounter claims that expanding international competition degrades working conditions and labor rights, particularly in developing countries. Opponents of trade liberalization argue that international competition encourages jobs with low pay and poor nonmonetary conditions, such as long work hours and unsafe working environments. They claim further that free trade undermines the four "core" labor rights stressed by most international economic organizations: freedom of association, nondiscrimination, elimination of forced labor, and reduction of child labor. Asian countries figure prominently in lists of low-wage production sites, and anecdotes from Asian countries are frequently invoked to support these assertions. This paper explores the relationships between labor conditions (i.e., working conditions plus

*Robert J. Flanagan is the Konosuke Matsushita Professor of International Labor Economics and Policy Analysis, Emeritus, at the Stanford Graduate School of Business. Niny Khor is an economist with the Asian Development Bank Resident Mission in the People's Republic of China. The authors gratefully acknowledge outstanding research assistance by Ryan Jacildo.

core labor rights) and the growth of international trade and investment, emphasizing differences between Asia and the rest of the world.

We begin by briefly reviewing how labor conditions changed during the globalization of the late 20th and early 21st centuries, contrasting developments in Asian and non-Asian countries in section II. Previous analyses of the relationship between trade and labor conditions focus on the last decades of the 20th century and take a global approach (Flanagan 2006, Kucera 2002); they provide no comparisons of experience in Asia and other regions. We then discuss the mechanisms through which trade might influence working conditions and labor rights and estimate the impact of trade flows on working conditions and labor rights in section III. Section IV examines the links between direct foreign investment, the activities of multinational companies (MNCs), and labor conditions. The final section presents our conclusions regarding the long-run adjustment of labor conditions to trade liberalization.

II. Labor Conditions in Asian and Non-Asian Countries

Whether measured by flows of international economic activity or the restrictiveness of trade policies, the globalization of Asia (along with the Americas) ranked about the middle of the international league tables in the late 20th and early 21st century. By these same measures, Europe was the most globalized region and Africa the least. Globalization increased in each of these regions from the mid-1990s until the recession that ended during the first decade of the 21st century, making this interval a fruitful period to examine possible links between globalization and labor conditions.[1]

We first contrast working conditions and labor rights in Asian countries with those in the rest of the world at the end of the 20th century. We then examine how these labor conditions changed in the first decade of the 21st century. Working conditions include measures of pay (annual compensation per manufacturing worker), work hours (weekly work hours, annual work hours, and the percentage working more than 40 hours per week), and job safety (fatal industrial accident rate in manufacturing). Labor rights include indicators of freedom of association (indexes of civil liberties and collective bargaining rights, scaled so that low values indicate superior rights), children's employment (labor force participation rate of children 5–14 years old), nondiscrimination (gender pay differential), and forced labor (number of types of forced labor and number of forced laborers).[2] Some of these indicators exist only for

[1] The globalization indexes are respectively the *Konjunkturforschungsstelle* (KOF) index of actual economic flows and the KOF index of trade restrictions. The former index is a weighted average of flows of trade, foreign direct investment, portfolio investment, and income payments to foreign nationals, all taken as a percent of gross domestic product. The latter index is a weighted average of the mean tariff rate, hidden import barriers, taxes on trade, and capital account restrictions. The data along with further details on the construction of the indexes are available at http://globalization.kof.ethz.ch/

[2] To enable comparison with global data over time, we follow ILO (2010) by defining children's participation in work employment as *child employment/child labor force participation*. This is broader than the term child labor

Table 1. **Labor Conditions in Asian and Non-Asian Countries, Late 20th Century**

	Asian	Non-Asian
Working Conditions		
Annual Compensation (1995)	2,643	17,630
Weekly Work Hours (1995)	46.7	39
Annual Work Hours (1995)	1,977	1,732
Percent Working over 40 Hours (1995)	73.4	57.3
Fatal Accident Rate (2000)	5.5	5.9
Labor Rights		
Child Labor Force Participation Rate (2000)	19.1	15.7
Civil Liberties Index (2000)	4.6	3.1
Collective Bargaining Rights (mid-1990s)	7.7	5
Net Gender Wage Differential	–0.085	–0.105
Forced Labor Varieties (mid-1990s)	1.26	0.03
Number of Forced Laborers (mid-1990s)	5,312,927	39,670

Note: Labor force weighted estimates.
Source: See Appendix.

one year or time period, and data availability varies widely for each country. For no measure of labor conditions do we have data for every Asian country, for example.[3] In the empirical analyses, the value of each country observation is usually weighted by its labor force, with exceptions noted. (See the Appendix for further discussion of these measures and their sources.)

In the late 20th century, monetary compensation was comparatively low, and all measures of work hours were comparatively high in Asian countries (Table 1). Job safety (inversely indicated by the fatal job accident rate) was greater in Asian countries. Turning to measures of labor rights, both measures of freedom of association—the Freedom House index[4] and the freedom of association and collective bargaining index,[5] which focuses on collective bargaining rights—indicate that freedom of association is stronger on average in non-Asian countries. (Recall that each of these indexes is constructed so that lower scores denote superior rights.) Child labor force participation is slightly higher in Asian (19.1%) than non-Asian (15.7%) countries. Both measures of forced labor are higher in Asian countries; on average, there are more varieties of forced labor and more people subject to forced labor in Asia. Finally, by our measure there is somewhat less gender wage discrimination in Asian countries.

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or "hazardous work." In the most recent global review, child labor accounts for about 87% of all child employment, while hazardous child labor accounted for approximately half of child labor (ILO 2010). We discuss this further in the Appendix.

[3] Data on labor conditions are most frequently available for the People's Republic of China; India; Indonesia; Japan; the Republic of Korea; Malaysia; the Philippines; Singapore; Taipei,China; and Thailand.

[4] The annual Freedom House index is not limited to worker freedom of association. However, an index of worker rights developed for a 2010 study (Freedom House 2010) was highly correlated with the general Freedom House index for that year. Therefore, the general index, which is available annually, appears to provide an adequate measure of worker rights. See the Appendix for further discussion.

[5] This workplace-oriented index is compiled by Kucera (2002).

Table 2. **Labor Conditions: Recent Developments**

	2008		% Change since 1999	
	Asian	**Non-Asian**	**Asian**	**Non-Asian**
Working Conditions				
Hourly Compensation	3.7	19.4	236.6	152.6
Annual Work Hours	2,156.0	1,914.0	2.3	0.2
Job Accident Rate	5.9	n.a.	−22.2	n.a.
Labor Rights				
Child Labor Force Participation Rate* (%)	14.8	14.2	−22.7	−9.6
Civil Liberties	4.5	2.0	−8.1	−22.3

n.a. = not available.
Note: Labor force weighted estimates.
*Child labor force participation of 5–14 year olds, in 2000 and 2008.
Source: See Appendix.

How did labor conditions *change* with the globalization of the late 20th and early 21st century? Tracking the changes in conditions requires before and after data for a common set of countries—a requirement that further reduces sample sizes and eliminates meaningful comparisons for some measures.

Previous research found a broad improvement in worldwide working conditions and labor rights during the last decades of the 20th century (Flanagan 2006). The early years of the 21st century also show improving labor conditions around the world (Table 2).[6] Pay, job safety, and freedom of association all improved in both Asian and non-Asian countries. Annual work hours increased more in Asian than in non-Asian countries but it is difficult to disentangle cycle influences from other factors. Child labor also decreased in both Asian and non-Asian countries. During this period, per capita (purchasing power parity [PPP] adjusted) GDP grew at virtually identical rates in the two sets of countries, but the trade share of GDP advanced more rapidly in Asian countries. At least in the descriptive data, there is no sign of a negative relationship between globalization and labor conditions.

Nonetheless, these descriptions do not establish that globalization improves labor conditions or even help us understand how globalization might influence working conditions and labor rights. Having described the evolution of globalization and labor conditions, we now analyze linkages between trade and labor conditions in the early 21st century.

III. Trade and Labor Conditions

Traditional trade theories imply that in countries that specialize in their comparative advantage, labor will move over time into sectors where productivity and

[6]Some of the measures used in Table 1 have changed, and others are no longer available. See the Appendix for definitions on the measures used in Table 2 for 1999–2008.

hence (monetary plus nonmonetary) compensation is highest. In this scenario, countries adopting open trade policies should develop superior working conditions over time as labor moves from import-competing to export production. At the other extreme are arguments that free trade will degrade labor conditions as international competitors seek to gain advantage by cutting labor costs.

If we are to isolate the effects of trade and other mechanisms of globalization on labor conditions empirically, we must first consider how working conditions and labor rights evolve in closed economies. Not surprisingly, the foremost influence on labor conditions is a country's level of development. Countries with higher income per capita tend to have higher wages, shorter hours of work, and safer jobs. High-income countries also have stronger labor rights—stronger civil liberties and freedom of association, lower child labor force participation, and less forced labor.

Over time, countries that grow most rapidly experience the most rapid advances in working conditions. Even under autarky, a country's labor conditions can improve with higher rates of technical progress, investments in physical and human capital, and the establishment of institutions that clarify property rights, enforce contracts, and reduce corruption, for example. To an important extent, the inequality in pay, nonmonetary working conditions, and labor rights observed around the world result from differences in the level of economic development and national economic growth rates (Flanagan 2006).

Stressing the important role of economic growth and development should not obscure the huge variance in outcomes around this relationship. Earlier research found that countries at a given level of development vary widely in their labor conditions. The fact that some countries have much better conditions while others have much worse conditions than one would predict from their level of development reflects a myriad of additional factors that influence labor conditions. The rest of this section analyzes one of these factors—the influence of trade flows.

A. Direct and Indirect Effects of Trade

International trade theories predict that free trade will improve a country's working conditions indirectly by increasing its per capita income. Whether comparative advantage or economies of scale motivates trade, a country's resources are used more productively in a free-trade environment than under autarky. The greater efficiency permits higher monetary and/or nonmonetary compensation. Transfers of technology that may accompany increased trade flows likewise raise productivity and compensation. In each case, free trade should improve working conditions to the extent that it raises per capita income.

To the extent that trade liberalizations raise per capita income, they become a mechanism for improving a country's working conditions and labor rights. A large literature has explored and debated the lines of causality between openness to trade and per capita income. After sorting out the significant methodological issues

involved in identifying a relationship, key studies and literature reviews conclude that trade liberalizations tend to raise economic growth (Berg and Krueger 2003, Wacziarg and Welch 2008). This channel provides what we label the *indirect* effect of globalization on labor conditions. Important distributional effects accompany the long-run gains from trade liberalizations, so that efforts to record the short-run impact of trade liberalization on working conditions with aggregate data pick up some average of the impact on gainers and losers.

Arrayed against the predictions of trade theories are claims that international competitive pressures degrade working conditions and labor rights in countries with open trade policies. How trade would diminish working conditions is a matter of some mystery. Open trade policies raise foreign demand for a country's exports and for the services of workers who produce those exports. What then happens to wages and nonmonetary working conditions depends on labor supply conditions, which themselves are determined by the domestic labor market alternatives available to workers. Where there is substantial unemployment or underemployment, increased export demand will raise employment without necessarily improving pay and nonmonetary working conditions. This situation may be the norm in countries with significant reserves of underemployed rural agricultural labor or high urban unemployment rates. The additional employment derived from increased export demand will raise total wage income while producing little change in the pay and other employment conditions of *individual* workers.

For economies with little unemployment, export firms will have to meet additional demand by attracting workers away from other jobs in agriculture, the informal sector, or elsewhere in the formal sector. As export firms improve working conditions to attract workers, non-export firms may improve working conditions in an effort to retain their workers. Labor market competition effectively spreads the benefits of increased export demand to other sectors. Trade liberalizations may also reduce the demand in import-competing industries, so that to an extent, the positive impacts of trade on labor conditions rest on the mobility of resources from import-competing to export industries.

Convincing scenarios in which increased export demand degrades working conditions remain elusive. If increased export production raised monopsony power, trade liberalization could produce such degradation. Nonetheless, it is hard to imagine how increased export production would reduce workers' choice of employers.

Comparisons of wages in export and non-export firms in both developing and industrialized countries support these arguments. These studies invariably find that after controlling for industry and firm size, export firms pay higher wages than non-export firms, and the "export wage premium" is largest in less developed countries (Aw and Batra 1998, Bernard and Jensen 1995, Hahn 2004, Van Biesebroeck 2003). In short, international competition does not lead exporters to reduce wages below national norms according to these studies. Since the studies rarely can control for all worker skills, the possibility that the employees of exporters have more education, training, and experience than the employees of non-exporting firms remains.

Nevertheless, one can doubt that unobserved worker quality differences account for wage premiums as large as 10%–12% in the Republic of Korea; 15%–17% in Taipei,China; and 40% in Sub-Saharan Africa.

Although there has been little previous attention to theoretical links between free trade and core labor rights, increased trade alters some of the incentives that influence these rights. Consider first the effects on children in employment. Since child labor force participation falls as adult incomes increase, trade liberalization should *reduce* the numbers of working children through the positive effects of free trade on per capita income. Increased trade carries with it a potential countervailing effect on child labor force participation, however. For a given level of family income, the relative return to current work versus schooling, summarized by the rate of return to schooling, will influence the extent of children's employment. If reducing trade barriers raises the wage of unskilled work and reduces the return to schooling, the relative attractiveness of schooling to children and their families falls. On the other hand, if trade expansion includes technology transfers that raise returns to schooling, incentives for children to attend and remain in school increase.

Consider next employment discrimination. The leading theory of labor market discrimination predicts that increased competition to hire labor should erode discrimination by providing labor force minorities with additional employment opportunities with employers who have less discriminatory tastes (Becker 1957). To the extent that open trade policies increase the number of export firms and/or MNCs competing for labor in local labor markets, discrimination by employers may decrease (Bhagwati 2004, pp. 75–76).

In theory, the linkage between trade and workers' freedom of association rights is ambiguous. One underlying question is how free trade influences the relative bargaining power of labor and management. On the one hand, a larger number of export firms or MNCs are likely to reduce any employer monopsony power, thereby increasing workers' choice of employers and hence their bargaining power. On the other, competition from imports and the increased ability of local employers to outsource may reduce workers' bargaining power. In short, the net effect of open trade policies on bargaining power must be settled empirically.

To summarize, the hypothesis that increased trade will degrade working conditions and labor rights lacks theoretical support, except in the dubious case that monopsony power grows with trade. Theoretical considerations also suggest that to the extent that free trade influences labor conditions, it will be *indirectly* through the effect of trade on a nation's GDP. We now turn to the evidence on links between trade and labor conditions in the late 20th and early 21st centuries.

B. Trade and Labor Conditions in the Late 20th Century

Econometric analyses reported in an earlier study (Flanagan 2006) tested whether a country's openness to international competition was significantly related to labor conditions, given a country's level of development, in the late 20th century.

As implied by international trade theories, openness influenced working conditions (pay, work hours, and job safety) only indirectly, by raising per capita income, in both cross-section instrumental variables and fixed effects estimation. The openness measures, which tested for a direct effect, were not statistically significant—in short, trade liberalization improved working conditions mainly by raising per capita income. The study found no negative impact of international competition on working conditions.

That study also found more complex links between labor rights and trade (captured by both the Sachs–Warner index of openness and the trade share of GDP). For 1980–1995, both the adoption of free trade policies and increased trade shares were associated with lower child labor force participation rates after controlling for per-capita GDP and institutional structure (Flanagan 2006). Greater openness to international markets therefore reduced the number of working children in two ways. To the extent that trade raised per capita income, fewer families needed to rely on children's labor force participation to obtain the necessities of life. Greater openness was also directly associated with lower children employment rates in addition to its indirect effect through income. We do not know the exact explanation for the direct effect, but the possibility that trade raises returns to schooling is one candidate. The finding of a significant positive direct trade effect undermines the hypothesis that free trade reduces the return to schooling for children. It also signals an important policy implication: Using trade sanctions to induce countries to reduce child labor is counterproductive.

Countries with more open trade policies had superior civil liberties, and civil liberties improved more rapidly in countries that adopted open trade policies, ceteris paribus. There was no significant relationship between civil liberties and trade volumes, however. Open economies had neither more nor less forced labor than closed economies after controlling for level of development, institutional structure, and the possibility of reverse causation. In short, openness reduces forced labor indirectly by increasing per capita income.

This earlier study indicates that with few exceptions the dominant trade influence on labor conditions is indirect, through its effect in raising GDP. It also confirms that trade is not generally associated with poorer working conditions or labor rights. However, that study does not target specific regions and, in particular, does not explore allegations that free trade degrades labor conditions in Asian countries. We now turn to this question.

C. Trade and Labor Conditions in the Early 21st Century

The debate over the effect of international economic integration on labor conditions has continued into the 21st century, with particular interest in conditions in Asian countries. Both economic growth and trade expansion proceeded apace until the end of the century's first decade. Between 1995 and 2008, the average

growth of (PPP-adjusted) per-capita GDP was similar—about 5.5% for both Asian and non-Asian countries. When weighted by labor force size, however, growth was more rapid in Asian countries (8.7%) than in non-Asian countries (4.8%), reflecting in part very rapid growth in the People's Republic of China (PRC) and India. The trade share of GDP grew much more rapidly in Asian countries. Within each set of countries, trade share growth was more rapid in smaller countries. These regional differences alone imply more rapid advancement of labor conditions in Asia than in the rest of the world.

To assess the links between trade and labor conditions, we estimate the following cross-country regression model using a database of 58 countries at varying stages of development for each labor condition in 2005.[7]

$$LABOR\ CONDITION_i = a_0 + a_1\ ln\ GDPCAP_i + a_2\ TRADE_i + a_3\ ASIA + e_i$$

The independent variables are the natural logarithm of per capita (PPP-adjusted) GDP and the TRADE share of GDP in each country i and a dummy variable for Asian economies.[8] Although theoretical considerations imply a link from trade to labor conditions, we must also consider the possibility that a country's labor conditions could influence its volume of trade as alleged by some critics of globalization. Given this potential endogeneity, we provide instrumental variables estimates of the effect of trade on labor conditions.[9] If the coefficient, a_2, lacks statistical significance, trade has solely an indirect effect on the labor condition through its (unobserved) effect on per-capita GDP. Where a_2 is statistically significant, greater international economic integration has both direct (a_2) and indirect effects on the labor condition. Table 3 provides the coefficient estimates and robust standard errors, weighted by each country's labor force size.

The estimates first confirm the powerful effect of per-capita GDP growth in improving working conditions, but per-capita GDP is not significant in the civil liberties regression—a result that changes in the unweighted regressions discussed in the next paragraph. These estimates also indicate that the trade expansion of the early 21st century had only indirect effects (i.e., via increased per-capita GDP) on labor conditions. The fact that estimates of a_2, the direct effect of trade, are not statistically significant indicates that the net effect of the trade expansion on labor conditions is positive and results from the GDP-enhancing effects of increasing trade. Neither the direct nor the indirect effects of trade diminish labor conditions. The results for the *ASIA* dummy variable are not significant: After adjusting for

[7] Unreported cross-country estimates for years 2000 and 2008 produced similar qualitative results.

[8] The index of open versus closed trade policies developed by Sachs and Warner (1995) is not available for the 21st century. In some regressions, we used the KOF index of global flows described in footnote 1 instead of TRADE, but these experiments produced no material changes in the results.

[9] The variables used to instrument the trade share variable, as suggested by gravity models of trade, are dummy variables for small countries, island countries, and landlocked countries and the land to labor ratio.

Table 3. **Trade and Labor Conditions, 2005**

	ln Per Capita GDP	Trade Share of GDP	Asia	R^2	Countries
Working Conditions					
Hourly Pay (ln)	1.36	−0.00015	0.08	0.94	48
	(0.11)*	(0.0035)	(0.26)		
Annual Work Hours	−152.05	0.85	−13.96	0.51	55
	(57.44)*	(1.29)	(107.23)		
Labor Rights					
Freedom of Association	−0.46	−0.01	1.94	0.40	56
	(0.63)	(0.02)	(1.28)		

Notes: Instrumental variables estimates; labor force weights; robust standard errors.
*p-value < .01.
**p-value < .05.
Source: Authors' computations.

international differences in per-capita GDP and trade shares, labor conditions were no different in Asia and the rest of the world midway through the first decade of the 21st century.

We encountered two notable differences when we recomputed the regressions without labor force weights. First, the coefficient on *ASIA* was significantly positive in both the hours and civil liberties regressions. Ceteris paribus, Asian countries had longer work hours and fewer civil liberties, but only when each country's data were equally weighted. Second, higher trade shares were associated with lower pay. Apparently, these effects are concentrated in smaller Asian countries.

We also conducted panel data analyses of the relationship between national labor conditions, per-capita GDP, and several measures of globalization for 1995–2008. The globalization measures include the trade share of GDP and the KOF indexes of global flows and trade restrictions described in footnote 1. The direction of causation between labor conditions and globalization remains a central concern, but the instruments used in the cross-section estimation lack the time variation required to serve as appropriate instruments in the analysis of panel data. Instead, we use lagged values of the globalization measures as instruments.

We estimate random effects models in which labor conditions are a function of per-capita GDP, instrumented measures of globalization, and a dummy variable for the Asia region to determine whether labor conditions in the region vary significantly from what one would expect based on economic fundamentals.[10] The panel instrumental variables analyses confirm the importance of per capita income in improving labor conditions, but yield no statistically significant findings of direct influence from any of the globalization indexes.[11] Whether measured by

[10] The following Asian economies are in the database: the PRC; Hong Kong, China; India; Indonesia; Japan; the Republic of Korea; Malaysia; the Philippines; Singapore; Taipei,China; and Thailand.

[11] Unreported regressions using lagged globalization measures rather than using the lagged values as instruments also found no statistically significant links with the measures of labor conditions.

Table 4. **Trade and Labor Conditions, 1995–2008**

	ln Per Capita GDP	Trade Share of GDP	Asia	R^2	Obs.
Working Conditions					
Hourly Pay (ln)	1.183	−0.003	−0.337	0.82	523
	(0.080)*	(0.004)	(0.210)		
Annual Work Hours	−0.029	0.0001	0.112	0.43	594
	(0.012)**	(0.0008)	(.037)*		
Labor Rights					
Freedom of Association	−0.905	0.004	0.692	0.51	646
	(0.070)*	(0.003)	(0.178)*		

Notes: Instrumental variables estimates.
*p-value < .01.
**p-value < .05.
Source: Authors' computations.

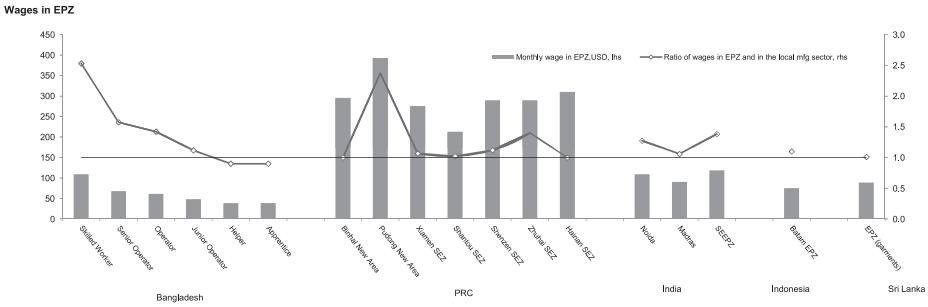
the trade share of GDP (see Table 4) or the (unreported) KOF indexes of global economic flows or restrictive trade policies, there is no significant direct linkage between globalization and labor conditions in the early 21st century. Instead, globalization has an *indirect* influence on labor conditions through its positive effect on GDP. There is another parallel with the earlier unweighted cross-section analyses: After controlling for the influence of per-capita GDP and trade or global flows, the Asia region had significantly higher work hours, fewer civil liberties, and lower pay. When each country's observations receive equal weight, regional differences in growth and trade do not fully account for the regional differences in labor conditions.[12]

Challenges to liberalizing trade policies sometimes single out labor conditions in export processing zones (EPZs) in Asia and other countries for special criticism. During the late 20th century, such zones spread to 130 countries and played a particularly prominent role in the export-led growth in Asia. The zones produce a disproportionately large share of a nation's exports but account for a small share of total employment.

While the national data used in the foregoing estimations should include information from export processing zones (EPZs), we have computed wage differentials between EPZ and non-EPZ workers using data gathered by the International Labour Organization (ILO) in several Asian economies (Figure 1). As late as the 1980s, there was some evidence of relatively low EPZ wages in the Republic of Korea and Malaysia (Oh 1993, Kusago and Tzannatos 1998). By the late 20th century, however, overall average wages in EPZs equaled or exceeded wages outside the zones after accounting for worker characteristics (Robertson et al. 2009), despite the fact that the right to organize unions remains restricted in EPZs in many countries.

[12] Weighted estimation was not available for random effects analysis.

Figure 1. **Wages in Export Processing Zones**



EPZ = export processing zone, SEZ = special economic zone, SEEPZ = Santacruz Electronics Export Processing Zone.

Sources: ILO, CEIC, http://cressence.org, http://understand-china.com, www.icrier.org, www.bbc.co.uk, www.wsws.org, Aggarwal, Aradhna (2007). Wage Board for Garments and Manufacturing Trade (Sri Lanka); respective central banks (exch rates).

Source: Authors' compilation.

IV. Foreign Investment and Labor Conditions

With the relaxation of many capital controls, a significant increase in investment flows between countries accompanied the late 20th to early 21st century globalization. As with international trade, much of the growth regained ground lost during the retreat from the late 19th century globalization. A parallel growth of MNCs accompanied the resurgence of foreign direct investment (FDI).

These developments raise two sets of questions about the relationship between foreign investment and labor conditions. First, do labor conditions influence FDI inflows? Do cheap labor, poor labor conditions, and weak support of labor rights attract FDI? And if labor conditions influence FDI flows, how important is their influence relative to other influences, such as market size and investment risks? These issues are addressed in an econometric analysis of the determinants of FDI flows.

Second, irrespective of what attracts FDI to a host country, how do the human resource management policies of MNCs influence host country labor conditions? Allegations that foreign investment degrades host-country labor conditions often rest on examples of appalling labor conditions at some Asian workplaces. But are these anecdotes typical? In particular, do MNCs on balance degrade or improve labor conditions in host countries? This question is best addressed by micro studies comparing the working conditions at MNCs with comparable host-country firms. The rest of this section examines evidence on each of these issues.

Table 5. **Foreign Direct Investment Inflows, 1990–2009**

	Share of World FDI Inflows, %		
	1990–1992	**1999–2001**	**2007–2009**
World	100.0	100.0	100.0
Developed Countries	75.3	78.1	60.7
Developing Countries	24.3	21.1	33.6
East Asia	6.3	8.2	9.9
South Asia	0.3	0.5	2.5
Southeast Asia	7.4	2.2	3.2

FDI = foreign direct investment.
Source: UNCTAD, World Investment Report 2010, Annex Table 1.

A. Labor Conditions and FDI Flows

The 19th-century globalization included significant international capital flows, but most FDI flowed from capital-rich European countries to less-developed countries, where capital was scarce and its marginal value was accordingly high. Following the interwar retreat from global economic activity, international capital flows regained their earlier peaks during the 1990s, but with a distinctive change in the destination of investments. Most capital no longer flows toward the least developed nations where capital is scarce. Capital-poor developing countries received less than a quarter of world FDI flows during the late 20th century (Table 6). Instead, "capital transactions seem to be mostly a rich–rich affair, a process of 'diversification finance' rather than 'development finance'" (Obstfeld and Taylor 2003, p. 175). Only in the early 21st century did the share flowing to developing countries begin to increase, although it had reached only a third of FDI inflows by the end of the century's first decade. The entire continent of Africa received less than 4% of world inflows in 2007–2009—little different from the 1990s. While the volume of FDI received by Asian countries increased, their *share* of FDI inflows changed little over the past 20 years and their share of the flows to developing countries declined (Table 5).

Even this snapshot of FDI flows undermines the notion that countries with inferior labor conditions attract international investment flows. With most FDI now flowing between industrialized nations, which offer superior labor conditions, efforts to find cheap labor and weak labor standards cannot be the primary factor motivating the international distribution of FDI.

We have explored this implication more formally in a panel data analysis of the distribution of world FDI inflow shares across countries between 2003 and 2009, a time period governed by the availability of some key variables. Our strategy is to estimate a baseline model and then to see if the explanatory power of the model improves with the addition of measures of labor conditions. The baseline analysis assumes that investors seek to maximize their expected return and tests the hypotheses that these returns depend on market size, investment risks, the availability of

complementary inputs, and a country's openness to international trade. In measuring market size, we capture both the number of potential consumers (population) and their income (per-capita GDP).

Our preferred measure of investment risk is a Euro money country creditworthiness scale reported in the *World Competitiveness Yearbook*. Higher scale values imply lower risk and hence higher FDI shares. We also tested for the influence of several institutional and regulatory factors that might influence the cost of doing business in a country. The ratio of government consumption expenditure to GDP is frequently used in growth studies as a proxy for the degree of government intervention in the economy, but this variable was not statistically significant in our analyses. Other variables were based on *World Competitiveness Yearbook* survey responses of business executives' perceptions of the regulatory environment, personal security, the protection of property, and bribery and corruption. Two of these measures—survey responses to the statements "Bribing and corruption do not exist" (Bribe) and "Labor regulations do not hinder business activities" (Labor Regulation)—were significantly related to a country's share of world FDI inflows in some regressions. Each of these variables is measured on a 0 to 10 scale with higher values indicating stronger agreement with the statements.

The regressions also tested for complementarity between FDI and land (the area of a country in millions of square kilometers) and with the skill of the labor force. Skill is measured variously by the percent of the population achieving at least tertiary education, executive survey responses indicating whether "skilled labor is readily available," and (inversely) by the percent of the population over 15 years old that is illiterate. The trade share of GDP (lagged 1 year) tests for the effects of international economic integration on a country's FDI inflow share.

The analysis finds that countries with large markets, low investment risks, and a large trade share of GDP attract larger shares of FDI inflows (Table 6, regression 1). FDI and land appear to be complements. We found no significant correlation between any of the measures of labor skill and FDI inflow shares. At least in the early 21st century, there was no evidence that FDI shares increased in countries with abundant unskilled labor, ceteris paribus. The overall regression fit is good, with the model accounting for more than 60% of the variance in FDI inflow shares among 55 countries between 2003 and 2009.

The baseline model highlights factors that would tend to reduce FDI shares in Asian countries as well as factors that would tend to raise them. Relative to the rest of the world, Asian countries on average have lower per-capita GDP, higher investment risks, and more concerns about bribery. On the other hand, average population size and trade share are larger in Asia. Nonetheless, the baseline model does not capture all the factors producing relatively lower FDI inflow shares in Asia.

We made a preliminary assessment of the effect of national labor regulations on FDI by adding a labor regulation variable to the baseline specification (Table 6, regression 2). The coefficient was significantly positive, meaning countries in which

Table 6. **FDI Share Regressions, 2003–2009**

	1	2	3	4	5	6	7	8
GDP per capita	0.412	0.395	0.295	0.324	0.308	0.349	0.834	0.272
	(0.186)**	(0.186)**	(0.174)*	(0.176)*	(0.177)*	(0.184)*	(0.294)***	(0.27)
Population	0.675	0.673	0.966	0.953	0.958	0.947	1.015	0.895
	(0.090)***	(0.090)***	(0.095)***	(0.095)***	(0.097)***	(0.098)***	(0.105)***	(0.101)***
Land area	0.039	0.048	−0.099	−0.106	−0.107	−0.1	−0.115	−0.044
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Country credit-worthiness	0.02	0.021	0.019	0.017	0.015	0.014	0.009	0.026
	(0.009)**	(0.009)**	(0.008)**	(0.009)**	(0.01)	(0.01)	(0.01)	(0.013)**
No bribe/corruption	0.085	0.066	0.095	0.098	0.099	0.084	0.141	0.029
	(0.049)*	(0.05)	(0.046)**	(0.046)**	(0.047)**	(0.049)*	(0.057)**	(0.06)
Trade shares (lagged)	0.382	0.374	0.517	0.509	0.503	0.506	0.512	0.763
	(0.128)***	(0.128)***	(0.124)***	(0.124)***	(0.124)***	(0.125)***	(0.144)***	(0.158)***
Friendly labor regulations		0.066	0.116	0.109	0.115	0.11	0.107	0.088
		(0.05)	(0.044)***	(0.045)**	(0.046)**	(0.046)**	(0.057)*	(0.051)*
ASIA = 1			−1.275	−1.305	−1.244	−1.232	−1.542	−1.401
			(0.274)***	(0.275)***	(0.285)***	(0.288)***	(0.345)***	(0.383)***
PRC = 1				0.68	0.906	1.013	1.073	1.128
				(0.64)	(0.68)	(0.70)	(0.76)	(0.74)
Civil liberty index					−0.085	−0.09	−0.074	−0.062
					(0.08)	(0.09)	(0.11)	(0.11)
Skilled labor availability						0.043	0.032	0.085
						(0.05)	(0.06)	(0.06)
Manufacturing wage							−0.331	
							(0.197)*	
Annual hours							(0.14)	
							(1.13)	
Real effective exchange rates								−0.046
								(0.57)
Constant	−10.08	−10.32	−8.876	−8.892	−8.384	−8.97	−11.761	−10.291
	(1.916)***	(1.926)***	(1.814)***	(1.815)***	(1.878)***	(2.003)***	(8.983)**	(3.447)***
Overall R2	0.595	0.599	0.678	0.682	0.681	0.684	0.714	0.698
Observations	350	350	350	350	350	350	257	264
Number of countries	55	55	55	55	55	55	50	41

Notes: robust standard errors in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.
Source: Authors' computations.

business executives believe labor regulations do not hinder economic activity receive a larger share of world FDI, other influences being equal. Unlike other measures of labor regulation by country, this variable is available for several years, but it provides no indication of which labor regulations concern potential foreign investors the most.

Do geography and location matter? There is a widespread notion that the Asian region is especially attractive to FDI given the predominance of the export-led growth model in the region. However, when a dummy variable for Asian countries is added to the baseline regression model, the result is significantly negative and the statistical properties of the regression improve (Table 6, regression 3). Even after holding the effects of the independent variables constant, the Asian region receives a comparatively smaller share of world FDI inflows.

We also investigate whether the PRC receives a disproportionate share of FDI, and our results again do not suggest that this is the case (Table 6, regression 4). In fact, the evidence in the subsequent robustness checks suggests that the significant flow of FDI into the PRC can be explained by the list of independent variables included in the baseline model.

Our central interest in the FDI analysis is to assess claims that FDI is attracted to countries with poor labor conditions. We test for the influence of a country's labor conditions on FDI by adding measures of working conditions (such as average manufacturing wages, annual hours worked, and civil liberty measures) to the baseline econometric model (Table 6, regressions 5–7). With one exception (the marginally significant negative coefficient on wages), high FDI shares are *not* significantly correlated with poor labor conditions in these regressions. Notably, adding information on labor conditions to the analysis does not alter the earlier findings for either the Asian region or the PRC. Lastly, we investigate the effects of real effective exchange rates on FDI shares, and the result does not suggest that "cheaper" prices significantly determine FDI flows to these countries.

Overall, our findings show that FDI patterns between 2002 and 2009 were still significantly driven by factors that have been highlighted by previous literature surveys (e.g., Blonigen and Piger 2011). On one hand, "traditional" FDI determinants such as GDP per capita, population of host country, and the risk factors of host countries play an important role. On the other hand, institutional factors such as freedom from onerous and predatory bribery or corruption, as well as market-friendly labor regulations (which should not be interpreted as the equivalent of a complete lack of regulation), seem to exert more impact on FDI decisions rather than poor labor conditions alone.

The evidence on patterns and determinants of FDI inflows has a bearing on two views of why companies locate production abroad in the first place. One view holds that foreign investment is attractive when it offers specific location advantages such as mineral deposits or cheap labor. This view apparently underlies assertions that poor labor conditions attract foreign investment. An alternative view holds that MNCs transfer important productive inputs that host countries lack—unique

technology, managerial skills, and superior knowledge of organizational design and production methods (Hymer 1960, Caves 1996, Markusen 2002). MNCs need such firm-specific "knowledge capital" if they are to overcome their lack of familiarity with local regulations, marketing practices, human resource management policies, and other aspects of management that are sensitive to differences in local cultures. Under the knowledge-capital view, the possession of firm-specific assets that can profitably be combined with local inputs in host countries drives foreign investment—not an effort to exploit local inputs.

The fact that neither broad patterns of FDI inflows nor statistical analyses of the determinants of those inflows reveal evidence of significant links between foreign investment and labor conditions supports the "knowledge capital" hypothesis over the "location advantage" hypothesis of investment motivation. The difference in these views is also important for understanding the impact of MNCs on host-country labor conditions—the topic of the next section. Combining such firm-specific assets with local inputs should raise, not lower, the productivity of host-country affiliates. In short, the knowledge capital scenario explains why MNCs might offer higher wages than their host-country competitors.

Why are the results of the analysis of FDI inflows so inconsistent with the location-advantage hypothesis? Poor labor conditions signal low productivity as well as low wages, and not all investments thrive in a low-productivity environment. Moreover, countries with poor labor conditions tend to be countries in which direct risks to investment are high. Risks of expropriation and repudiation of contracts are highest in countries with few civil liberties, for example. These risks effectively counter whatever advantages cheap labor might provide.

B. MNCs and Labor Conditions

The impact of a multinational company on working conditions in a host country depends on the extent to which it must compete with other MNCs or host country firms for its workers and on the local elasticity of labor supply. If multinationals establish inferior conditions in newly-constructed plants, they will face recruiting and retention difficulties when competing with other firms for labor. If they instead acquire local companies and try to worsen working conditions, they will encounter increased quit rates as workers leave to join host-country firms offering superior conditions.

Whether the arrival of MNCs can improve working conditions depends on labor supply conditions in the host country and the human resource management policies of the firm. As noted earlier in the paper, in markets with a limitless supply of labor available at the current wage, increased labor demand from MNCs or host-country firms will raise employment, but not wages. When workers require inducements to overcome the costs of changing employers, however, labor supply is less elastic, and increases in labor demand from MNCs will raise both wages

and employment. When firms compete for labor, the effect of increased demand on wages depends on what workers are willing to accept—not on what firms may wish to pay.

If MNCs do not compete with other firms for labor services, they may force labor conditions below competitive levels. Firms in isolated locations may have such "monopsony" power, but situations in which labor has no choice of employers seem too rare to accept monopsony as a general phenomenon. Indeed, by adding to the number of employers in a labor market, the arrival of MNCs should improve labor conditions by reducing monopsony power in host-country labor markets.

The conclusions of the research community on the impact of MNCs on wages are nicely summarized in the following statement (Lipsey and Sjöholm 2001): "It seems to be a universal rule that, in every country, foreign-owned firms and plants pay higher wages, on average, than domestically owned ones. That is true not only in developing countries, but also in high income countries, such as Canada, the United States, and the United Kingdom." The persistence of higher pay in MNCs implies that labor productivity in foreign affiliates exceeds productivity in host-country firms. Comparisons of value-added per employee confirm this implication. According to United Nations data for the mid-1990s, foreign-affiliate productivity exceeded domestic firm productivity by 37% (Hong Kong, China), 65% (Malaysia), 137% (PRC), and 373% (Taipei,China). Significant but smaller productivity premiums for foreign affiliates were recorded in most major European and North American countries (UNCTAD 2002).

Some of the superior productivity and pay of foreign affiliates reflects differences in industry and firm size. In comparison to host-country firms, foreign affiliates also hire employees with more observable and unobservable skills (Malchow-Møller, Markusen, and Schjerning 2013). Wages also grow more rapidly in foreign-owned firms, suggesting that they may provide more specific training or other on-the-job learning opportunities than host-country firms (Table 7). Even after controlling for these factors, however, studies still find foreign-affiliate paying premiums (in the order of about 3%–5%). These premiums may reflect differences in management quality between foreign and domestic firms.

V. Concluding Comments

During the late 20th and early 21st century, a broad improvement in working conditions and labor rights around the world accompanied a significant expansion of international trade and investment. The analyses reported in this paper clarify the ties between these two developments. Trade itself advances working conditions and labor rights to the extent that increased trade flows raise per capita income. We find no separate direct influence of trade on labor conditions, however. The linkage between increased trade and improved labor conditions is consistent with

Table 7. **Ratio of Compensation in MNCs and Local Manufacturing**

	2000	2008	Avg. Annual % Change
Bangladesh	21.29	19.28	−1.2
Brunei Darussalam	8.48[a]	15.23	11.4
China	7.24	3.10	−7.1
Hong Kong, China	2.24	2.27[b]	0.2
India	11.63	11.43[b]	−0.2
Indonesia	20.58	13.56	−4.3
Japan (1)	1.36	1.14	−2.0
Korea, Republic of (1)	2.01	1.73	−1.7
Malaysia	2.66[a]	2.29	−3.5
New Zealand (1)	1.24	1.08	−1.6
Philippines (1)	4.99	3.22	−4.4
Singapore (1)	1.46	1.27	−1.6
Sri Lanka	21.25	11.39	−5.8
Taipei,China (1)	1.73	1.69	−0.2
Thailand	4.02	4.05	0.1
Viet Nam	3.62	6.87	11.2

MNC = multinational company
MNC wage def: Average monthly compensation per worker paid by the foreign affiliates of the US MNCs per year
1-Manufacturing wage data – Average direct pay per employee (BLS); or the other countries – data were obtained from ILO-Laborsta or CEIC.
a-2000 data: Brunei Darussalam-2001, Malaysia-2004.
b-2008 data: Hong Kong, China-2007; India-2007.
Sources: BEA, BLS, ILO, and CEIC.

the general predictions of international trade theories, but does not support claims that increased international competition will degrade working conditions and labor rights. Moreover, we do not find evidence that countries with poor labor conditions acquire larger trade shares, ceteris paribus. Concerns that reducing trade barriers will degrade labor conditions in a developing country are not supported by our analyses.

Instead, trade barriers can unintentionally undermine some labor rights. Consider the effect of applying trade sanctions against countries using child labor in the production process. Effective trade sanctions will reduce per-capita GDP, national income, and employment. Reductions in adult employment create pressures for increased child labor force participation in order to preserve family income. The goal of reducing the incidence of working children is better served by expanding, not contracting, employment opportunities for adults.

Although examples drawn from Asian countries are often used to support general claims that globalization degrades working conditions, we find no special "Asia" effect on labor conditions in our analyses when each country's data are weighted by the size of its labor force. That is, differences in per-capita GDP and trade shares fully account for differences in labor conditions between Asian and non-Asian countries. Analyses with unweighted data find that Asian countries have relatively high work hours and low freedom-of-association rights after controlling

for GDP and trade influences. In short, labor conditions lag most in the smallest Asian countries.

Our analyses also do not find evidence that countries with poor labor conditions attract disproportionate shares of FDI. Market size and investment risk are the dominant influences on FDI. After accounting for their influence, actual labor conditions play a negligible role in the destination of FDI inflows. Perceptions of the constraints imposed by national labor regulations can influence a country's FDI share, however. After accounting for the influence of these factors, the Asian region receives a smaller share of world FDI inflows than other regions. Detection of the factors driving this Asia effect is an important topic for future research.

Finally, our review of the growing literature on the impact of MNCs on host-country labor markets finds no evidence that multinationals depress wages. Instead, the evidence seems consistent with the "knowledge capital hypothesis" that foreign firms bring firm-specific technical and managerial advantages that produce the higher productivity that supports higher wages.

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Appendix: Sources and Concepts

This paper considers three dimensions of working conditions—pay, hours of work, and job safety—and four dimensions of labor rights—child labor, employment discrimination, freedom of association, and forced labor. For the data sources for analyses of the late 20th century, see Flanagan (2006, Appendix A). Analyses for the first decade of the 21st century use the database in the *World Competitiveness Yearbook*, downloaded from the IMD website. This database includes data acquired from international organizations and national governments as well as special survey data acquired by IMD from cooperating research institutes around the world. Annex IV of the *World Competitiveness Yearbook* provides a complete guide to all sources. To this database, we added variables provided by the ILO. This Appendix notes conceptual differences in data for the late 20th and early 21st century.

We use the *annual* compensation per worker in manufacturing to measure pay in the late 20th century (UNIDO 2002). This measure includes direct wages plus contributions by employers to social security programs. For the early 21st century, pay consists of *hourly* earnings per worker in manufacturing. Three measures of work hours are analyzed in the late 20th century: (i) the proportion of employees who usually work more than 40 hours a week; (ii) weekly hours of work in manufacturing; and (iii) annual work hours for all employees. The early 21st century analysis uses the last measure.

In contrast to data on pay and work hours, there is no general measure of job safety available for a large sample of countries. This paper uses the rate of *fatal* on-the-job injuries per 100,000 employees, available from the ILO. (Consistent data on nonfatal accidents are very scarce.) We have adjusted the ILO data to a common base (100,000 employees), but given the wide variation in reporting practices, changes over time within a country are likely to be more informative than cross-country comparisons.

Indicators of labor rights now exist for a substantial cross-section of countries, but measures for only two of the four core labor rights—workplace freedom of association and child labor force participation—are available for multiple years. For child labor force participation, we use data provided by the ILO (2010), which defines children's participation in work employment as child employment, and assigning a narrower definition to the term child labor, as defined by the ILO Minimum Age Convention, 1973 (No.138) and ILO Worst Forms of Child Labor Convention, 1999 (No. 182). Thus, *child labor* is a subset of the broader category *child employment* (child labor force participation). Nonetheless, there is a large overlap between the two categories: in 2008, 86.8% of all working children were also categorized under child labor. The data are estimated based on 60 national household surveys carried out in 50 countries, covering the period from 2004 to 2008.

Most of our analyses of freedom of association rights use a broad measure of civil liberties developed by Freedom House (http://www.freedomhouse.org/). The

Freedom House index evaluates actual national practices rather than constitutional guarantees and ranges from 1 to 7 with the *lowest* scores indicating the strongest liberties. A recent study (Freedom House 2010) permits an evaluation of how well the general civil liberties index captures workers' freedom of association. For 2010 only, Freedom House developed a five-point measure of worker rights for each country, with the *highest* scores indicating the strongest rights. The cross-country correlation between that index and the general civil liberty index in 2010 is –0.91 (where the negative sign reflects the different scaling of the two measures). For the mid-1990s only, there is an index of workplace freedom of association and collective bargaining rights (Kucera 2002). The index, based on an evaluation of 37 potential interferences with rights to form unions and bargain collectively, ranges from 0 to 10 with low numbers reflecting superior workplace freedom of association rights.

We measure labor market discrimination as the percentage difference between male and female wages that remains after adjustments for gender differences in schooling, experience, and other performance-related variables. The focus on gender provides a benchmark for discrimination that is widely applicable across countries. The data come from a meta-analysis of 263 published papers offering 788 estimates of gender pay differentials in various years from the 1960s through the 1990s in 63 countries (Weichselbaumer and Winter-Ebmer 2003). The meta-analysis generated estimates of *net* gender wage differences for each of the countries, after controlling for year and characteristics of the study. The estimated country effects constitute the measures of discrimination used in this study. Only one observation per country is available—dated here as 1985, about the middle of the period covered by the studies in the meta-analysis.

We rely on two approaches to measuring the prevalence of forced labor. The first approach estimates the number of forced laborers. One study estimates 27 million forced laborers worldwide in the late 1990s and provides tentative country-by-country estimates with many caveats (Bales 2004). We use the midpoint of this estimated range for each country. The ILO later published a much lower estimate of 12.3 million victims of forced labor worldwide based on double-sampling of reports between 1995 and 2004 (ILO 2005). The report stated reasons why this figure might be an underestimate and did not report estimates by country. The second approach counts the varieties of forced labor found in a country, as indicated in qualitative reports by the US Department of State and human rights organizations. Ranging from 0 to 8, this variable is available only for the late 1990s (Busse and Braun 2003).

ADB's Distinguished Speakers Program Measuring the Connectedness of the Financial System: Implications for Risk Management

Robert C. Merton*

Well thank you very much. I greatly appreciate the opportunity to speak to you today and for your taking the time to do so.

As an introduction, the topic for today is looking at connectedness in the financial system, and I'll talk about a specific way that connectedness is defined in this context, and we'll talk about developing a new and better way to measure the degree of connectedness in the institution in a useful way, with respect to credit risk in particular, and how we might look at all that information and convey it in a fashion that we might be able to monitor, or at least get better information or insights, into potential systemic events.

Macro financial risk propagation is a big issue for governments and financial stability, but it also is important in the private sector, particularly for very large asset managers. Managers that have very large asset pools, are too large to actually get out of harm's way, and so like the rest of us, they have to be prepared to deal with large market shocks—instead of simply trying to get away from them.

The crisis of 2008 and 2009 was centered on credit risk involving money markets as well, but it was essentially a credit risk issue. The ongoing European debt crisis is, at least in my mind, not yet fully resolved—that also is an issue of credit. So the substantive issue for my remarks is going to be on credit. What I want to deal with is to look at how the propagation of credit risk among financial institutions and sovereigns is related to how connected they are, and in the process develop tools for measuring the connectedness and its dynamic changes. As you'll see from the numbers and the pictures, mostly pictures, the degree of connectedness among institutions and sovereigns is not constant or even approximately so through time. It changes quite substantially and dynamically. A last point, this is work that I've done with five other co-authors; they are not to be held accountable for my bringing it here. The scientific paper is not yet finished. There was an earlier paper describing it but I'm bringing it to you because I, as well as my co-authors, believe

*Robert C. Merton is School of Management Distinguished Professor of Finance at the MIT Sloan School of Management. He received the Nobel Memorial Prize in Economic Sciences in 1997. This lecture is based on the work of Billio et al. (2012) and Merton et al. (2013).

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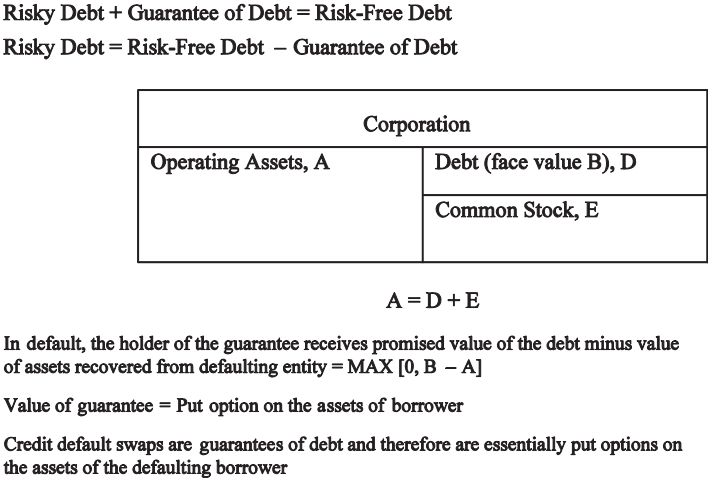
the technique, the tool used here, will prove to be well-founded. The numbers may change with the data when we finish refining it, but the principles I don't think will. So I stake my reputation that this approach is worth looking at, even though the scientific paper to support it is not complete.

What I'm going to show you mostly are lots of very colorful pictures. So it's going to be a picture show, and in past people have liked it. But as Eugene Fama who just received a Nobel Prize is fond of pointing out, as did the late Paul Samuelson in a similar work, there's no free lunch; and there's no free lunch for you in this talk. So if you want to get to the pretty pictures you're going to have to pay a price (by going back to school). I've already said to you we're going to be looking at the connectedness of credit risk. If we're going to do that you've got to know about credit. So in the next 14 minutes, I'm going to make you credit risk experts. Those of you who want after the talk to be certified as level 1 credit risk experts, see me, and I'll sign your program. So are you ready to go back to school? We're going to learn first how to become credit experts, and then you'll get your reward.

Let's get started. Now, as I talk about lending, I'm going to invariably use United States (US) dollars which is provincial but you can make the translation. If I start to try doing it and multiplying by 45 which is current currency rate, I'll surely get it wrong. So, what I'm holding here in my hand is a mortgage on a house or residence, a corporate bond, a bank loan, not a 16 or 22 tranche structured product, a vanilla mortgage bank loan or corporate bond, in dollars. If I now staple to this, a full faith and credit guarantee of the payments on this, whatever it is, loan, mortgage, or so forth, from the US government, what is this combined instrument functionally? It's a risk-free asset in dollars. Why? Because if the issuer of this bond, mortgage, or loan doesn't pay, the US government will. By the way, this isn't hypothetical: the US government has for a long time and in recent times in great quantity issued quite a number of such guarantees so you should be familiar with it. Everybody, just move your head up and down, if you agree this is a risk-free asset. Now watch. I just ripped the guarantee off, we're back to where we started. So if the original was risk-free, what's this? Risk-free minus a guarantee, what I just ripped off, you see the logic of that. This is shown in Figure 1: risky debt is risk-free debt minus the guarantee of debt.

Now, the first lesson to draw from this is the following: every time we issue these kinds of loans, whatever they are, to whomever buys those loans or holds those loans, they will actually be engaging in two very different kinds of investing or risk-taking. They're lending money risk-free, which is very well defined. It's the time value of money—I give money now, and I get money with some interest, not much lately, for sure, we all understand risk-free lending. But they're also doing what else? They're writing a guarantee, that's what a minus guarantee means. It means instead of owning one, they're issuing one. And what is a guarantee? Functionally it's insurance. I guarantee the value, I guarantee an asset, or I guarantee something, that's insurance. So every time the investor pursues debt, he has two activities: risk-free

Figure 1. **Functional Description of Being a Lender or Guarantor of Debt When There Is Risk of Default**

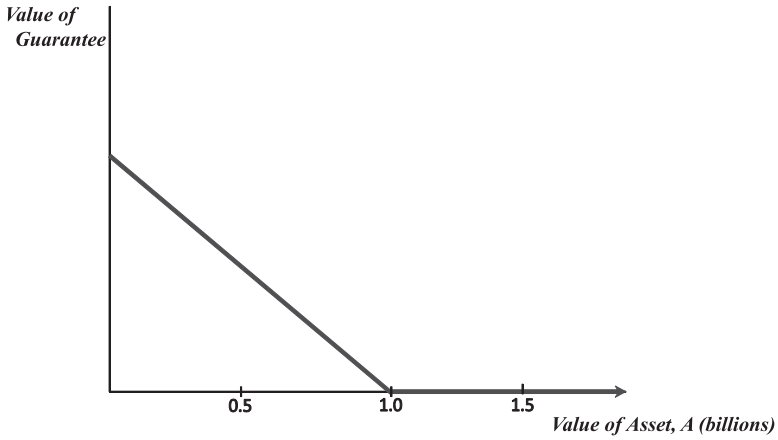


lending and insurance writing. Now insurance writing is an old and honorable activity. So there is nothing wrong with it. But it is very important to know that's what you're in the business of doing. So functionally that's what it is. That's the first lesson. Debt buyers are engaging in two very different financial activities.

Let's focus on the guarantee, and try to understand a little bit more about it. And so what I am going to propose for you is a very simple example. From Figure 1, we have a corporation that has assets with value A, it has debt, and it has common stock with value E—the simplest case. Assume all the debt is the same, and let's make it zero-coupon, and what it basically says on the maturity day of the debt, the firm or the corporation promises to pay B dollars, say, or a billion dollars. It's very simple. If you don't want to use a corporation as an example, replace it in the box title with Household, on the left side instead of Operating Assets write House, on the right side instead of Debt write Mortgage and Equity for Common Stock.

Now to figure out what the guarantee is, first we know always the left side and right side of the balance sheet are equal to each other as an identity, both in value and risk, and we can ask the question of what the guarantee is worth. I ask you the question: What happens on the day when the debt comes due? There are actually two possibilities: Possibility one is when you get there you bring your bond, and they pay you what they promised you, your billion dollars. You're happy and you go off, end of story. In that case if the corporation pays, what was the guarantee worth after the fact? Nothing, you didn't need it, so the value is zero. That's very common for insurance isn't it? This is typical to buy insurance and in fact, we even hope that it doesn't have value, because the thing we're insuring against we don't want to have happened. So, if we get paid it's worth zero. But what if we're not paid? What happens when we show up and the corporation says, sorry, we can't pay you? Well

Figure 2. **Value of Guarantee at Maturity**



what happens next in the real world is a pretty complicated process called default, and bankruptcy, and trying to get the money back. I'm not going to take you through all of the nightmares of the legal system.

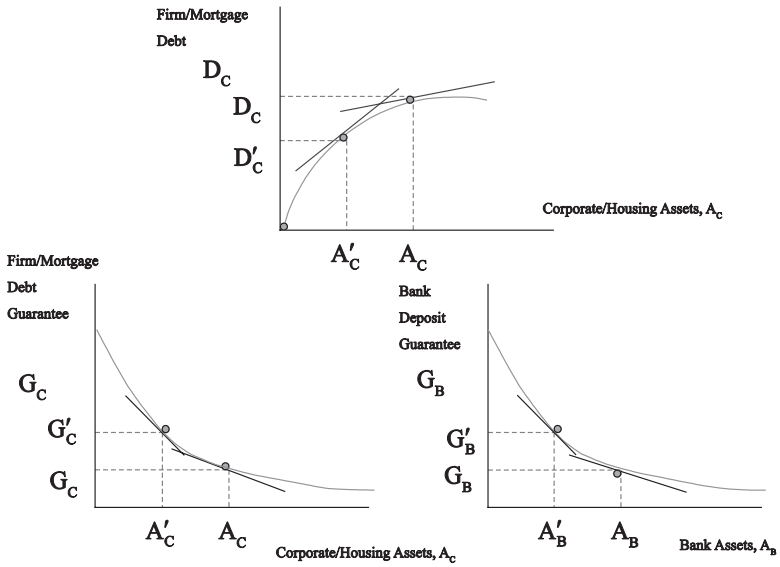
But one thing in common to all debt contracts no matter where they are issued or by whom, is the basic principle that, if the borrower does not pay back what was promised, then the lender gets to seize the assets of the borrower that are behind that. It may be hard to get them, it may be expensive to get them, but at the end of the day, that's what they get. So in the second case, what will the holder of the bond get? They'll get the value of the assets A. We know the assets aren't worth as much as a billion dollars because if they were, they've been better off just to pay us, by either selling the assets or raising new money to do it which they could. So we know A is less than a billion, and A is what we call recovery value. So if there's $700 million, even if we were promised a billion, we get the assets worth $700 million, so that our recovery value is only $700 million. That's basically the outcome.

So, therefore, what is the value of the guarantee as a function of the value of the assets of the borrower? It's equal to either zero or the amount that you would receive with the guarantee which is the $1 billion or B dollars minus what you would have gotten in the recovery which is A. So in the example, the guarantee would be worth a billion minus $700 million or, $300 million. So the simple mathematical statement of what that payoff is, the guarantee at the end is worth the maximum of zero or B the promised payment, minus A, the value of the assets of the borrower, or max(0,B–A) = max(0,1000–700 million).

Does anyone in the audience recognize that payoff? It looks like Figure 2.

Yes, it's a put option, that's exactly a structure of put option. You knew I'd bring options in somehow, those of you who know my background. Put option is insurance, it is value insurance, and the strike price of the put option is the promised payment on the debt, the expiration date of the option is the maturity of the debt,

Figure 3. **Non-Linear Macro Risk Build-up**



and the option is on the assets of the borrower, but it is an exact analogy with the same payoff. Why do I point that out to you other than curiosity? Because for more than 40 years, we have been trading, valuing, estimating, understanding the risk and valuation of options, particularly puts, and calls, so we have a lot of experience and have made many sophisticated models for valuing them.

So, for those of you who never knew anything about credit, but took at least some class sometime in finance, and know what an option is, you now are already well on your way to becoming an expert in credit risk, because everything you know about puts can be used to value the credit, and that's why I show you this. So, for example, you know that the value of put option or call option goes up, when the volatility of the asset increases even if the value doesn't change. The same thing here, if the volatility of the borrower's assets increases, nothing else changes—the asset value doesn't, then the guarantee goes up which means that that value of debt goes down. So you see once you understand put options you have all that knowledge, you can instantly bring all of that empirical and theoretical knowledge to bear in understanding credit. The bottom line is, a bond is nothing more or less than risk-free lending, minus a put option on the borrower's assets, and you're writing a put option, so you're writing value insurance.

Now with your newfound knowledge, let's look at what we can use this to figure out about what happens to debt as conditions change. You understand that risky debt is risk-free minus the put option or minus the guarantee, and the topmost chart in Figure 3 gives a representation of that. The horizontal axis shows the assets of the borrower and the vertical axis the value of the loan, the mortgage, or whatever

and it is, this value curve is concave. This is no one's particular theory, this concave value curve holds in general. All the action in the debt valuation comes from the put option component. We all know what a risk-free bond is, so let's focus on the guarantee, shown in the lower left chart in Figure 3. The guarantee is a put, and a put option will in general have a shape like this one. So you all see that's generic not model specific and what it says is, at a given point in time the higher the assets, the lower the value of the guarantee. If you're insuring and there are a lot of assets there, such that if value of the assets goes down, the value of the insurance goes up.

Let's now look at this work in the context of banks because we want to talk about the banking system and institutions. Banks issue loans, therefore with bank loans, the banks are writing put options on their customers' assets. Let's look at the bank's liability for the guarantee. If you start in A_C, at the time you make the loan and the guarantee is written, there's some value in the guarantee which is G_C. What happens if the assets fall to A'_C? Then, if they did it within a short span of time so you don't have to shift curves, you see the guarantee goes up to G'_C. So what's the next observation? If the assets fall, the debt has to fall in value, even though the borrower has not defaulted on any payment; it just becomes less valuable. And in most real-world cases, bank loans are not marked this way. They only get marked down when some event occurs, but economics says it has to fall. By how much? That's an issue of empirics and analytics, but we know it has to fall.

What would that mean for a bank? Well, if the bank had these loans, if the assets of the borrowers fell, and nothing else changed, bank assets go down. What happens to their capital? It has to go down. What does that mean about risk? Well, the bank is more leveraged now if it changes nothing and so the risk goes up. Most people understand that, if bank capital goes down, then risk goes up, but the part of risk change that's insidious about credit risk, and I think quite *a propos* for understanding at least a portion of what happened in the crisis, is you can see from this fact that these curves in the bottom charts in Figure 3 happen to be convex. What I mean is it holds water, using an analogy from high school math class, versus spills it when it is concave. But look here, when you did the original loan and if you use risk analysis, ask the following question: As a result of doing this loan, and as a result of writing this guarantee, how exposed are we to risk of the assets that support it? And for small movements in the asset price, it moves up a dollar, or down a dollar, what is that risk? It's roughly the slope of the curve in the left, bottom chart in Figure 3, or the tangent to point (A_c,G_c).

So let's say that the tangent is –0.10, it's minus because the slope is downward sloping. What that means is, for a small movement in asset value when you first did the loan the risk exposure to the bank through the guarantee written, is that for each dollar declines in borrower's assets, the loan would lose, for example, ten cents. So it's pretty easy to quantify how much risk, which in our example, is about $0.10 on this loan for a dollar of asset value movement. Moving to A'_c, we already have an

increase in the bank's leverage because of the capital reduction, but what else do you see now? What if I ask the same question for the second round now after value of the asset moves?

The slope is steeper at point (A'_c, G'_c) i.e., it is bigger in absolute value. Instead of being –0.10, the slope is now –0.15, and you see it's getting steeper just by the convexity nature of the curve; this is no specialized theory, it is inherent to the structure. What does that mean? It means for the same loan, nothing has changed except for another decline of the same magnitude that just occurred before. When it occurs a second time, the impact is larger, in this example 50% larger, so instead of losing a dime on every dollar, you lose $0.15. Do you see that if you had a third move down, it gets steeper and steeper? So one of the insidious things about credit (which at some level looks simple but isn't), is that as the asset value moves, the risk of the particular loan changes, and its risk is not linearly changing. So what does that mean? It means when you have this decline, the bank becomes much riskier than simply the decline in bank capital, that's just one part of the increase. Because its asset value went down. You have a second impact that each loan itself is now riskier. Thus, even if the bank replaced its lost capital to keep the same leverage, it is still riskier than before and therefore a second shock will have a bigger effect than simply a reduction in capital ratio would predict. If you haven't seen this before, that's what you want to embed it in your head to remember, because that's the real secret to understanding how risk evolves and how bad things can happen in credit if you don't recognize its convexity.

Now we're going to use that to examine the crisis. This is not to say this is the complete explanation. So you can see for example if you go back when banks were losing billions every quarter back in 2008, 2009, into 2010, what did you hear? Banks lost $5 billion, they've announced they're going to do no more new loans, they're not going to increase their portfolio, they're not going to do anything, and next quarter, similar declines happen, and they lose more than $5 billion. How can they lose more if they have the same assets? The answer you can see is, although it's the same asset by name, that asset is more risky. There are alternative possible explanations for increased losses for the same loan base. They could have cooked the books or whatever. However, what you see here is structural, and thus applies always as at least a part of the driver of credit risk change.

This is no one's particular theory, and if borrower asset declines happen several times, this is how you can get what looks like ever larger losses even though the positions haven't increased. They also give you a hint of how you can get what are called "ten sigma" events. You know these sensational stories we all heard where a journalist would call up someone in Goldman Sachs or Deutsche Bank, and say, "What's the likelihood that a 10-sigma event will occur in a normal distribution," and of course the answer is like once every billion years. "Well, we have seen three of them in the last week." There are other possible explanations for that, no one doubts there can be fat tails in the distributions of events but you can see that it can

come from another structural source. Suppose that most of your experience with bank loans in recent times had been when the assets were large, out in the right-most portion of borrower assets in the Banking System Liability chart in Figure 3, then you see the measured sensitivity of the guarantee to asset values, the slope, is very flat. So historical experience when you fit the data of how sensitive bank loans are to the assets, are all measured in a period where it's pretty flat, that is, insensitive. So you fit the data, whether it's a regression, or something more fancy, that guarantee sensitivity is low in what you get for the numbers. If you assume that the elasticity of response is the same going forward, when the value of the assets fall and you move toward the left side portion of the Banking System Liability chart, which of course you can see is an incorrect assumption, then you say, well if the slope is only this, and if the loan value changed by a large amount, that looks like a 10-sigma event for the underlying assets and for loan value change. Actually the slope is now five times larger and what actually happened was a 2-sigma event in the underlying asset, not a 10-sigma one.

So, two sigmas with a five times larger slope looks like 10-sigma with a constant slope. That's the concept. So from all your hard work of learning to be credit experts, you can explain phenomenon about the crisis that many people asserted were "outside the box" of past experience did not fit the models or the principles of economics and finance being used, and thus calling for the creation of a new paradigm. With your acquired understanding of the credit risk structure, you can see that there is no need to scrap the current principles to understand the phenomenon. This offers a mainstream explanation.

You've been so good let me offer you an extra learning dividend. You now understand what the banks' or other lenders' risk is. But what do we know about sovereigns' role in the risk propagation process? Among many things, they have a habit: namely they almost always guarantee their banks, either explicitly or implicitly. So, what do the sovereigns do, what is their liability? It's a put option again they are providing. Right? That's what we saw was the structure of a guarantee. So when the US government writes that guarantee, they're writing a put option. On what? On the bank assets. What are the bank assets? Bank loans. What are bank loans when you pierce through them? Risk-free lending, and having sold an insurance contract or a put option on the assets of the borrower. So what is that government guarantee? It's a put option on a put option. In other words, if you break through and look at the actual assets that are affecting things, whether it is real estate, or stocks, or corporate assets, or whatever, the government then issues a put option on a put option. That's a convexity on convexity, making government guarantees doubly convex. What does that mean?

If we were to plot the government's guarantee value not against bank assets but against the real economic assets that are behind it, we will see that while it has the same shape, it is much flatter and rises much quicker because it's convex on convex, it's like double speed of change. Why is that interesting, just from a qualitative point

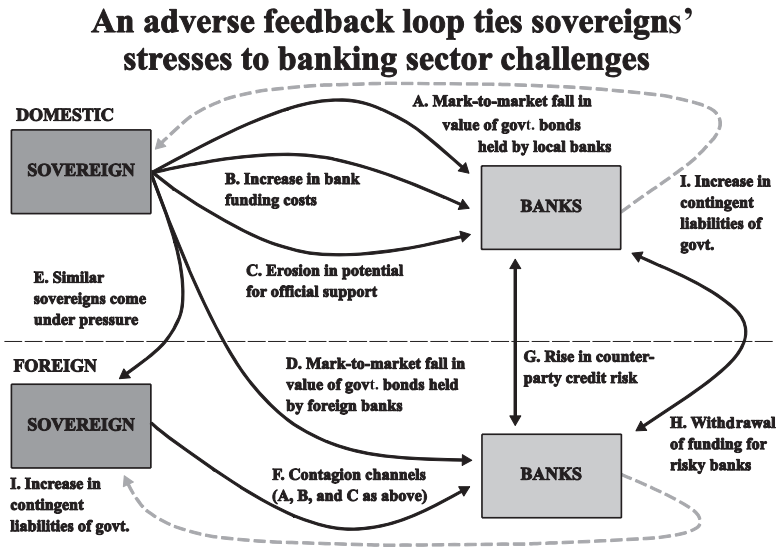
of view? It shows you, right from the core theory how it's possible for a country, or government, to be in relatively stable environment and guaranteeing loans and other things without a problem, and then seemingly get into a lot of trouble rather quickly when borrower assets decline. And what I'm saying is because of the high convexity or the high rate of change of the risk, that's possible to happen from nothing more than the structure, and as you would predict it happening.

Now, I wasn't here in Asia in 1997, but if you look at some of the cases, you know there were countries that had real estate sector problems, and all of a sudden the banks also had problems, and not too long after that, the whole country was finding itself with enormous liabilities from the guarantees by government. Some estimate guarantee liability of as much as 70% of gross domestic product, clearly untenable to deal with, and the currency decline took care of the rest. How could that happen? It is predictable as a structure—not that I could've predicted the crisis itself—I'm saying given the assets fell, say real estate falls, this is how you can get this propagation. You also see that depending on what sector it happens in, this is how you start getting propagations from the real sector, to the financial sector, to the government sector, and so forth. And there are many of these you can map. So this is the lesson we're going to use, and now I think I'm prepared to qualify those who want it, as credit risk experts level 1.

Consider guarantors writing guarantees of their own guarantors. What does this mean? Well I've got some examples here. Let's say I am a bank, and the lady seated in front of me represents my sovereign. We know that as sovereign, she's guaranteeing me, but suppose at the moment she's having a little trouble funding herself. So she comes to me and says, "Bank, I think it will be great if you bought some of your own country's bonds," and that seems reasonable. I say fine. Now what? Will she guarantee me? What do we know from our credit-learning work? I've written a guarantee on the sovereign, because I'm holding the sovereign's debt. Now let's talk our way through as what happens next if there is a shock. It's never the fault of the sovereign; I'm a bank, so I made a mistake, I bet too much or just bad luck. My assets, or my borrowers' fall, and I become a worst credit. She's my guarantor, so because I'm a worst credit her guarantee liabilities go up which makes her weaker, but since I've guaranteed her as well and she's become weaker, what does that mean about my guarantees that I've written on my liabilities? They go up again which makes me weaker. Do you see feedback?

Well, this is a simplified version. You are likely aware of this in what you've seen in Europe and Figure 4 shows a more colorful and complex version of this feedback. In Europe for example, what was the circumstance? Banks did not only own their own sovereign's debt, but also other sovereign debt, and vice versa. So you now have this feedback between the bank and sovereigns, and since the banks deal with each other, the dynamics of asset values, risk, and cost of guarantees become a lot more complicated because these feedbacks go across geopolitical borders, and one sovereign can affect other sovereigns through this mechanism. So destructive

Figure 4. **Feedback Loops of Risk from Explicit and Implicit Guarantees**



Source: IMF GFSR 2010, October, Figure 1.5, p. 4.

feedback moves are something that are inherently in there, and Figure 4 shows the various channels.

So let me describe what we did with credit risk structure on connectedness. We estimated the cost of the credit guarantee—the put option—for banks, insurance companies, and sovereigns. The higher the premium for the insurance, for every dollar of loan insured, the more risky the credit for the same maturities. If you pay a larger amount for the insurance policies, the higher the premium paid, the higher the put option value, and the less worthy your credit is, just by definition.

So you know how we measure creditworthiness. Next I describe a little more detail within the data. We divide the value of the guarantee by the risk-free value of the bond that would be the value of the instrument or the loan or whatever, if it were fully guaranteed by a credible guarantor. This calculation is shown in the first equation in Figure 5.

The resulting number is a percentage, say 6% or 7%. One never pays more than 100% for the guarantee because that's just the whole thing if you do. So it's a number like 6%, or 7%, or 12%, or 4%, or 2%. It has an inaccurate name: expected loss rate (ELR). This ratio has nothing to do with expected values in the usual sense, but that's the terminology convention. We then compute that measure of the cost of the creditworthiness such that the higher that number, the less credit worthy to analyze connectedness in credit among institutions and sovereigns.

This approach to the credit guarantee valuation for sovereigns is different than for banks and insurance companies, so let me explain why. We measure the credit risk estimate for the sovereigns using the credit default swap (CDS) market.

Figure 5. **Measuring Connectivity and Influence on Credit Ratings between Sovereigns and Financial Institutions**

- **Expected Loss Ratio = Guarantee/Risk-Free Debt**

  = PUT/B exp [-rt]

  = ELR

- **Fair Value CDS Spread = -log (1 – ELR)/ T**
- $ELR_k(t) = a_{jk} + b_{jk}\, ELRj(t-1) + \xi_t$

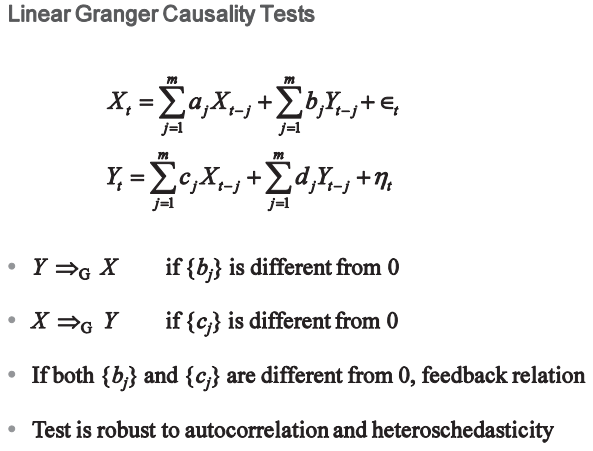
  $ELR_j(t) = a_{kj} + b_{kj}\, ELRk(t-1) + \zeta_t$
- **If b_{jk} is significantly > 0, then j influences k**
- **If b_{kj} is significantly > 0, then k influences j**
- **If both are significantly > 0, then there is feedback, mutual influence, between j and k.**

Source: Billio et al. (2012).

So these are market prices, and not ratings based. These are market prices for what it would cost to guarantee that sovereign's debt. Why did we not use CDS for banks and insurance coverage, even though they were available? The answer is we want the *total* credit risk of the entity, not just that part of the credit risk being borne by the private sector. CDS prices reflect only the latter. So for example, when Ireland guaranteed all the Irish banks, the CDS of the Irish banks fell dramatically. But did these Irish banks have better coverage ratios, better assets, or better loans? No, they're exactly the same banks they were before that announcement. Why CDS for them fell is essentially the government took on some of the credit risk (or more of the credit risk than the market had thought it was taking on), by making that statement and taking that action.

As an extreme case, imagine you have a terrible bank in terms of its financials, and the US government guaranteed 100% of the bank. Its CDS rate would be very close to US Treasury CDS not because there was no credit risk in the sense that the bank was sound, but simply because the credit risk had been transferred out of the private market and this is all that CDS measures. Since we want to understand the connection of actual institutions' real credit exposures, we don't want to use just the private sector's risk, and that's why we use an alternative. What we use is a family of models, connected in some way or another to my name. The Merton (1974) model for pricing corporated debt, published 39 years ago, was derived with the same perspective on credit that I give here and a refined version is used here to estimate the Expected Loss Ratio for institutions. This is a well-known credit model, although a much more sophisticated version than is in my paper, and has been widely used in practice for at least 20 years. It took 25 years for the innovation to be widely adapted—sometimes you have to wait a while for adoption, but it is widely used, and for purposes of measuring credit (we're not trading on it), it's going

Figure 6. **General Measures of Credit Connectedness and Influence among Institutions**



Source: Billio et al. 2012.

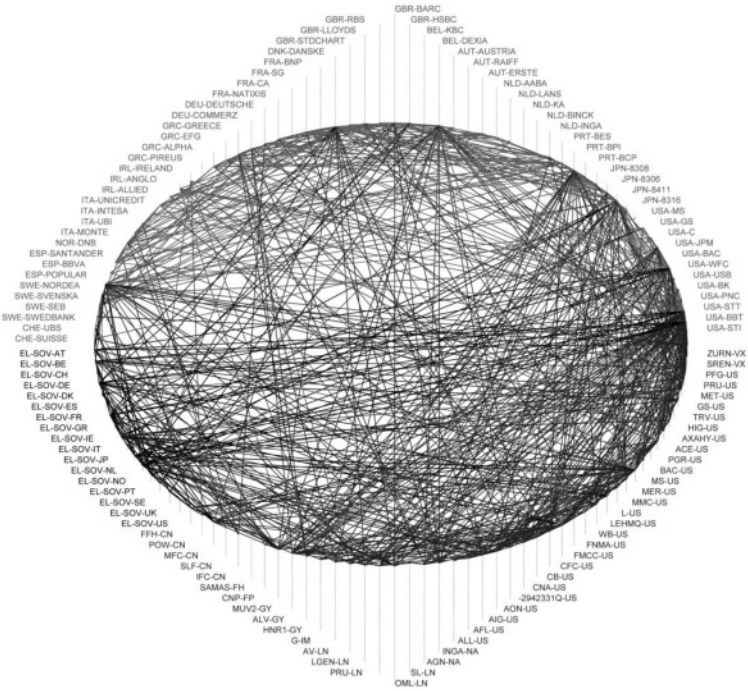
to be more than accurate enough for estimation of market value for total credit risk. Understand that we're not trading these things; we do not have to get it precisely. We just want a good credit cost indicator. The advantage of that, this allows us to get a valuation of all the credit risk that you can't get with the CDS, the reason for which I explained earlier.

Now what do we do with these data? As can be seen in Figure 6, we compute credit cost, for every institution, at each point in time, and for every sovereign, and then for each month, we ran a regression of the following: we look at the credit cost of institution *J*, or sovereign *J* last month, and apply a simple regression of that on the credit cost of institution *K* this month, so a simple one period lag—you'll recognize this looks like a Granger Causality test specific with a simple regression.

So you could interpret the relation as causal, but shouldn't because this is a reduced form. You could say that if the regression coefficient is positive and significant, then the credit of institution sovereign *J* last month, has an effect on the credit of institution of sovereign *K* next month. Its effect, however, is only in a narrow sense of timing order and is not necessarily causal in the economics sense because this model is a reduced form. We ran that regression for every institution and sovereign each month and we took all the significant coefficients in a given month and we say, in that case *J*'s credit causes *K*'s. We could run the reverse direction, and regress institution *K*'s credit last month, against institution *J* this month, and run the opposite way regression and if that also is significant we would say it's causal this way, if it is significant both ways, we say there is feedback; the effect runs both ways.

Figure 6 simply shows the specification if you want to use a more complicated version with multiple distributed lags, the principle is the same. We did the simple univariate one to begin with. I want to make it clear that the presentation of the data is mostly to illuminate methodology and hope to tease you into wanting to explore

Figure 7. **Connectedness July 2004–June 2007: Sovereigns, Banks, and Insurance Companies**



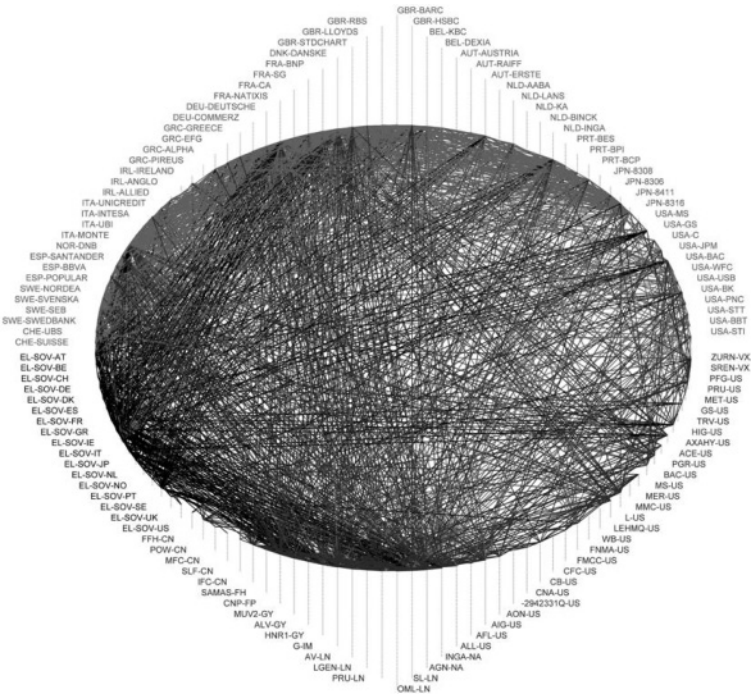
Source: Billio et al. 2012.

this approach more. You can use the same concept expanded to its own credit, and longer lags in the past, and you can do exactly the same thing we have done here. The sample we used was monthly credit data from January 2001–March 2012, for 17 sovereigns, 63 banks, 39 insurance companies, and 102 institutions. So we estimate the regression coefficients for each month, and take all the ones that are statistically significant. That's an awfully large pile of regression coefficients. The question is, what do we do with all these numbers, what does it mean, and how can be understand what we found?

So much of what I will show you, is how to compactly convey all the statistics in a fashion that is useful. The idea is take this vast array of numbers, and find ways of presenting them so our minds can see patterns or changes in patterns that are interesting to make these useful, and to do that we draw on work from network theory and things like centrality.

The idea is, how can we show a lot of information that can then be processed, by making this interesting information to look at. The first picture is shown in Figure 7. Imagine you have a camera and you left the shutter open for 3 years, that's a very slow acting shutter, and you just took the average exposure, that's what you see here. When we say 2004–2007 in one picture, think of this as superposition as an exposure for 3 years even though we could do it every day. The lines representing

Figure 8. **Connectedness April 2009–March 2012: Sovereigns, Banks, and Insurance Companies**



Source: Billio et al. 2012.

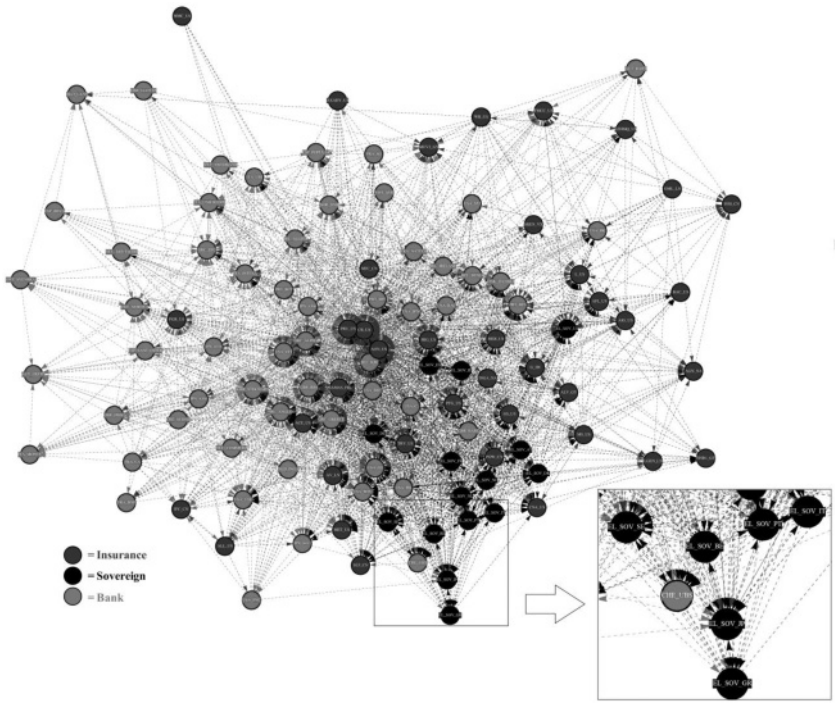
significant connections are color coded such that red lines represent the banks; blue represent insurance companies; and black represent sovereigns.[1] The only lines you see in there are the ones connecting institutions and sovereigns and so forth, which are significant, and not every possible combination so what you see are significant connections.

If you could magnify each of those lines, each has at the end of it an arrow, which means that the coefficients causing significant effect are in the direction of the arrow. If there are arrowheads at both ends, it means significance in both directions. So we put them all there and we do something further. We look at all these vast numbers and their statistical significance, and calculate their centrality, their eigenfunction or principal components, and then physically locate the most important ones, only measured in terms of their centrality, closer to each other so you can see clusters. When you see a bunch of entities piled around each other, you know there is something that has a lot of influence in that period, and there are a lot of influences measured by the regression coefficients.

Figure 8 shows a similar picture from a time lapse camera from 2009 to 2012. So now we have a picture during the crisis, and one that is pre-crisis. Before you do

[1] The color version of Figures 7–12 are available upon request (asiandevreview@adb.org).

Figure 9. **Connectedness to Greece: August 2008**



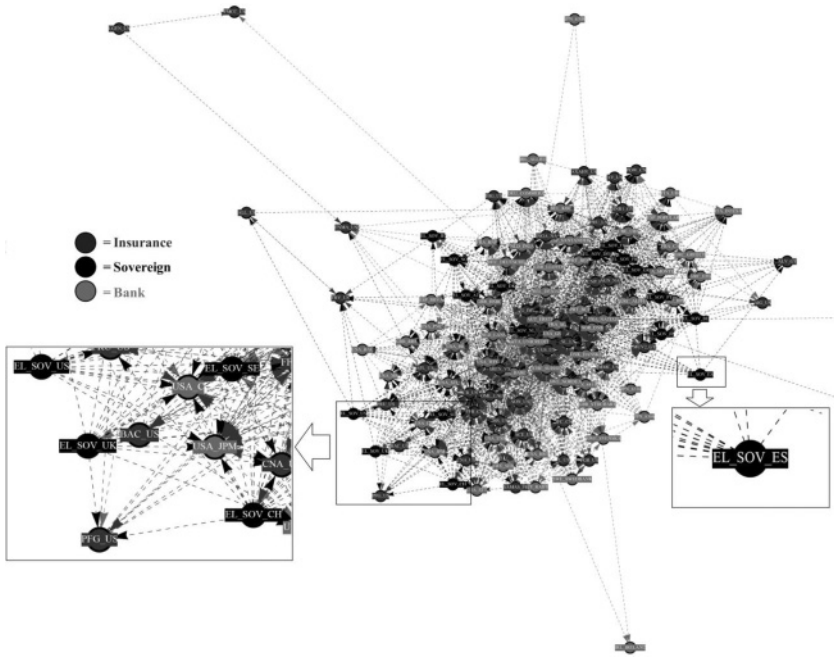
Source: Billio et al. 2012.

anything—notice that Figure 8 is much richer. The color is deeper, there are obviously many more lines, and many things bunched closer together than they were before.

These lines and how they are located tell you that there are many stronger connections and bigger connections with greater centrality. And then how those are apportioned among the two institutional classes and the sovereigns, you can gauge from the color. Now I like to say this, although this is a little overstretched—do you ever go to the museum to look at a painting you like, and you find a bench, and you sit down for the afternoon and look at the painting? What do you discover if you like the painting to begin with? The more you look at it, you see more and more nuances, that's why you do it. If you look at Figures 7 and 8, you'll see more and more nuances in the same way.

So the first thing you see, no matter what else, is the degree of connectedness changes materially, where you understand what I mean by connectedness between the two entities, and I remind you this connectedness only applies to credit, not other degrees of connectedness. You'll see that's important. So, now we zoom down, I'm just going to show you pictures of some periods to see how you could use this. Figure 9 shows a zoomed-in picture showing Lehman and Greece in 2008. Figure 10 shows December 2011 before Spain really got into the front pages. We were worried

Figure 10. **Connectedness to Spain: December 2011**



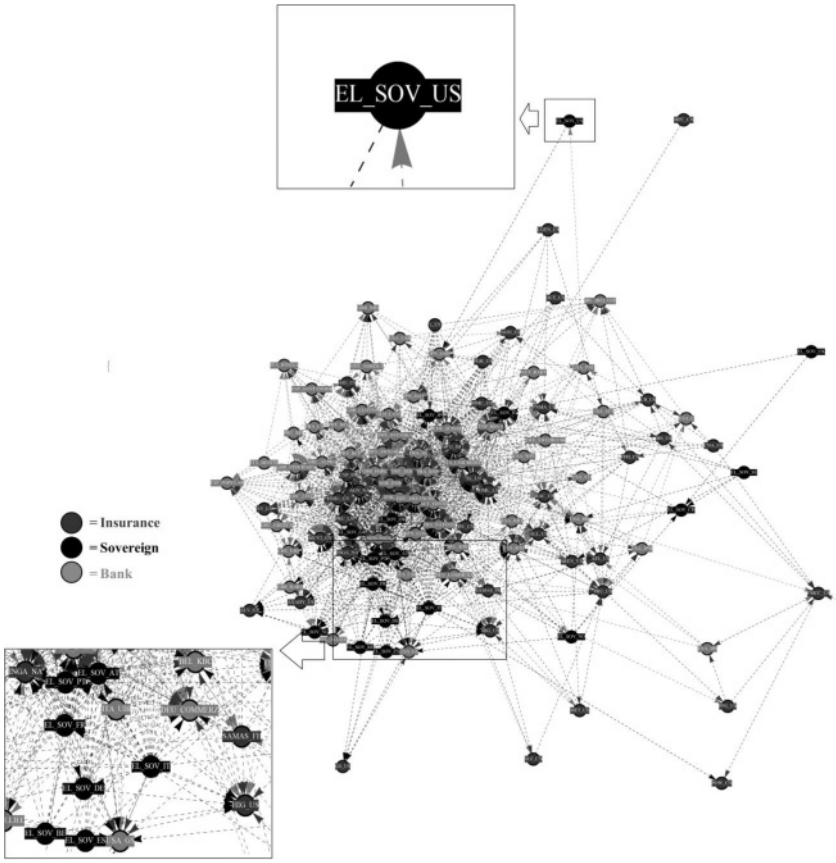
Source: Billio et al. 2012.

about Spain, but then here's Spain in that mess where you see the circle that's Italy, they're different.

Now jump ahead to March 2012 as shown in Figure 11. Note the position of the US, what do you see? It is practically unconnected. This scenario by itself, how could the largest economy, with the most important central bank, not be highly connected with all of the other sovereigns and institutions? Now I'm not telling you this is the only interpretation possible for the data. I'm trying to help you see how to read the figure. Note that these interpretations are simply hypotheses—questions—not statements or judgments or answers. One hypothesis could be that 2012 was an election year for president. What are the odds in your mind that the United States was going to step up and do some big credit action like say, write another $500 billion euro swap facility, or something else, to impact credit for others in the election year? I would think very unlikely. It just was not going to happen politically. At that time, the US was the strongest credit in the world, and so its credit would not be impacted by other sovereigns or institutions.

So you have a situation where it could be the case that the US had very little importance of connectedness in terms of impact on credit in either direction. It doesn't mean there are no other kinds of connections like other flows. We're not looking at those connections. Connection doesn't mean amount of business, say

Figure 11. **Connectedness to Italy and US: March 2012**



Source: Billio et al. 2012.

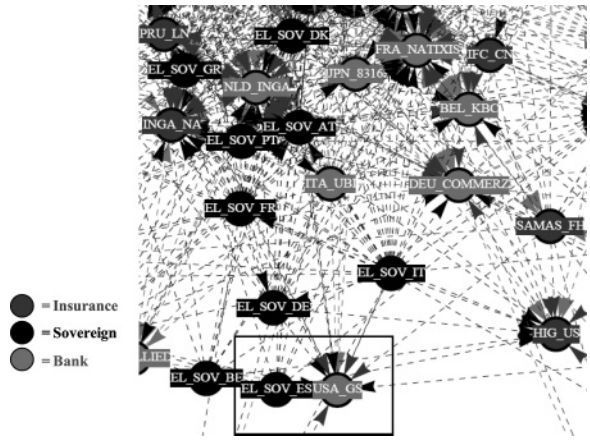
measured by number of phone calls between them. In our example, it is precisely and only the relationship between their credit levels. So we are filtering out, in that sense, all other dimensions and focusing on one important dimension with this analysis.

Figure 12 shows another close-up of the connectedness of sovereigns and institutions. Italy is right in the middle of things, as usual. Note also that sovereign Spain, the black one node at the bottom, is practically married to that red bank. That bank is a US bank; it is Goldman Sachs. You'll also see that there are quite a few blue lines, those are insurance companies. Italy and other sovereigns, and red lines are also connected to the two.[2]

So, why do I show you these pictures? I want to demonstrate how this type of technology could be used. I think it's a technology for asking good questions. What do I mean by "good questions"? Think of when you try to go for a stress test, how

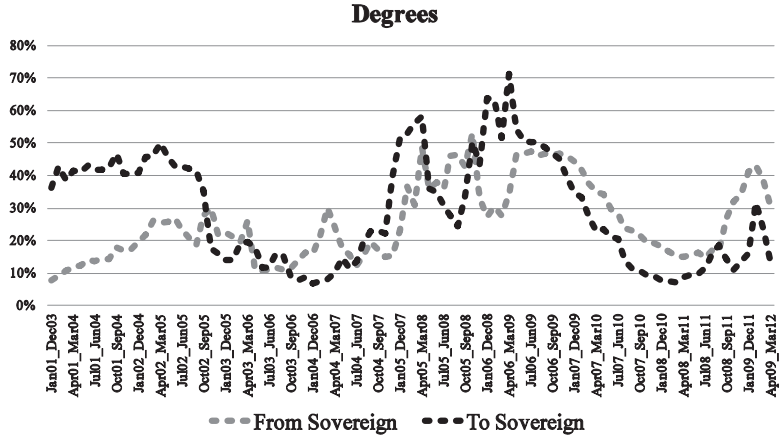
[2]See footnote 1.

Figure 12. **Connectedness to Italy: March 2012**



Source: Billio et al. 2012.

Figure 13. **Network Measures: From and to Sovereign**



Source: Billio et al. 2012.

do you decide what stress test to run? You ruminate. If you do a hundred stress tests, how many tests didn't you do? The answer is an uncountable number. I believe this connectedness analysis is a procedure for systematically suggesting what stress tests are to be undertaken by asking interesting questions and therefore try to find the answers. If I see that "marriage" between Spain and Goldman Sachs in Figure 12 at that time, at the very least if I were responsible for financial stability I think I would call Goldman Sachs, and say, "Does this make sense to you, is there something here?" If they don't know, they would probably be happy you called because they do care about such things too. If they do know, you resolved it, or conclude, "I'm fine." If you don't, maybe that becomes a recommended stress test: Why is this the

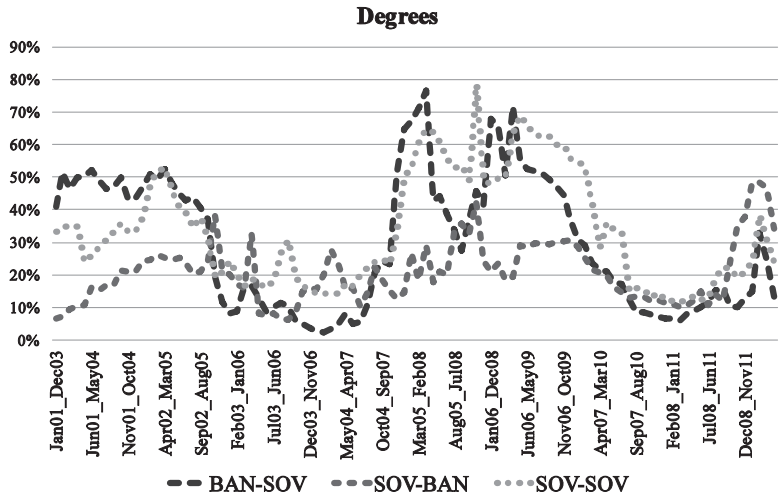
credit relationship between the institution and the sovereign? What does it mean? I think this can be a very useful tool because it's dynamic. We can run this every day. Being driven by market data as inputs, we can do this analysis every single day, and as I'll show you in the last few minutes, the dynamics of those connections can change dramatically. So the runs and resulting stress tests you run today, a month from now, or two months, or three months from now, will likely be very different. So this method is both dynamic and it picks up from the system what kinds of connectedness are going on. There's nothing inherently wrong with the degree of connectedness shown in the figures, it just indicates it's an interesting question to understand why it has this degree of connectedness, and that's what it is about here.

Figure 13 shows the dynamics of how these things change. What we plotted here is estimated coefficients from sovereigns.

The arrow coming from the sovereign means that the credit impact is coming from sovereigns, and the other denotes that the credit impact is coming from the bank or the insurance company to the sovereign. Figure 13 plots the percentage of the significant regression parameters of all the connections from 2003 to 2012, such that it includes both pre-crisis and post-crisis periods. I'm not going to go through each of these; the blue line is the percentage of credit flows from sovereigns, that is, sovereigns credit causing effects on credit of others. You'll see a very small percentage was from sovereigns in the pre-crisis period, and then it went up to almost double that, and down again and subsequently went back up. So all I want you to see is you're just measuring how many times that apex coming from sovereigns changes quite a bit depending on the period. The same thing for credit risk going to, impacting sovereigns, which starts the time period much higher, then when banks get in trouble, they have an effect on the sovereigns, and so the red line moves down and then up and so forth. The main reason for this chart is to show you the degree of direction of connectedness changes a lot through time. Figure 14 shows how sovereigns affect other sovereigns as represented by the dotted line. So all I want you to take away is that a lot is changing as you go through the pre- and post-crisis periods, and this analysis seems to be capturing that. With this kind of dynamic change, this analysis is not something you do once but it's something you want to do often.

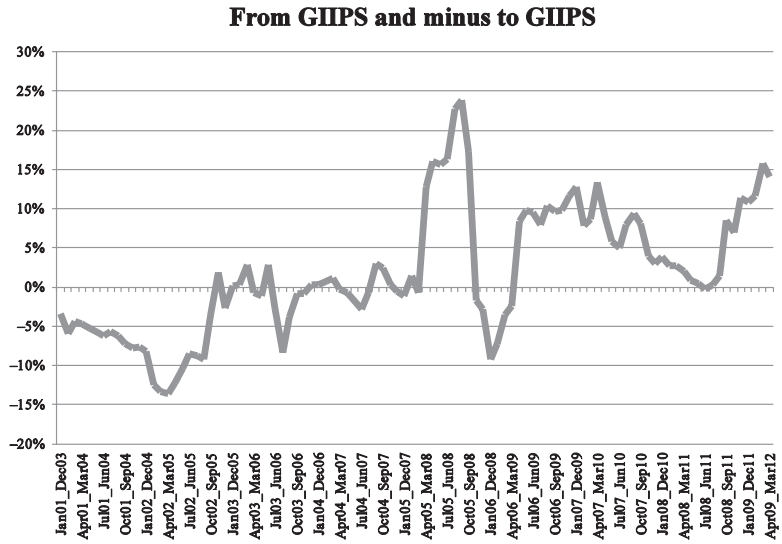
Figure 15 shows a similar chart for a group of European countries. I'm sure you're all aware of the somewhat disparaging acronym that has been applied to these five countries starting with Portugal. My colleagues and I tried to find some other acronym—the best we could come up with is G–I–I–P–S, for Greece, Ireland, Italy, Portugal, and Spain. These countries were very much involved in Europe and the European crisis, and what we ask here is what patterns emerge between those five sovereigns impacting other sovereigns and institutions versus those five being impacted by them. Figure 15 plots "from GIIPS" that's when these five sovereigns are impacting other sovereigns or institutions and minus "to GIIPS" when they're being impacted by others.

Figure 14. **Network Measures: From and to Sovereign**



Source: Billio et al. 2012.

Figure 15. **Greece, Ireland, Italy, Portugal, and Spain: GIIPS**



Source: Billio et al. 2012.

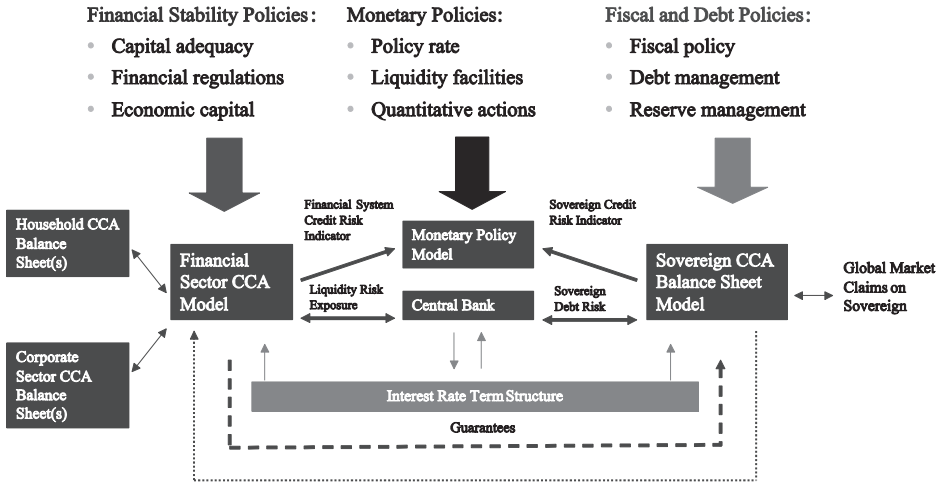
Notice prior to the crisis the net is largely quite negative which means that the effect on the GIIPS was much larger than the effect from them. I think that makes intuitive sense, for the most normal conditions, certainly Greece and Portugal are very small economies. So if you had to guess, you would normally think smaller entities would be pushed around more by the other entities rather than the former

doing the pushing. So that's normal, but look at what happens in the crisis period. It changes to a large net positive for "from GIIPS." The curve goes up, then comes back down, and then goes back up again. So, if you now map the time series what you see is how differently the impacts, one way or the other, change during the time series pre- and post-crisis periods.

In my last formal remarks, I make a plea and present an opportunity that I hope you will consider. The general plea is—normally, in the official sector, we have three major activities that we talk about: financial stability, monetary policy, and fiscal or debt policy. While there are some discussions among those making policy decisions on each of the three, I do not believe, (and you can correct me if I am wrong), that there's a coordinated process by which these three policy and decision makers talk to one another. I'll give you an example from the US as to what I mean by that. As you are well aware, with quantitative easing, the Fed made a decision to bring long-term rates in the US way down, and the rationale was to try to stimulate investments and consumption. However, by driving long-term rates down, what was a major unintended impact of that?

The pension system in the United States got absolutely crushed because the value of pension liabilities, which are a promised fixed set of payments like a long-term bond, exploded. This large increase came from change in value of existing payment liabilities, and not from additional promised payments being added to them. When rates come down in the long end, it really impacts the value of existing liabilities. You need a lot more money to fund the same level of payments. Seven years ago a bank deposit of a million dollars could earn $40,000 or $50,000 a year. Last year you'd be lucky if it earned $1,500. The same million, in one case earning $40,000 or $50,000, and in the other $1,500. You see that you need a lot more money to generate income when rates are low, so the pension liabilities exploded upward and in fact, a fair estimate of impact on the public pension system of state and local governments in the US is somewhere around $3.5 trillion underwater. I don't mean that's the liability, I mean that's the delta, the amount of underfunding. Although there are many other reasons, including not enough contribution, that chronically contribute to the underfunding problem, the important portion of the increase in shortfall was the fall in interest rates. Does this mean lower interest rate policy is not a wise thing? No. What it simply says is—it would be wise to consider as a part of the policy that you're looking at, is that trying to lower rates to stimulate the economy can have a large unintended impact in other areas—in financial stability for example, and on insurance companies liability that would be suffering from the same thing. So if you're worried about insurance companies who make long dated guarantee liabilities, they face the same problem, and whether or not you mark-to-market the liabilities, it doesn't change the economics. I still get $1,500 a year on my million versus $40,000–$50,000, and I can't live on that. So the economics, no matter how you treat it, whether you realize it in accounting or not, doesn't change, so that's the suggestion.

Figure 16. **Unified Macrofinance Framework Targets**
Inflation, GDP, financial system credit risk, sovereign credit risk



Source: Gray 2011.

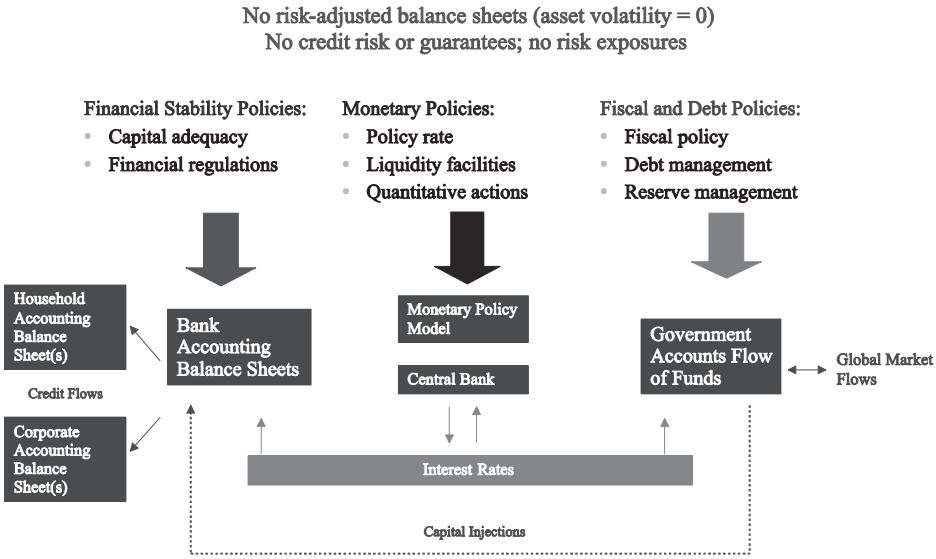
The other suggestion is, if you look at these macro models, the ones my understanding are used, like in the New York Fed and so forth, those models are structurally certainty models and don't have uncertainty in them. So you say, "Wait a minute. They have their error terms and they do Monte Carlo simulations on them. In fact we generate whole distributions."

So what do I mean? I mean, they don't have some of the risk components of Figure 16 that relate the Financial Sector to Monetary Policy and Fiscal Balances. What do we see here? When you build a finance model with the actual uncertainty taken explicitly into account, there's real uncertainty in the structure, then guarantees and other insurance have value. If you build a certainty model, insurance has no value and thus disappears completely from the model structure because you know what the future is.

So that's what I mean by "structural." If you have options, if you have these uncertainties and the insurance, you have taken account of uncertainty explicitly in the structure. If you have a certainty model, your options show up as being zero value and zero risk, they are useless and you have no role for insurance. Now what does that mean? We know in this model we already saw it. Changes in volatility will impact the system even if there's no change in asset value and even if there are no defaults. You know that this curve will shift. So if you don't have that it in there, your model will never pick that up.

Figure 16 has models with the feature that if assets drop in the model, even if there are no defaults, the risk for the next play in the simulation has gone up, if you don't have that in your model you're not capturing it. So what I suggest to

Figure 17. **Traditional Flow and Accounting Framework**



Source: Gray 2011.

think about is a potentially very big addition so these models explicitly built in the structure, and it can be done. If instead you simply apply Monte Carlos where you just add on error term to the certainty model, the math does what you tell it. Even though you get another distribution, for any path the model "thinks" the rest of the future path is certain, because the model is the only thing that comes after that.

And if you put square terms and non-linearities in some general regression, that in itself is not likely to get it. So part of the lesson here given the audience that I'm talking to is to say—I think there's a very big growth area for introducing explicitly the uncertainty structure and understanding in the modeling. Now maybe that's already going on. Dale Gray, IMF, has been a long and tireless proponent. I've been a co-author with Dale Gray; I don't know if he's been here to ADB. He's been about everywhere else in the world and has been on a quest for 10 years building these models for whole countries. So there has been some work but I would say it's not the mainstream of macro and I think it's a great new area for applied research and academic research as well, where you're trying to capture the impacts going through on risk propagation. If you can have these uncertainty models as part of the structure, you will improve on the richness of it.

For reference and comparison, Figure 17 shows the traditional approach, with all structural uncertainty eliminated.

It shows the set of connections as they exist now with certainty models where you set the standard deviation of every variable to zero in this structure. Note that there are some things missing compared to Figure 16. That's what I wanted you to

see. So as a last sort of little plea, I thought you might find this as something that might convince some of you to do a little more investigation, or at least be aware it's out there.

This is all doable since these models have been used for 40 years. They are very highly refined. Institutions like Goldman Sachs and Deutsche Bank probably have better models than the academics because of the resources they have to build them, and the data, and partly because they use them in a much higher degree of precision than most of the time in academia. For what I'm using them for today, you don't need that precision, but you do know since you understand the structure, any option model, any credit model, that you put in there can be plugged in this system. So if you use a simple Black–Scholes model, or if you use a complex Goldman Sachs version, the principal structure of the interpretation is the same, it's just a better model.

Thank you very much for your long attention. I'll take any questions.

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