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Road Maintenance and Local Economic Development: Evidence from Indonesia’s Highways
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ABSTRACT

This paper estimates the local welfare impacts of highway maintenance investments. We instrument road quality exploiting Indonesia’s two-step budgeting process for allocating funding to local road authorities. Using comprehensive data on road quality from 1990-2007, we find evidence that better roads help manufacturers create new jobs, enabling worker transitions out of informal employment, and increasing labor income. Road quality also changes the cost of living, reducing perishable food prices but also raising housing prices. We estimate the elasticity of household welfare with respect to road quality to be 0.1 and the benefit/cost ratio for road maintenance investments to be 2.3.

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1 Introduction

Road maintenance, which includes repaving and resurfacing existing roads, is often justified as a public goods investment to stimulate economic activity and create jobs. In fact, a common response to macroeconomic shocks such as the great recession and the COVID-19 pandemic has been to increase spending on infrastructure, a large part of which is dedicated to road maintenance. Although most public expenditures on roads are allocated to rehabilitation instead of new construction, we lack credible evidence on how maintenance investments impact local economic development outcomes and welfare.[1] Our lack of understanding is particularly acute for developing countries, where the effects of smoother pavement surfaces, and the faster speeds they allow, could be particularly transformative.

In this paper, we study how changes in road quality impact local economic development outcomes and welfare in Indonesia. Our empirical analysis relies on a long and comprehensive administrative database on highway surface quality. From 1990 to 2007, Indonesia's highway authority measured the roughness of each segment of all national and provincial highways on three of the country's most populous islands: Java, Sumatra, and Sulawesi. We use these data to measure average road quality in each district using the World Bank's International Roughness Index, a widely used measure in the civil engineering and transportation literature. We combine these spatio-temporal measures of road quality with economic outcomes from two main datasets: (1) the Indonesia Family Life Survey (IFLS), a nationally representative panel that tracks households over time; and (2) the Industrial Survey (SI), an annual census of manufacturing firms that allows a study of firm responses to road quality.

As is common in the infrastructure literature, the main challenge to obtaining causal estimates of the effects of road improvements is that investment decisions are usually not determined exogenously. For example, if planners target areas for improvement based on economic trajectories or political characteristics, this generates selection bias (Blimpo et al., 2013; Burgess et al., 2015; Asher and Novosad, 2020). We overcome this challenge with an instrumental variables strategy that takes advantage of Indonesia's multi-stage budgeting process for road financing. This budgeting process separates overall road budget allocations to local road authorities from the decision of which roads to upgrade.

Indonesia has a unitary central government. Nested below the center are provinces, the first tier of local government, and each province is divided into districts, which comprise the second tier.[2] Independent road authorities at the provincial level make investment decisions in the districts under their jurisdiction based on a two-stage budgeting process.

In the first stage, the central government sets an annual national budget for road maintenance, and this common pool is subsequently allocated to provincial units based on formulas that depend on observable characteristics. All variables that go into these formulas are measured with long lags. This ensures that the formula-based funds each province receives are uncorrelated with shocks to recent provincial economic activity. In the second stage, each provincial authority uses these allocated funds to upgrade its choice of highway segments in different districts. This implies that endogeneity of road investments at the district-level is limited to second stage decision making.

[1] Engineering models from the World Bank (1994) estimate that the returns to road maintenance are twice as high as those for network expansions. Foster et al. (2022) find that across developing countries from 2010-2018, infrastructure spending has been low and declining over time.

[2] In 1990, the beginning of our sample period, Indonesia was divided into 26 provinces and 290 districts.

Because each province's formula-based funds are not responsive to changes in local economic conditions in the short run, the province's total budget for national and provincial roads, established in year $t-1$, should be a good instrument for road quality in districts within that province in year t. Unfortunately, since annual data on each province's financial budgets for road maintenance are not available, we proxy these budgets using the road quality data directly. Our budget proxy for funds allocated in year $t-1$ to province p is the total number of kilometers of roads upgraded in that province between $t-1$ and t. We use separate budget proxies for national and provincial roads as instruments for district-level road quality, and we show that our budget proxies have a strong first stage relationship with district road quality. Moreover, the provincial budget proxies are not predicted by lags of log provincial GDP or lags of log provincial population. We also conduct several robustness checks, including a leave-one-out budget IV, to rule out a mechanical relationship between our IVs and the variable we instrument.

To guide our empirical analysis, we begin by providing a simple framework that allows us to decompose the channels through which marginal changes in road quality affect household welfare. These channels include: (1) wage income; (2) firm profits; and (3) living costs (prices). We causally estimate the effect of road quality on these different outcomes using our instrumental variable approach, and then we combine those estimates with our framework to estimate overall household welfare effects.

Using household and individual-level outcome measures from the IFLS, our main results show that when road quality improves, nominal income and consumption expenditures increase. The elasticity of per capita consumption to road quality is about 0.3, implying a large and significant effect. We neither observe extensive margin effects on employment nor intensive margin effects on hours worked. Instead, we show that the observed increase in consumption expenditures is mainly driven by an occupational shift out of informal employment and into higher-wage manufacturing and other formal jobs.

Given the importance of this formal sector employment result, we verify that it also holds using SI data on large manufacturing firms. District-level panel specifications confirm that better road quality is indeed reflected in increased entry, higher total value added, and larger output of manufacturing firms. Because the output response is larger than the impact on employment, output per worker also increases. Next, using firm-level panel regressions where we control for firm fixed effects, we demonstrate that increases in the performance of manufacturing firms appear to be driven by newly created firms, and not by changes in output or output per worker of incumbent firms.

When evaluating the impact of road quality on the cost of living, we find that road quality improvements reduce the price of perishable food products, but they also have a positive (though insignificant) effect on non-perishable goods prices. Road improvements also generate higher land values and rents, consistent with road quality being a productive amenity. Finally, we examine the extent to which road improvements impact migration, as this could play an important role in our welfare analysis. Using cross sectional census data, we provide suggestive evidence that better roads lead to increases in internal migration, but these statistically significant effects are not economically large. For this reason, as a first approximation, we ignore the potential for road quality to induce migratory responses when calculating household welfare.

Our main estimates are robust to a number of different specifications, including: (1) different ways to construct instruments; (2) controls for lagged values of own-province and adjacent-province economic conditions; (3) controls for island and province-specific time trends; (4) controls for other simultaneous changes in infrastructure; (5) different cuts of the sample; and (6) different ways of conducting inference. We also show that the increase in the number of firms and employment is not driven by footloose, foreign-owned manufacturing firms, nor is it simply due to displacement of other firms from nearby districts. This suggests that our results represent genuine development effects instead of a reshuffling of economic activity.

Next, we combine these separate reduced form effects to estimate how road improvements impact overall household welfare. We find that a 1 percent increase in road quality increases household welfare by 0.1 percent on average. The median upgrade in the data increases road quality by 5 percent and therefore increases total welfare by 0.5 percent. Most of these welfare effects owe to increases in labor income that are driven by the growth of formal factory employment opportunities.

Finally, we conduct a benefit-cost analysis using counterfactual simulations in which we pave all national and provincial roads in each district. For the median district, positive stimulus benefits are enjoyed for 6 years before deterioration erodes road quality back to its initial levels. Accounting for the costs of road upgrades and effects on prices (as in Egger et al., 2022), we find that for the median district, upgrades would generate a positive net present value (NPV) of 3 percent of district GDP. Expressed in terms of a benefit-cost ratio, the median upgrading program is quite cost effective even after considering price effects, with the stream of benefits equal to 2.3 times their costs. One limitation of this exercise is that it involves taking estimates of the welfare effects of marginal road improvements and extrapolating them to a setting with potentially non-marginal road upgrades, so it may ignore important general equilibrium impacts. Nevertheless, we think it provides a useful benchmark for the signs and magnitude of expected overall welfare improvements.

This paper is related to the macroeconomic literature on the benefits of infrastructure spending as fiscal stimulus (Gramlich, 1994; Chandra and Thompson, 2000; Leeper et al., 2010; Leduc and Wilson, 2013). Unlike new highway construction, which takes much longer to plan and develop, the road resurfacing and upgrading projects we study are "shovel ready" and better suited for fiscal stimulus. Few estimates of the transport stimulus effects exist in the literature, and the vast majority of evidence is from developed countries (see Leduc and Wilson, 2014, for a review). Recent transport spending in developing countries has largely been pro-cyclical (Foster et al., 2022), which is unfortunate given that the economic boost provided by government spending tends to be largest during recessions (Auerbach and Gorodnichenko, 2012). It is important to assess the stimulus effects of infrastructure spending in a lower-middle income country, where the benefits and transmission mechanisms could be quite different, and our paper fills this gap.

We also contribute to a sizeable literature evaluating the impact of transport infrastructure improvements in developing countries. A large body of work studies the effects of newly created surface links that expand transportation networks, including China's new national trunk roads (Banerjee et al., 2020; Faber, 2014), India's Golden Quadrilateral Project (GQ) (Ghani et al., 2016), new railways in colonial India (Donaldson, 2018), or new highways in Brazil (Morten and Oliveira, forthcoming; Bird and Straub, 2020). Our work instead focuses on quality improvements to existing roads, which are relatively understudied (Cosar et al., 2022; Currier et al., 2023).[3] Such projects may both be more politically feasible and also more cost effective.

[3] An important exception is Chaurey and Le (2022) who study the effect of rural road upgrades in backward districts in India.

Our work is also related to a large body of evidence on the impacts of transport improvements in rural areas, which often involve upgrades to existing unpaved roads (e.g. Aggarwal, 2018; Gollin and Rogerson, 2014; Khandker et al., 2009; Valdivia, 2011; Khandker and Koolwal, 2011; Casaburi et al., 2013). Asher and Novosad (2020) find that new rural roads built by the Village Road Program in India lead to a transition of workers out of agriculture, but there were no impacts on income, unlike in our work. One possible reason for our different findings is that our paper studies highways instead of rural roads, which are far more important for moving local goods and people. Another advantage of our work is that it benefits from a continuous road quality measure, instead of binary treatment indicators that are often used in the literature.

This paper's results also relate to the structural transformation literature and the shift from agricultural to manufacturing employment (Lewis, 1954). Our results show that road quality can reduce informal employment and expand formal employment opportunities, increasing wages. The potential for formal factory employment to provide a source of wage and income gains for workers outside of urban areas has also been identified in other contexts, such as India (Foster and Rosenzweig, 2004).[4]

The rest of the paper proceeds as follows. Section 2 provides a framework for evaluating the welfare effects of road improvements. Section 3 describes the data we use, and Section 4 describes the historical and institutional background behind the evolution of road quality in Indonesia. Section 5 describes our identification strategy, and Section 6 presents our reduced form results. Section 7 shows that our main results are robust to different specifications and investigates some of the mechanisms behind our findings. Section 8 presents our estimates of how road quality impacts welfare and conducts counterfactual simulations, and Section 9 concludes.

2 Welfare Framework

In this section, we present a simple framework to describe the channels through which marginal improvements in road quality impact household welfare. Let i index households, and let $U_i = U(\mathbf{c}_i, H_i)$ denote household i's utility function, which is defined over a vector of consumer goods, $\mathbf{c}_i = (c_{i1}, c_{i2}, ..., c_{iK})'$, and housing, H_i. For simplicity, we specialize to a Cobb-Douglas functional form:

$$\max_{\mathbf{c}_i, H_i} \left(\prod_{k=1}^{K} c_{ik}^{\alpha_k} \right) H_i^{\alpha} \quad \text{s.t.} \quad Y_i = \sum_{k=1}^{K} p_k c_{ik} + p_H H_i \ ,$$

where $k = 1, ..., K$ indexes the different types of consumable goods available to households, $\{\alpha_k\}_{k=1}^{K}$, and α are constants, and we assume that $\sum_{k=1}^{K} \alpha_k + \alpha = 1$.

Households earn income primarily through labor sources, Y_{Li}, but they may also derive income from

[4] Another strain of literature that our work relates to studies how transport infrastructure improvements can shape economic geography using the lens of trade theory. For example, Donaldson and Hornbeck (2016), Asturias et al. (2019), and Storeygard (2016) find large, positive effects of transportation infrastructure on aggregate welfare and income, but for these studies, the impacts largely came about due to a reduction in transport costs and an increase in trade volumes. We complement that work by interpreting local transport infrastructure improvements as having a productive amenity effect and show that Indonesia's road improvements led to the development of new firms, leading to a transition of labor out of informal employment. Our setting also complements work in urban economics on highway improvements that has mostly focused on the U.S. interstate highway system. For example, Duranton and Turner (2012) investigate city growth effects, Michaels (2008) analyzes skill premia changes, while Baum-Snow (2007) documents suburbanization effects.

a farm or non-farm business, earning profits given by π_i. Total income, Y_i, is therefore given by the sum of wage income and business profits: $Y_i = Y_{Li} + \pi_i$. Household i's indirect utility function is proportional to:

$$V(\mathbf{p}, Y_i) \propto Y_i \cdot \left(\prod_{k=1}^{K} p_k^{-\alpha_k} \right) \cdot p_H^{-\alpha} \quad . \tag{1}$$

Taking logs of (1), and totally differentiating with respect to log road quality, $\log A$, we obtain:

$$\mathcal{E}_{V_i,A} = \underbrace{\theta_{Y_{Li}} \mathcal{E}_{Y_L,A} + \theta_\pi \mathcal{E}_{\pi,A}}_{\text{(I)}} - \underbrace{\sum_{j=1}^{J} \alpha_j \mathcal{E}_{p_j,A}}_{\text{(II)}} - \underbrace{\alpha \mathcal{E}_{p_H,A}}_{\text{(III)}} , \tag{2}$$

where $\mathcal{E}_{y,x}$ denotes the elasticity of y with respect to x, and $\theta_x \equiv x/Y$ denotes the share of x in total household income.[5]

Equation (2) shows that the elasticity of household welfare with respect to road quality is the sum of three components. The first component, (I), is the effect of changing road quality on household labor income and household-owned business profits. On the labor income side, we account for the fact that many developing economies are characterized by the dual-economy nature of their labor markets, where there is a high wage, productive formal sector, and a low-wage, unproductive informal sector (Lewis, 1954; Temple, 2005; La Porta and Shleifer, 2014). If a local labor market faces poor road quality, formal sector employment opportunities may be limited or nonexistent, and this forces workers to instead supply their labor to the informal sector. Agriculture represents a large source of informal sector employment, but there are also other informal employment opportunities, such as manufacturing and local services (e.g. Singh et al., 1986; Benjamin, 1992; Bardhan and Udry, 1999).

However, when road quality improves, formal manufacturing firms may now be able to operate in locations that were previously infeasible for production. When those firms are created, they provide new, higher wage, formal sector jobs that were not available before, facilitating worker transitions out of informal employment (Foster and Rosenzweig, 2004). Growing availability of higher wage employment opportunities is expected to increase workers' total earnings and consumption. In the empirical analysis, we therefore analyze the margin of informality in labor income. We also allow road quality to increase farm and non-farm business profits, as in Jacoby (2000).

The second term, (II), is a weighted sum of the elasticities of prices with respect to road quality across different goods, where the weights are baseline expenditure shares. As road quality improves, transport costs may fall, and this could directly impact the prices of goods consumed by households. We expect that perishable goods prices may be more sensitive to road roughness than prices of non-perishable goods, which are durable and less sensitive to transit times.

The third term, (III), is the elasticity of housing prices with respect to road quality, multiplied by α, the share of the consumer's income spent on housing. If road quality is a productive amenity, valued by both producers and consumers, firms and workers may sort into locations with better roads, bidding up the price of land, as in standard spatial equilibrium models (e.g. Rosen, 1979; Roback, 1982). With negligable migration costs, road improvements would lead to population growth and increased housing

[5] Appendix C provides a derivation of equation (2).

prices, hurting renters and benefiting homeowners. However, if internal migration is costly (e.g. Bryan and Morten, 2019), migratory responses to road quality improvements could be dampened. New manufacturing firms may be able to bring jobs to a community, raise wages, and increase incomes, leading to positive welfare benefits for affected communities that are not completely bid-away by housing price effects.

In our empirical work, we separately test each of the different aspects of this theoretical framework. We study whether road quality improvements: (1) lead to new manufacturing jobs; (2) encourage workers to switch sectors; (3) increase total earnings and consumption; (4) increase housing prices and land values; and (5) change prices of perishable and non-perishable goods. We first obtain causal estimates of these effects using an instrumental variables approach described below. Then, we use this framework to quantify the overall welfare effects, examine distributional impacts, and describe the relative importance of different channels affecting household welfare.

3 Data

In this section, we describe the main data sources used in our analysis. These are the road quality data, household survey data and manufacturing data. We briefly describe each data source here, leaving additional details for Appendix B.

Road Quality Data. Our road quality data are from Indonesia's Ministry of Public Works and Housing (*Kementerian Pekerjaan Umum dan Perumahan Rakyat* or Kemen PUPR). Every year, Kemen PUPR monitors pavement quality for all segments of national and provincial highways. Surveyors collect information on surface type, width, and road roughness. Our data contain more than 1.2 million kilometer-post-interval-year observations for all national and provincial highways in Indonesia from 1990-2007. The road survey data were merged to maps of road networks to construct an annual spatial panel of road quality.

Road quality is measured using the International Roughness Index (IRI), a widely accepted indicator from civil engineering developed by the World Bank in the 1980s. The IRI is defined as the ratio of a vehicle's accumulated suspension motion (in meters), divided by the distance traveled by that vehicle (in kilometers) during measurement.[6] All else equal, when driving on rougher roads marked by potholes or ragged pavement, drivers decrease speeds and increase their travel time. Rough surfaces may also increase maintenance costs, lead to greater fuel consumption, and even cause accidents (Bock et al., 2021).[7]

Let $\mathcal{R}_d$ denote the set of national and provincial road segments in district d, and let len_r denote the length of road segment r. We measure the average road quality in district d by taking the negative of a length-weighted average of roughness for all road segments in that district:

$$\text{Road Quality}_{dt} = (-1) \times \frac{\sum_{r \in \mathcal{R}_d} \text{len}_r \cdot \text{IRI}_{rdt}}{\sum_{r \in \mathcal{R}_d} \text{len}_r} \quad , \tag{3}$$

[6] See Appendix Section B.1.2 for more details on the IRI.

[7] During our period of study, fuel consumption and labor costs accounted for more than 50 percent of vehicle operating costs in Indonesia (Asia Foundation, 2008).

where IRI_{rdt} denotes the roughness of road segment r in district d at time t. The average in equation (3) is taken over all national and provincial roads located in the district.[8]

Figure 1 shows the distribution of average road roughness across districts in two years of our sample (1990 and 2000); this is equal to $(-1) \times \text{Road Quality}_{dt}$. The graph displays large variation in roughness over space and time, and it also shows a significant leftward shift in the distribution of road roughness across districts between the two years. Similarly, Figure 2 documents substantial spatial variation in road improvements over time on Sumatra, Indonesia's 2nd most populous island. Nearly 84 percent of Sumatra's network of national and provincial highways was unpaved in 1990, but this figure fell to 46 percent only a decade later. Similar trends can be seen for the highway networks on Java and Sulawesi.[9] Finally, Figure 3 shows that the distribution of roughness across road segments narrowed substantially between 1990 and 2007, and that highway quality has periods of both improvements and deterioration across the whole distribution.

Indonesia Family Life Survey (IFLS). Our main source of data for individual employment and household-level consumption outcomes is the Indonesia Family Life Survey (IFLS). The IFLS is a national longitudinal survey, representative of 83 percent of Indonesia's population, and it tracks more than 30,000 individuals in 5 waves over a 22 year period (1993-2015). These individuals are observed in more than 300 communities (*desa* or *kelurahan*), which are the lowest administrative unit in Indonesia and comprise one of our main spatial units of analysis.[10] IFLS communities are spread across 13 of Indonesia's 27 provinces and are located in over 200 districts (*kabupaten*). We use data from the first four waves of the IFLS (1993, 1997, 2000, and 2007), matching the timing of our data on road quality. The IFLS is notable for its low attrition rate, as more than 87 percent of the original set of households were tracked through the first four survey waves. The panel data allow us to track the same households and individuals facing different road infrastructure conditions over 14 years. Appendix Figure A.3 shows the locations of IFLS villages used in our analysis.

District Road Quality and Stated Travel Times from the IFLS. In the community module of the IFLS, survey enumerators asked community informants questions about typical travel times between their village and the nearest district or provincial capital.[11] To verify that changes in district-level average road quality are associated with changes in travel times, we measure the correlation between both series using a two-way fixed effects regression. In the regression, both road quality and travel times are measured in logs, and we condition on village and survey-wave fixed effects.

Row 1 of Appendix Table A.1 shows a least squares elasticity that implies that a 1 percent increase in road quality reduces perceived travel times to the nearest provincial capital by 0.18 percent. Row 2 also shows a negative relationship between road quality and stated travel times to the nearest provincial

[8] Note that Indonesia's district boundaries are not stable during our sample period. Especially after decentralization, many subdistricts split off from their original districts, and new districts were created. The number of districts increased from 302 in 1999 to 514 in 2014, through a process known as *pemekaran* or blossoming (Bazzi and Gudgeon, 2021). Because of this, in order to achieve a consistent geographic unit of analysis, we aggregate all data back to 1990 district definitions.

[9] Appendix Figure A.1 shows the evolution of road quality in Java, and Appendix Figure A.2 shows the evolution of road quality in Sulawesi.

[10] According to Census data, the communities in our sample had an average population of 3,100 in 2000 and 3,145 in 2010.

[11] In the early waves of the IFLS, community informants were mostly village heads. In later waves, local leaders also served as community informants if village heads were not available. These included school principals, health professionals, religious leaders, or local community organizers.

capital, but the point estimate is not significant.

In Panel B of Appendix Table A.1, we run the same regressions but instead use travel times derived from the road roughness data. To construct these variables, we first selected the provincial (or district) capital that was closest to each IFLS community, based on crow-flies distance. We then calculated travel times between location pairs based on the continuous roughness data. We again find that road quality improvements reduce travel times, and the magnitudes of the coefficients are larger than in Panel A.[12] These results provide a "smell test" that our measure of road quality is correlated with perceived travel times by IFLS respondents and explains variation in travel times to typical destinations.[13]

Census of Manufacturing Firms. Our primary data source of firm-level outcomes is the Annual Census of Manufacturing Establishments (*Survei Tahunan Perusahaan Industri Pengolahan*, or SI), collected by Indonesia's Central Statistical Agency, (*Badan Pusat Statistik* or BPS). The SI covers manufacturing plants with more than 20 employees and contains detailed information on plants' cost variables, employment sizes, and measures of value added. SI data also contain firm-level identifiers, enabling us to track changes in firm-level outcomes over time. The data also record information on plants' starting dates, locations at the district level, as well as firm-level outcomes, such as employment, wage rates, value added, and output.[14]

Other Datasets. In addition to our main analysis datasets, we also make use of extracts of Indonesia's 2000 Population Census from the Integrated Public Use Microdata Series (IPUMS). These data record individuals' current districts of residence, birth districts, districts where individuals resided 5 years ago, and other socio-demographic characteristics. We also supplement the 2000 Population Census data with earlier IPUMS waves from the 1971, 1980, and 1990 censuses. We use these censuses to quantify migration responses to changes in road quality.

Finally, our analysis relies on administrative boundary shapefiles from BPS that identify district borders. Because district boundaries have changed substantially over time (Booth, 2011), we assign all observations to their districts in 1990. We also use these boundaries in combination with data from the Harmonized World Soil Database (HWSD) to construct several basic topographic characteristics (e.g., area, ruggedness, slope, and elevation).

[12] See Appendix B for more details on the mapping between road roughness and travel speeds. Note that the regression relationships in Panel B of Appendix Table A.1, Panel B are not mechanical. For instance, if the government had upgraded unimportant roads that did not connect IFLS villages to provincial or district capitals, these specifications would show no impact.

[13] Our estimates of the elasticity of travel times with respect to roughness can also be compared to similar estimates from Currier et al. (2023) who use roughness measures derived from Uber data in the United States. They find a semi-elasticity of speeds with respect to roughness z-scores for arterial roads of -0.109 and highways of -0.070 (Table 8, columns 8 and 9). At the median roughness of 5.92 in our sample, the implied semi-elasticity of speed with respect to roughness from Table A.1, Panel B is -0.087, slightly higher but very similar to the highway elasticity reported in Currier et al. (2023).

[14] New firms are counted when they appear in the dataset having never appeared before. For the purpose of our analysis, we dropped all firms coded as state-owned enterprises (less than 3 percent of all firm-year observations). Throughout this article, we use plants and firms interchangeably since less than 5% of plants in the dataset are operated by multi-plant firms (Blalock and Gertler, 2008).

4 Background on Road Maintenance

During the colonial period, the Dutch built most of Indonesia's current road network.[15] After independence in 1945, roads deteriorated substantially until 1967, when President Suharto assumed power. Road rehabilitation then became a top priority, and quantitative improvement targets were included in multiple national five-year development plans (*Rencana Pembangunan Lima Tahun*, or Repelita). Spending on roads increased rapidly until the late 1970s, then slowed in response to the collapse of state oil revenues and remained stagnant during the 1980s.

In the early 1990s, road upgrading again became a priority. During Repelita IV (1984-1989), the total budget for road improvements was $2.1 billion, but in Repelita V (1989-1994) this budget was increased by 84 percent, to $3.9 billion (in constant 2000 U.S. dollars). Transportation investments were the single largest item of the development budget during Repelita V, forming nearly 18 percent of total planned development expenditures. Almost all resources were allocated to improving the existing road network, especially upgrading dirt roads to asphalt, instead of for new construction.[16] Although policymakers planned to maintain a high level of transport investments during Repelita VI (1994-1999), the Asian financial crisis and its concurrent political upheaval resulted in lower spending than originally intended. Road expenditures have experienced a slow recovery ever since (World Bank, 2012).

Two Stage Budgeting Process for National and Provincial Highways. Our empirical analysis focuses on road quality for two types of roads: (i) national highways; and (ii) provincial highways.[17] Every province in our dataset has two road authorities managing national and provincial highways, respectively. Although the funding processes are separate, in both cases the central government allocates an annual budget to provincial agencies who are then responsible for using that budget to upgrade road segments of their choice under their jurisdiction.[18]

Road maintenance funds are distributed to provincial road agencies based on national formulas. While these formulas can change over time, such changes are made in the capital with no local inputs, nor are there negotiations with local governments over allocation totals (Crane, 1995). Moreover, the variables used as inputs to the distribution formulas are measured with long lags (Shah et al., 1994). This is important for our identification strategy, since it implies that contemporaneous economic activity does not affect the distribution of funds.[19] Note that we neither observe the allocation formulas directly nor how they changed over time, but below, we do verify our identification assumption that funding for national and provincial highway upgrades is not responsive to lagged provincial economic conditions.

Prior to decentralization in 1998, the central government allocated provincial road budgets primarily through the *INPRES Jalan Propinsi* program. Formulas for *INPRES* grants were based on variables such

[15]Especially on Java, transport networks constructed by the Dutch were considered high quality by regional standards. By 1900, Java already had "a sophisticated agro-industrial economy integrated by overlapping networks of telegraphs, telephones, railways, and narrow-gauge tramways and good roads. Nowhere in Southeast Asia could boast better infrastructure. Elsewhere in East Asia, only Japan could compare" (Dick, 2000).

[16]Some new road investments, such as the Trans-Java Expressway, were also planned in the early 1990's under Suharto, but they were mired in delays during our study period. Difficulties in acquiring land and reduced state power to enforce eminent domain made new projects difficult to implement (Davidson, 2010).

[17]We ignore local and collector roads managed by district-level authorities in our analysis.

[18]Maintenance and upgrading of national highways is primarily the responsibility of *Kantor Wilayah*, or *Kanwil* offices of the Directorate General of Highway Development (Leigland, 1993). Provincial public works agencies (*Dinas Pekerajaan Umum*, or *Dinas*) are responsible for maintaining and upgrading provincial highways (Leigland, 1993; Lewis, 2017).

[19]Leduc and Wilson (2013) make a similar point in their study of Federal highway grants to states in the U.S.

as the total length of the road network and density of roads, but as explained above, all these variables were measured with substantial lags and were not sensitive to local economic activity (Shah et al., 1994). In 1998, the INPRES system was replaced by general allocation fund grants (*Dana Alokasi Umum*, or DAU) and special allocation fund grants (*Dana Alokasi Khusus*, or DAK) (Lewis, 2001). Crucially for our identification strategy, both types of grants were again allocated to provincial units based on national formulas.[20]

We also note that direct central government spending on local infrastructure improvements is rare. In fact, Lewis and Chakeri (2004) explain that such spending is illegal according to Law 25/1999 on the Financial Balance between the Center and Regional Governments.

Project Execution. Another important aspect for our identification strategy is the delay between provincial budget allocations and project execution. In Appendix Table A.2, we study the relationship between the log total provincial budget for highway spending from the Ministry of Public Works and the log total kilometers of (national and provincial) roads upgraded, from the IRMS data. For this analysis, we hand collected financial data from reports on file at the library of the Ministry of Public Works. Historical budget data were only available for 4 years: 1993, 1994, 1996, and 2000. Despite this data limitation, in Appendix Table A.2, column 1, we see that the log total kilometers of roads upgraded at time t (our dependent variable) is neither related to the total official budget in year t (Panel A), nor to the total official budget for road improvements in year t (Panel B). Instead, in column 2, we see a positive and significant relationship between current road upgrades and 1 year lags of the provincial budget, with an elasticity of around 0.6.[21] Column 3 shows an insignificant relationship between budgets in t−2 and upgrades at time t, which suggests that most upgrades take place within a single year. This is as expected, since local governments in Indonesia do not rely on capital markets to finance infrastructure investments. They instead finance capital spending entirely out of operating balances (cash and reserves) (Lewis and Oosterman, 2011). Also consistent with our findings above, Ray and Ing (2016) find that even as late as 2015, 93% of the contracts signed by the Ministry of Public Works and Housing are single-year.

While we do not observe annual financial budget data for road maintenance by province, the sparse data available we obtained does point to a tight, one-year lag between financial budgets allocated in year t−1 and road maintenance executed in year t. Thus, our main analysis relies on using a proxy for total spending on roads in province p in year t−1, denoted by $\overline{B}_{p,t-1}$. In practice, our proxy for financial budgets consists of two separate measures of the total kilometers of roads upgraded between year t−1 and year t in province p, distinguishing between national and provincial roads. We describe how we construct these budget proxies in more detail below.

Reliance of Local Governments on Central Transfers. Unlike the U.S., where state tax revenues play an important role in infrastructure financing, provincial governments in Indonesia rely much more heavily on central transfers to finance spending on infrastructure. Before decentralization, local tax rates were equalized everywhere, and local governments had limited autonomy in their revenue raising policies (Hill, 1998). Even after decentralization, the central government maintained control of all major tax bases and the bulk of local government revenues came from central transfers.[22]

[20] The criteria and weights for the DAK over some available years are shown in Appendix Figure A.6.
[21] Appendix Figure A.4 shows the residual-on-residual plots behind these regression relationships.
[22] Appendix Figure A.9 shows the growth in revenues and expenditures for road maintenance at a national and regional level

For instance, Fane (2003) uses Ministry of Finance data to show that in FY 2002, local governments generated just under 5 percent of their total revenue from own-tax and non-tax sources, on average, with the central government accounting for the remaining 95 percent. By the end of 2007, sub-national governments accounted for 38 percent of total public sector expenditure but only about 8 percent of total public revenue (Lewis and Oosterman, 2009; Lewis, 2010). This fact alleviates concerns that local economic activity feeds back into road quality via local tax revenues for infrastructure maintenance. If there is any scope for local spending on roads to be responsive to local economic activity, it would only occur in provinces that are large oil and gas producers, and we explore this possibility in robustness checks.

Fixed Administrative Authorities for Road Segments. A final aspect of Indonesia's road system that is useful for our analysis is that, over the period we study, the administrative authority responsible for different road segments remains mostly unchanged. Appendix Figure A.10 shows that in our sample of roads, provincial roads accounted for 52 percent of total national and provincial road length, a figure that remained completely stable over time. This eliminates concerns that road agencies with more resources might be taking over road segments from less well funded authorities and interfering with our research design.

Summary. In this section, we have argued that in Indonesia, road maintenance decisions can be thought of as following a two-stage budgeting procedure (Deaton and Muellbauer, 1980). In the first stage, the central government transfers funds to provincial authorities using pre-determined allocation formulas. These formulas are based on variables that either do not change over time or only do so over very long horizons, and we show that provincial allocations in year t−1 are only positively related to maintenance investments in year t. We also explain that local governments rely on central transfers to conduct infrastructure spending. They seldom borrow to finance road maintenance, and most road improvement contracts are single year. Taken together, these facts imply that recent economic dynamics are decoupled from the funding that provincial authorities receive for road improvements, a fact we directly test below.

5 Empirical Strategy

In this section, we explain how to use Indonesia's two-stage budgeting process to construct instruments that enable us to identify the causal effect of a marginal improvement in road quality. We first describe our basic regression model without instrumentation at the district level:

$$y_{dt} = \alpha_d + \alpha_t + \beta \log \text{Road Quality}_{dt} + \mathbf{x}'_{dt}\theta + \varepsilon_{dt} \ , \quad (4)$$

where y_{dt} is an outcome for district d at time t, and $\log \text{Road Quality}_{dt}$ is the log average road quality measure, defined in equation (3). We include district fixed effects, denoted by α_d, to control for time-invariant unobservables that may be correlated with road quality and outcomes. Year fixed effects, denoted by α_t, control for year-specific national factors that affect outcomes in all districts. The vector

over time. Until 2000, almost all of the financing came from the center, after which the regional public budget share rose only very modestly (Green, 2005; World Bank, 2012).

$\mathbf{x}_{dt}$ represents a set of time-varying controls, including for example, controls for other infrastructure projects. In some specifications, we estimate a regression model similar to equation (4) at the individual, household, or community level; in those cases, we include corresponding control variables and their respective cross-sectional fixed effects. When y_{dt} is measured in logs, the key parameter of interest, β, measures the elasticity of y with respect to road quality.

Causal estimates of the effects of road improvements are challenging to obtain because maintenance decisions are not exogenous, and the sign of the selection bias is difficult to ascertain a-priori. For example, areas that receive greater maintenance investments may be selected by policymakers, either to target rapid economic growth, or to stimulate growth in lagging regions. On the other hand, if better roads increase local economic activity, this feedback may generate attenuation bias if roads deteriorate faster from more extensive use. These endogeneity concerns are not solved with time and location fixed effects.

As we describe above in Section 4, the two-stage budgeting process for financing road maintenance is helpful for identification. Let $p = 1, ..., P$ denote different provinces in Indonesia, and let $d = 1, ..., N_p$ index the districts that comprise province p. In the first stage of the budgeting process, in year t–1, the national government allocates a total budget for maintaining national roads (N) and provincial roads (P) in province p. Let the $\overline{B}^N_{pt-1}$ and $\overline{B}^P_{pt-1}$ denote the log of these budget totals, respectively.

In the second stage, provincial authorities decide how to allocate $\overline{B}^N_{pt-1}$ and $\overline{B}^P_{pt-1}$ to upgrade the national and provincial roads in different districts under their jurisdiction. Although this second stage is clearly endogenous, we argue that the first stage funding is not. This is because provincial budgets are determined by allocation formulas, those formulas depend on factors that are measured with lags, and year t–1 provincial allocations affect road maintenance investments only after budgeted projects are executed in year t. The combination of formula-based financing and lags in project executions help to break the simultaneity problem and eliminate the usual concern that current economic activity influences the formula-based funding that the province receives. As a result, we can use $\overline{B}^N_{pt-1}$ and $\overline{B}^P_{pt-1}$ as instruments for log road quality in district d in year t.

Budget Proxies. Because we do not observe each province's financial budgets every year, we proxy for them based on the total kilometers of roads upgraded in each province, measured using the road roughness data.[23] Let $L \in \{N, P\}$ index road maintenance authorities (e.g. National, N, or Provincial, P), let r index road segments, and let $\mathcal{R}^L_p$ denote the set of road segments in province p under maintenance authority L. We classify a road segment r as upgraded in year t if its roughness declines in year t, i.e.: $U_{rt} = \mathbf{1}\{\text{IRI}_{rt} < \text{IRI}_{rt-1}\}$. Using these upgrading indicators, we proxy for the total budget for roads allocated to province p in year t–1 as follows:

$$\widetilde{B}^L_{pt-1} = \log\left(1 + \sum_{r \in \mathcal{R}^L_p} \text{len}_r \times U_{rt} \times (\text{IRI}_{rt-1} - \text{IRI}_{rt})\right) , \tag{5}$$

where len_r denotes the length of segment r. In words, the budget proxy for road improvements made by

[23] Financial data from developing countries with low state capacity may not be as informative about actual road improvements as one would like given corruption and monitoring issues (Olken, 2007). In this sense, our $\overline{B}_{t-1}$ measures can be thought of as effective road budgets net of corruption and administration inefficiencies.

province p's administrative authority L in year $t-1$, $\widetilde{B}^L_{p,t-1}$, equals the log of one plus the total kilometers of roads upgraded in year t and province p that are administered by that authority, weighted by the change in roughness for each road segment. This implies that larger quality improvements represent a larger financial budget spent improving roads.

Table A.3 reports the first stage relationship between our budget proxies and road quality across different units of analysis and shows that our budget proxies predict road quality at different levels of disaggregation. In columns 1 and 2, we see that at the kilometer post level, increasing the provincial budget for national and provincial roads improves road quality, and our first stage F-stat is large, between 31 and 158. Column 3 reports the district-year first stage relationship, while columns 4 and 5 report the first stage relationship for individual-waves and household-waves of the IFLS data. In all cases, first stage relationship is similar, and the first stage F-statistics are large, between 111 and 136.[24] Overall, we observe that provincial budgets are good predictors of district level road quality improvements.[25]

Budget Proxies and Local Economic Activity. We argued in Section 4 that because the central government allocates budgets to provinces based on formulas whose inputs are measured with lags, the provincial budget allocations should not be correlated with lagged economic activity. To confirm this, in Table 1, we estimate regressions of the following form:

$$\widetilde{B}^L_{p,t} = \gamma_p + \gamma_t + \beta \mathbf{x}_{p,t-1} + \varepsilon_{pt} \ , \tag{6}$$

where $\widetilde{B}^L_{p,t}$ denotes the log budget proxy defined in (5), γ_p and γ_t are province and year fixed effects, $\mathbf{x}_{p,t-1}$ is a vector of measures of lagged economic activity at the province level, and ε_{pt} is an error term.

In Panel A, the dependent variable is the log of the total number of kilometers of national roads upgraded in province p (i.e. $\widetilde{B}^N_{p,t}$), while in Panel B, the dependent variable is the log of the total number of kilometers of provincial roads upgraded in province p (i.e. $\widetilde{B}^P_{p,t}$). Columns 1-2 regress these provincial budget proxies on lags of log provincial GDP, columns 3-4 include lags of log provincial population, and columns 5-6 include both. In all cases, the estimated effects are cannot be distinguished from zero, the coefficient magnitudes tend not to display regular patterns, and the regression F-statistics are very low (below 2). Moreover, the within-variation explained by these regressions is exceedingly small.

In Appendix Table A.4, we provide further evidence of our identifying assumption using other proxies for provincial economic dynamics: (1) average nighttime light intensity in the province; (2) the share of the province without any nighttime lights; (3) the log of total output of medium and large manufacturing firms in the province (from the SI); and (4) the log of provincial total manufacturing employment (also from the SI). In no case do we find robust, statistically significant relationships between lags of these variables and the current values of the provincial budget proxies. These results provide evidence consistent with our main identification claim: recent provincial level economic and population dynamics are not driving the levels of investments available for road improvements in the province. This is expected given Indonesia's two stage budgeting process explained in section 4.[26]

[24]Opposing signs for the different budget variables are due to the high degree of correlation between the two measures (ρ = 0.88 at the district-year level).

[25]Note that both the province level budgets, $\overline{B}_{pt-1}$, and the proxies we use for them, $\widetilde{B}_{pt-1}$, feature no district level variation within a province-year.

[26]To be conservative, we also include these variables as controls in robustness checks presented below.

6 Results

This section presents reduced-form estimates of the impact of road quality on local economic development outcomes and provides evidence on possible causal pathways behind these effects. We first present our main estimates of the impact of local road quality improvements on individual and household-level consumption, income, and employment outcomes. We then examine how road quality affects large manufacturing firms. Finally, we estimate how road quality affects prices.

Road Quality and Consumption, Income, and Employment. To study the relationship between changes in road quality and individual and household outcomes, we use detailed panel data from the IFLS over 4 survey waves. We estimate household (or individual) fixed effects regressions of the following form:

$$y_{idt} = \alpha_i + \alpha_t + \beta \log \text{Road Quality}_{dt} + \mathbf{x}'_{idt}\theta + \varepsilon_{dt} \ , \tag{7}$$

where α_i denotes a household (or individual) fixed effect and α_t denotes a survey wave fixed effect. For household regression specifications, $\mathbf{x}_{idt}$ includes survey month indicators and controls for household size. In the individual-level regression specifications, we also add time-varying controls for individual age and years of schooling.

In the first row of Table 2, Panel A, we present our estimate of the effect of road quality where the dependent variable, y_{idt}, is log per-capita household consumption expenditures. Because equation (7) is a log-log specification, the parameter of interest, β, can be interpreted as the average elasticity of household consumption with respect to road quality. Column 1 reports fixed effects least squares (FELS) estimates of β, while column 2 reports our IV specification, using the national and province budget proxies as IVs. The corresponding Kleibergen-Paap Wald Rank F statistic is listed next to column 2. Finally, the last two columns report means of the dependent variables and sample sizes. Robust standard errors, clustered at the community level, are reported in parentheses.

Row 1 of Table 2 shows a least squares elasticity that implies that a 1 percent increase in road quality increase per-capita consumption by 0.12 percent. This elasticity increases to 0.28 in the IV specification, and it is significant at the 1 percent level. In the next two rows, we examine how road quality impacts household businesses, distinguishing between farm and non-farm profits. We report positive effects on both farm and non-farm profits, but only the latter are significant, and the impacts are quite small. These two results suggest that road quality improves consumption expenditures, but despite positive estimates for farm and non-farm business profits, the effects on those variables are not large enough to explain the increases in consumption we observe.

In Panel B, we examine whether the increased consumption expenditures result from changes in labor income. Individual-level fixed effects regressions show that the elasticity of labor earnings with respect to road quality is 0.24, which closely tracks the increase in per capita expenditures from Panel A. In the next set of rows for Panel B, we examine the impact of road quality improvements on hours worked and employment outcomes. We first show that road quality improvements do not have any impact on the probability of being employed (row 5), nor do they affect the total number of hours worked (row 6). However, in the final rows of Table 2, we find significant impacts of road quality on the sector of employment.

Using the IFLS employment module, we first assign every employed worker into one of three different employment sectors: (1) formal agricultural employment, which includes wage-earning agricultural labor; (2) other formal employment, which includes manufacturing; and (3) informal employment, which includes both informal agricultural employment and also other informal jobs.[27] Rows 7-9 in Table 2 show that improving road quality slightly reduces agricultural employment (row 7), although the effect is not significant. We also find that the elasticity of formal employment to road quality is 0.17 (row 8). Mirroring this increase in formal employment, we find a similarly-sized reduction in the probability of working in the informal sector (row 9).

Taken together, the findings from Table 2 suggest that the positive consumption benefits and positive total earnings effects of road quality are due to workers moving out of low wage jobs in agriculture and the informal sector and into higher wage, formal-sector jobs. Indeed, we find that improved road quality does not affect hours worked (row 3) but it increases total earnings (row 2). These results on sector switching in Indonesia explain one mechanism through which the growth of non-farm factory employment can be a source of wage gains for workers in rural areas, as emphasized by Foster and Rosenzweig (2004) using data from India. They are also consistent with road quality playing a crucial role in structural transformation and local economic development (Lewis, 1954).[28]

Table 2 shows that across specifications, the Kleibergen and Paap (2006) Wald Rank F statistics, which generalize the first-stage F-statistic for multiple instrumental variables, are large. In Appendix Table A.6, we report more diagnostics tests for these regressions for the "other informal employment" outcome. Both the Kleibergen-Paap LM tests and the Anderson-Rubin (AR) tests strongly reject the null of weak instruments of the endogenous road quality variable. The Sargan-Hansen J test statistic for overidentifying restrictions is also small, and we cannot reject the null that the instruments are correctly excluded from the estimation equation.

Because our analysis studies the effects of local road quality improvements on a large set of outcomes, we need to account for multiple hypothesis testing. In all tables, after reporting conventional clustered standard errors, we also report two-stage false-discovery rate (FDR) sharpened q-values in brackets (Benjamini et al., 2006; Anderson, 2008). These q-values represent adjustments to p-values that account for the multiple hypothesis tests we run in each table. The results in Table 2 confirm that the significance of our estimates is robust to multiple testing concerns.

Road Quality and District-Level Manufacturing Outcomes. To what extent are the employment effects of road quality driven by the increased presence of manufacturing firms? To investigate this, we begin by creating district-year aggregates of the individual firm-level data from the SI. We then use these district-year variables in a panel regression specification, similar to (7), where we control for district and year fixed effects. Standard errors are clustered at the district level.

Columns 1 and 2 of Table 3 show the results of estimating an overall β for an average district. In Panel A, we focus on how road quality impacts the quantity of large manufacturing firms in the district. From the IV specifications in row 1, we find that the elasticity of firm openings to road quality is 1.2. The fact that our IV estimates of β are larger than the FELS estimates suggests that naive estimates of treatment effects of the impact of road quality may suffer either from negative targeting bias (e.g. policymakers

[27] Following Comola and De Mello (2011) and BPS, we define informal employment as workers who are self-employed or who are family/unpaid workers that work in a household business.

[28] Appendix Table A.5 confirms that our results on the full IFLS sample are similar to results if we just focus on the sample of households that do not move to different locations over the 1993-2007 period.

upgraded roads in less developed areas) or from negative feedback (e.g. faster growing areas had greater road deterioration).

In row 2, we find no evidence that road improvements are associated with changes in firm closures. Finally, in row 3, we show that the elasticity of the number of firms to road quality is 0.3. In summary, the results from Table 3, Panel A show that road quality improvements lead to increases in the number of firms in the district, and this effect is driven by new firms instead of by firm closures.[29] The Kleibergen-Paap Wald Rank F statistics are also large for all outcomes, suggesting that our IV models are well specified.

In Panel B, we study the effect of road improvements on district-level production outcomes. The IV specifications show that a 1 percent increase in road quality leads to a 1.7 percent increase in output (row 4) and a 1.4 percent increase in value added (row 5). We also find positive, but smaller effects of road improvements on the total number of manufacturing workers in large firms in the district. Although we estimate an elasticity of road quality with respect to employment of 0.4 (row 6), an estimate that is similar to the survey-based result, the estimate is not statistically significant. Given the large increases in output and the smaller effects on employment, it is not surprising that we also see positive effects on output per worker; a 1 percent increase in road quality leads to a 1.2 percent increase in district-level output per worker (row 7).

Road Quality and Firm-Level Manufacturing Outcomes. One potential explanation for the manufacturing results from Table 3 is that they reflect intensive-margin improvements of existing firms, instead of new firm creation. To investigate this claim, we exploit the firm-level identifiers in the SI data that allow us to track how input and production outcomes for existing firms change over time in response to road improvements. In Table 4, we use our IV strategy to estimate a firm-level panel specification, similar to (7), where we include both firm and year-fixed effects. Because we do not observe firms that move in our sample, the firm-fixed effects also control for any time-invariant, district-specific characteristics. We estimate this regression using more than 275,000 firm-year observations over the 1990-2007 period. Standard errors are clustered at the district level, as before.

Table 4 shows results on firm-level output (row 1), value added (row 2), total employment (row 3), and output per worker (row 4), mirroring the outcomes listed in Table 3, Panel B. Although our Kleibergen-Paap Wald Rank F statistics fall substantially, we find that overall, road improvements had no significant impact on any outcomes for existing firms. Taken together with evidence from Table 3, our results suggest that road quality improvements had modest impacts on the extensive margin of firm creation, but they also had no significant effects on production or employment outcomes for preexisting firms. This suggests that the sector switching effects of road improvements are most likely due to newly created manufacturing firms.

Road Quality and Prices. To study how road quality improvements affect prices, we use our IV strategy with community-level price data from the IFLS to estimate two-way fixed effects regressions, similar to (7), but now with separate community and year fixed effects. Table 5 shows our results. In row 1, we show that local road improvements are associated with positive increases in factory wages,

[29] Appendix Table A.7 shows estimates of the number of new firms by 2-digit product code. We find that road quality improvements seem to be associated with increases across most industries, with the largest effects on food and beverage processors and textile firms.

but this effect is not statistically significant. Row 2 shows that the impact on farm wages is positive but also not significant.

In rows 3 and 4, we study the effect of local road quality on food prices, an important component of consumer welfare, particularly for lower-income households. The food price measures we use are Laspeyres price indices composed of perishable goods (including meat and fish) and non-perishable traded foods (including rice, oil, sugar, and salt). We use initial consumption values for expenditure weights. In row 3, we show a significant negative relationship between road quality and perishable prices. A 1 percent increase in road quality reduces perishable food prices by nearly 1 percent. Row 4 shows that road quality has a small, insignificant, but positive impact on the prices of non-perishable goods.

Finally, in the last two rows of the table, we study the relationship between road quality and housing prices, using estimates of log land values and log rents from hedonic specifications. In the first step, we estimate a hedonic price regression of log rents or log land values on a large vector of household and plot characteristics, in addition to controlling for fixed effects at the community-by-wave level.[30] In the second step, we use the estimated community-by-wave fixed effects from the first-step regressions as the dependent variable in rows 5 and 6. We find significant, positive elasticities of both land values and rents to road quality that are both around 0.5.[31] This result is as expected for a policy that makes an area more attractive to firms, which bids up the cost of land and housing. In Section 8 below, we take this higher cost of living effect into account when calculating overall welfare effects.

7 Robustness and Mechanisms

In this section, we first demonstrate that our results on the reduced form relationships between road quality and different economic outcomes are robust to variations in how we construct instruments. Next, we show that our results are robust to including different controls, to different sample splits, and to different ways of conducting inference. Finally, we investigate several mechanisms that could explain our findings.

Instrument Robustness. Appendix Table A.10 shows that our main results are robust to different ways of constructing instruments. Columns 1 and 2 repeat our baseline FELS and provincial budget IV estimates. One concern with these instruments is that because the budget variables directly depend on a district's road quality investments, there might be a mechanical relationship between our IVs and the variable we instrument.

In column 3, we use leave-one out budget IVs, where we construct budget proxies for district i's province based on all roads upgraded in the same province but outside of district i. Results are robust to this change. In column 4, we use our original instruments, but we first residualize them, stripping out the effects of 3 lags of provincial GDP and population. Column 5 does the same residualization

[30] The hedonic regression includes the following controls: (1) indicators for dwelling type; (2) separate indicators for whether the house is surrounded by human or animal waste, piles of trash, or stagnant water; (3) an indicator for whether the house is owned or rented; (4) the number of rooms in the house; (5) indicators for the types of floor, outer walls, and roof; and (6) indicators for electricity access, piped water, type of water used for cooking. Estimates of the hedonic relationships between these variables and rents and land values can be found in Appendix Table A.8.

[31] We also tried specifications using the community's median land value and median log rent, instead of relying on the hedonic approach. Overall, these specifications produced similar results, which can be found in Appendix Table A.9.

procedure but does so using the leave-one-out budget IVs. In column 6, we revert back to our baseline IV, but instead of using residualized measures, we control directly for 3 lags of provincial GDP and population. Column 7 includes these additional controls but instead uses the leave-one-out budget IVs. Overall, Appendix Table A.10 shows that across all outcomes and different units of analysis, our main results are robust to these permutations.

Controls and Sample Splits. Next, Appendix Table A.11 shows that our results are also robust to different sample splits, as well as to several different time-varying controls. To address concerns that districts' own revenues may be used to finance road maintenance investments, in column 3, we drop districts engaged in any oil and gas production, and in column 4, we drop districts where the share of GDP in the mining sector exceeds 5 percent. Overall, the effects are largely unchanged.

If Indonesia's road improvements were driven by the presence of multinational firms, who might negotiate with local governments to obtain better roads or maintain key roads themselves, then controlling for the presence of FDI could attenuate our effect sizes. In column 5, we return to the full sample of districts, but we add a control for the share of output in the district produced by large foreign-owned manufacturing firms. Introducing this control does little to change significance or effect sizes, suggesting that FDI or the presence of multinationals is not responsible for explaining our results.

Next, in column 6-9, we include other time-varying controls, constructed from multiple waves of *Podes* data, to proxy for changes in other infrastructure that may be correlated with road maintenance investments. Column 6 includes a time-varying control for the share of households connected to the national electricity grid (*Perusahaan Listrik Negara*, or PLN). In column 7, we control for the time-varying share of households with access to a national TV signal. Column 8 includes separate, time-varying controls for the number kindergarten, primary, junior secondary, and senior-secondary schools in the district. Column 9 includes time-varying controls for the number of hospitals, the number of community health clinics (*Puskesmas*), and the number of community based preventive and promotive care facilities (*Posyandu*). Finally, to deal with concerns about different allocation rules or formulas after Suharto, in Column 10, we only estimate effects using data from the post-decentralization period (1999-2007). Overall, the magnitude and significance of our results are robust to each of these different controls and sample splits.

Adjacent Provincial Economic Activity. Even if a district's own provincial economic activity is not driving provincial budget allocations, there could be coordination or strategic investment across multiple provinces. For example, if national highways span multiple provinces, road quality investments in one province could depend on economic activity in other provinces. Appendix Table A.12 explores the potential for this to affect our results. In column 3, we control for 3 lags of log GDP in adjacent provinces. In column 4, we control for 3 lags of log population of adjacent provinces. Overall, our results are robust to these additional controls.

Island and Province Trends. Another concern is that serial correlated unobservables could be driving both provincial budget allocations and economic outcomes. To partly explore this possibility, in Appendix Table A.13, column 3, we control for island-specific time trends, and in column 4, we add province-specific time trends. Our main results on per capita consumption, total earnings, sector switching, and district-level firm openings are robust to these additional controls.

Wild Bootstrap. Our main specifications report robust standard errors that are clustered at the district level. However, because the identifying variation is at the province level, these standard errors may not be conservative. Clustering standard errors at the province level would be preferable, but there are only a small number of provinces in our data. In Appendix Table A.14, we report three sets of p-values for tests of the significance of our main set of results. Column 3 reports our baseline p-values, while Columns 4 and 5 use the Wild-cluster bootstrap at the district and province level (MacKinnon and Webb, 2018). Although the p-values increase under province-level clustering, for our main results, we still tend to reject the null of $\beta = 0$ at conventional significance levels.

Mechanisms: Reallocation. One explanation for our findings is that instead of creating new jobs, road improvements could simply reshuffle activity away from one district to another. If that were the case, our estimates of the welfare effects of road quality improvements would not be true welfare improvements, but would instead reflect welfare gains in one district coming at the expense of welfare losses elsewhere. To investigate the potential for reallocation to explain our findings, in Appendix Table A.15, we estimate how road quality improvements in district d affect manufacturing outcomes in nearby districts. Column 1 reproduces the baseline IV estimates of the effects of road quality from Table 3. In panel A, column 2, we regress the number of new firms (row 1), the number of closed firms (row 2), and the percent change in firms (row 3) in districts within 50 km of district d on road quality in that district.[32] Panel B is structured similarly. Overall, we find no evidence for regional reallocation effects. Importantly, we see no evidence that road quality in district d is positively associated with increased firm closures in nearby districts. This suggests that the manufacturing results reported in Table 3 reflect new firm creation and not reallocation of existing activities to nearby districts.

Mechanisms: Migration. In this paper, we have argued that local road quality represents a productive amenity. A simple spatial equilibrium model would suggest that if migration costs are small, firms and workers would move to upgraded locations in response to increases in road quality. Greater in-migration should unambiguously increase land and housing prices, but the impacts on wages depend on the relative shifts of labor demand and labor supply (Rosen, 1979; Roback, 1982).

To assess the extent to which local road quality improvements impacted internal migration, we use census data from 1990 and 2000 to examine how the number of recent migrants at the district-level (i.e. those arriving within the last 5 years) was impacted by changes in road roughness. Our empirical specification is the following:

$$y_d = \alpha + \beta \Delta \log \text{Road Quality}_d + \mathbf{x}_d' \theta + \varepsilon_d \ , \tag{8}$$

where y_d is a cross-sectional migration outcome, α is a constant, $\Delta \log \text{Road Quality}_d$ is the log difference in road quality between 1995 and 2000, and $\mathbf{x}_d$ is a vector of additional controls, including logs of population and GDP in 1990. In this specification, instead of instrumenting road quality in levels, we instrument the change in road quality between 1995 and 2000 with the same IV strategy as above, but we also incorporate lags of the provincial budget shifters, because we do not observe precisely when people migrate over the previous 5 year period.

The first row of Table 6 shows that districts with improved road quality are associated with a positive

[32] We use centroid distance to define this distance cutoff.

rate of population growth, but the effect is not statistically significant. In the next two rows, we regress a district's log total number of recent migrants from different districts and different provinces (in 2000) on the change in road quality between 1995 and 2000. We find that a 1 percent increase in road quality growth leads to a 2.2 percent increase in the number of district migrants and a 2.8 percent increase in the number of province migrants (note that these categories are not mutually exclusive).

Despite the positive and statistically significant estimates of the effect of road quality on migration, the effect sizes are quite modest. At the average number of district migrants, a 1 percent increase in road quality would lead to roughly an additional 840 district migrants. The average district has a population of roughly 730,000 in 2000, so this increase would represent nearly one tenth of one percent of the population. Although these cross-sectional long-difference estimates are less well identified than the panel specifications used in the rest of the paper, they nevertheless provide evidence that road quality improvements lead to positive, statistically significant increases in migration. The fact that the migratory response is small reinforces our choice to focus on non-migrant households when approximating the welfare effects of road improvements in the next section.

In summary, using our IV strategy, we find that the effects of road quality are robust to a number of specification concerns, including using different instruments, different treatment measures, controls for other changes in infrastructure, different cuts of the sample, and different ways of conducting inference. We also find that reallocation cannot explain our results, and that migratory responses to road quality are positive, as expected, but small in relation to district population.

8 Welfare Analysis and Counterfactuals

In this section, we combine the reduced form estimates presented in Section 6 with our welfare decomposition formula, given by equation (2), to provide an overall welfare estimate. This exercise sheds light on the relative importance of different channels through which marginal changes in road quality affect household welfare. Finally, we use this framework to conduct counterfactual policy exercises.

We begin this section by describing how we calibrate key components of the welfare formula: (1) the share of food, non-food, and housing in the budgets of consumers; and (2) the share of labor income and profits in total household income. We then present our welfare decomposition results. Finally, we discuss counterfactual simulations.

Expenditure Shares. We use data from the first wave of the IFLS to measure households' expenditure shares in four separate product categories: (1) housing; (2) perishable foods; (3) non-perishable foods; and (4) non-food consumption goods. Appendix Table A.16 shows estimates of housing expenditure shares by quintile of total household expenditure. This table reports relatively low housing expenditure shares, with the lowest quintile of the distribution (Q1) devoting approximately 11 percent of their total expenditures to housing. As expected, this share falls as households grow wealthier.

Note that 20 percent of households in IFLS-1 did not report owning their home. For those households, we interpret the increase in housing prices associated with road improvements as costly, to the extent that it increases rental payments. However, for homeowners who constitute 80 percent of the sample, a similar increase in housing prices could actually increase welfare, as it could lead to greater borrowing and consumption through housing wealth effects (e.g. Gonzalez-Navarro and Quintana-Domeque, 2016;

Paiella and Pistaferri, 2017). In order to provide conservative estimates, in our benchmark calculation we ignore home ownership effects and assume that housing price increases do not impact welfare for home owners.[33] Because we ignore the potential for housing price increases to positively improve welfare, our estimates provide a lower bound on the true welfare impact of road quality.

Appendix Table A.17 reports the share of non-housing expenditures on perishable foods, non-perishable goods, and non-food products. The lowest quintile of households in IFLS-1 spent about 10 percent of non-housing expenditures on perishable goods, 58 percent on non-perishable goods, and 32 percent on non-food goods. As households grow wealthier, they spend relatively less on food, and they increase their non-food consumption.

To bring these expenditure shares to the welfare decomposition formula, equation (2), we set the values of α and $\{\alpha_k\}_{k=1}^{K}$ for each household equal to their expenditure shares, taking values from Appendix Tables A.16 and A.17 corresponding to their total expenditure quintile.

Income Shares and Road Quality Elasticities. To further calibrate equation (2), we use initial values of Y equal to the household's total earnings from labor income and self-owned farm and non-farm business profits. These initial values are taken from the IFLS-1 survey round.

Recall from Table 3 that we found that road quality improvements lead to a small increase in the number of new large manufacturing firms. If those large manufacturing firms were locally owned, their profits should show up in household income through profits. If not, we ignore their effects on welfare, and this again could cause us to understate the welfare gains from better road quality.[34]

Finally, to finish calibrating (2), we use constant average road quality elasticities to measure $\mathcal{E}_{Y_L,A}$, $\mathcal{E}_{\pi,A}$, $\mathcal{E}_{p_H,A}$, and $\mathcal{E}_{p_j,A}$ for $j = 1, ..., J$. In doing so, we also assume that the elasticity of road quality with respect to non-perishable food goods is the same as the elasticity of road quality with respect to non-food goods. However, we need to confront the fact that our estimates of these elasticities are subject to sampling error. To construct confidence intervals that take this sampling error into account, we use a parametric bootstrap procedure, following Horowitz (2001) and Atkin et al. (2018). In each replication, we take a random sample (with replacement) from the 6,567 households in IFLS-1. We redraw each road quality elasticity parameter from a normal distribution, with the mean equal to that elasticity's point estimate and the standard deviation equal to that elasticity's standard error, and we calculate the components of equation (2). We repeat this procedure for 1,000 bootstrapped replications.

Results. Table 7 shows the results of calculating equation (2) for the 6,567 households in IFLS-1. Row 1 of the table shows that the elasticity of household welfare with respect to road quality is 0.1 on average. Since the median upgrade in the data increases road quality by 5 percent, this elasticity means that it would be associated with a total welfare increase of 0.5 percent. When decomposing different channels in the rows below, we see that the positive average effects on welfare come both through impacts of road quality on wage labor income and on business profits, but given the small elasticities, the impact of the latter is quite small. There are also positive effects that owe to lower perishable food prices. Greater housing costs associated with increased road quality also reduce average welfare, but only for the 20 percent of households that are not home owners. Non-perishables and non-food prices contribute to a

[33] In emerging economies, this sort of housing wealth effect may be attenuated by imperfect access to credit markets, particularly for lower and middle-income earners.

[34] Given that these are large firms, it is probable that they are owned by residents who live elsewhere.

rise in the cost of living, but not by enough to swamp the positive effects on labor income.

Figure 4 plots estimates of the distribution of the welfare effects of road quality for each quintile of the baseline distribution of household consumption expenditures. We find similar effects across the expenditure distribution, although there is some suggestive evidence that road quality improvements benefit higher income households more than lower income households. This is because lower income households have a larger share of housing in their budget, and road quality moderately increases housing costs.

Policy Simulations: Stimulus Effects of Paving Roads. Finally, we investigate the extent to which road upgrades can act as a cost-effective local stimulus program. We conduct counterfactual simulations for each district, where we assume that all roads in the district are improved to a minimum IRI of 5.35, which is the median IRI for asphalt roads in the road quality data. We suppose that upgrades take place in period $t = 0$, which is equivalent to the year 1990 in our data.

For each period $t = 0, 1, ..., T$, we calculate the benefit of road upgrades as follows:

$$B_{dt} = 0.095 \times GDP_d \times \%\Delta RQ_{dt}^{CF} ,$$

where 0.095 is the average elasticity of welfare with respect to road quality from Table 7, GDP_d is district d's GDP in 1990 (converted to 2000 USD), and $\%\Delta RQ_{dt}^{CF}$ is the counterfactual percent change in road quality in simulation period t:

$$\%\Delta RQ_{dt}^{CF} = \left(\frac{RQ_{dt} - RQ_{d,0}}{RQ_{d,0}} \right) .$$

In the first year ($t = 0$), $\%\Delta RQ_{dt}^{CF}$ is driven purely by upgrading roads in the district. However, in subsequent years, those upgraded roads deteriorate. We estimate road deterioration using a regression of log road roughness on years since upgrading at the kilometer-post-interval level (k):

$$\log \text{IRI}_{kt} = \alpha_k + \alpha_t + \beta \, \text{Years Since Upgrade}_{kt} + \varepsilon_{kt} .$$

We estimate this relationship separately for paved and unpaved roads. Results are shown in Appendix Table A.18. We find that each additional year since upgrading increases road roughness for paved roads by 6.3 percent and unpaved road roughness by 12 percent. The log-linear relationship also fits the data well, as shown in the residual-on-residual plots in Appendix Figure A.11.

For $t = 1, ..., T$, we simulated the counterfactual change in road quality at the kilometer-post level assuming: (1) no upgrades occur and; (2) roads deteriorate at the fixed rates estimated in Appendix Table A.18. This procedure generates counterfactual average district road quality measures from predictions of the road quality that prevails at each road segment interval over time.

For flat roads, we assume that it would cost $93,350 per kilometer in 2000 USD to upgrade roads in this way. This cost estimate is taken from the World Bank's Road Costs Knowledge System (ROCKS), used by Collier et al. (2016).[35] It is equal to the average cost of a 60-79 mm asphalt overlay across contracts listed in the ROCKS database for Indonesia. To account for topographical variation, we assume that

[35]The World Bank's Road Costs Knowledge System (ROCKS) is a database of road-related projects completed by the World Bank Group. It is designed as a repository that can be used to calculate the average and range of unit costs based on historical data that could improve the reliability of new cost estimates and reduce the risks generated by cost overruns. The World Bank's Transport Unit last updated the data in 2008.

the total cost of road upgrades increases linearly in the slope of roads, following the civil engineering literature (Faber, 2014; Alder, 2023).

Using these measures of benefits and costs, we can calculate the net present value of the upgraded infrastructure projects as follows:

$$NPV_d = \sum_{t=0}^{s_d} \left(\frac{B_{dt} - C_{dt}}{(1+\beta)^t} \right) ,$$

where B_{dt} is defined above, C_{dt} measures the cost of road upgrades, β is the discount rate, set to 0.05, and s_d is defined as the largest period for district d where road quality improvements are still positive (i.e. $\%\Delta RQ^{CF}_{d,s_d} > 0$). We assume that the costs of road maintenance are borne entirely in the first period, $t = 0$, so that $C_{dt} = 0$ for $t > 0$. This is consistent with the pervasive use of single-year contracts by Indonesia's Ministry of Public Works and Housing (Ray and Ing, 2016).

To illustrate, Appendix Figure A.12 shows a plot of discounted $B_{dt} - C_{dt}$ against time for district Labuhan Batu in North Sumatra. In the first period, all of the upgrading costs are incurred, and these large costs do not outweigh the one year benefits of the upgrade. However, after one year elapses, the net present value becomes positive, increasing to over $10 million USD. The cumulative net present value remains positive for 9 years, until road deterioration completely erodes away the benefits of the upgrade.

Figure 5 presents scatterplots of NPV_d across districts in our sample against district road length (Panel A) and district GDP per capita (Panel B). In both plots, NPV_d is expressed as a percentage of district GDP. The median district upgrading project generates a positive NPV of roughly 3 percent of district GDP, and 81 percent of districts had positive NPV projects. Moreover, relative to the cost of upgrades, the median upgrading program would confer benefits equal to 2.3 times their costs. Some districts enjoyed considerably large NPVs relative to their GDPs, particularly urban districts with relatively smaller road lengths and larger values of output per capita. However, 19 percent of districts had negative NPVs; such districts tended to be poorer and have longer road lengths. The median district would also enjoy 6 years of positive stimulus benefits before deterioration erodes road quality back to its initial level, and the interquartile range of benefit length was between 3.5 and 11 years.[36]

Before concluding, we note an important caveat in this analysis. Our NPV calculations are based on the welfare formula, equation (2), which approximates the impact of marginal improvements in road quality on household welfare. However, our counterfactual exercise of paving roads involves potentially non-marginal upgrades to road quality. While our approach captures how small improvements in road quality can lead to income and local price increases through local equilibrium adjustments, it may not capture the welfare and equilibrium effects of larger-scale policy changes. Such significant improvements in road quality could affect margins we do not model such as migration decisions, or impact dynamic factors—like housing wealth effects that might enable increased borrowing for consumption or investment financing—all of which we ignore. We leave a more fully-specified, dynamic model of the local labor supply and local labor demand responses to larger changes in road quality, along the lines of Allen and Arkolakis (2018) and Allen and Donaldson (2022), to further research.

[36]A histogram of the number of years of postive NPV across districts is reported in Appendix Figure A.13.

9 Conclusion

Even though road maintenance investments typically account for a significant portion of countries' budgets, little is known about their stimulus effects in developing countries, where roads deteriorate rapidly and spatial disparities are particularly pronounced. This paper aims to understand the role that road maintenance investments can play in such countries, not only through looking at welfare effects, but also by investigating the different possible mechanisms through which these effects materialize. While much of the previous literature on this topic has focused on the construction of new roads, we add to the literature by evaluating the effects of substantial changes in road quality due to maintenance and upgrading of existing roads, using data from all national and provincial highways in Indonesia.

We combine a novel dataset documenting substantial variation in road quality from 1990 to 2007 with high quality panel data on households and firms. Using these data, we provide reduced form evidence that road improvements significantly increase nominal consumption and income. We do not see substantial changes in the extensive or intensive margin of labor supply; instead, we observe occupational shifts from informal employment (including agriculture) into higher paying, newly available manufacturing jobs. These employment results are consistent with results from manufacturing firms, which show that improved roads generate the entry of new firms in the formal manufacturing sector.

When we combine the reduced form estimates of the effects of road quality on labor income, firm profits, and prices with a simple model to estimate effects on welfare, we find that a 1 percent increase in road quality increases welfare by 0.1 percent. Extrapolating these marginal welfare changes to potentially non-marginal upgrades, and making reasonable assumptions about upgrade costs and deterioration speeds, policy simulations show that for the median district, paving all national and provincial roads would generate a positive NPV of roughly 3 percent of district GDP. For the median district, the benefit of paving roads is also 2.3 times its cost.

The methodological contribution of this paper is to address the common concerns of targeting bias and reverse causality by proposing a new instrument, replicable in many instances. We take advantage of Indonesia's two-step budgeting framework for funding road maintenance, where sub-national authorities are in charge of maintaining roads and funding different parts of the road network. This allows us to construct a time varying instrument for road quality based on allocation formulas. Our instrumental variables strategy identifies effects from the set of roads that get maintained when road budgets allow for it, but which get less maintenance when road budgets are tight or scaled back.

The evidence presented in this paper shows that improving major national and provincial highways can improve local economic development through increasing formal labor market opportunities. Conversely, deterioration of these roads may have adverse effects in the opposite direction. Our analysis suggests that governments should be aware of the impacts of road maintenance investments on household welfare when setting priorities for transportation budgets as well as when considering counter-cyclical fiscal policy.

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Table 1: Provincial Maintenance Budget Proxies and Lagged Economic Activity

Panel A: DV: $\widetilde{B}^N_{p,t}$, Log Weighted Upgraded Km, National Roads	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
L.Log Regional GDP	0.629	-1.640			0.467	-1.716
	(1.376)	(3.590)			(1.563)	(3.738)
L2.Log Regional GDP		4.495				4.570
		(3.871)				(3.913)
L3.Log Regional GDP		-2.223				-2.331
		(2.699)				(2.687)
L.Log Population			0.737	1.469	0.494	1.315
			(1.186)	(1.658)	(1.342)	(1.902)
L2.Log Population				-1.016		-1.180
				(2.161)		(2.175)
L3.Log Population				-0.010		-0.011
				(2.279)		(2.194)
N	289	289	289	289	289	289
F Stat	0.21	0.56	0.39	0.27	0.19	0.49
Adjusted R^2	0.58	0.58	0.58	0.58	0.58	0.57
Adjusted R^2 (within)	-0.00	-0.01	-0.00	-0.01	-0.01	-0.02

Panel B: DV: $\widetilde{B}^P_{p,t}$, Log Weighted Upgraded Km, Provincial Roads	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
L.Log Regional GDP	1.578	-1.375			1.523	-1.318
	(1.170)	(2.584)			(1.042)	(2.579)
L2.Log Regional GDP		1.082				1.046
		(3.215)				(3.270)
L3.Log Regional GDP		2.670				2.636
		(2.983)				(2.899)
L.Log Population			0.963	-0.103	0.168	-0.998
			(1.102)	(1.778)	(0.998)	(1.797)
L2.Log Population				0.866		0.722
				(3.749)		(3.787)
L3.Log Population				0.945		0.490
				(2.524)		(2.520)
N	289	289	289	289	289	289
F Stat	1.82	0.69	0.76	0.60	1.10	0.50
Adjusted R^2	0.56	0.55	0.55	0.55	0.55	0.55
Adjusted R^2 (within)	0.00	-0.00	-0.00	-0.01	-0.00	-0.01
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: We report coefficients of two-way fixed effects regressions of our national highways budget proxy (Panel A) and our provincial highways budget proxy (Panel B) on lags of provincial GDP and lags of provincial population. All regressions control for province and year fixed effects. Robust standard errors, clustered by province, are reported in parentheses. */**/*** denotes significant at the 10% / 5% / 1% levels.

Table 2: Effects of Road Quality on Consumption, Income, and Employment

	FELS	IV-GMM		Stats	
Panel A: Household-Level Outcomes	**(1)**	**(2)**	**KBP**	$\overline{Y}$	N
Log HH Per-Capita Consumption Expenditures	0.120*** (0.042)	0.279*** (0.086) [0.01]	129.090	11.079	23508
Log Farm Profits	0.002 (0.001)	0.003 (0.002) [0.15]	99.229	18.426	7159
Log Non-Farm Profits	0.001 (0.002)	0.012* (0.006) [0.08]	94.488	18.696	4724
Panel B: Individual-Level Outcomes					
Log Total Earnings	0.076** (0.033)	0.236*** (0.066) [0.01]	90.775	1.437	17652
Any Employment (0 1)?	-0.025 (0.021)	0.058 (0.038) [0.13]	104.087	0.702	36330
Log Total Hours Worked	-0.002 (0.038)	-0.069 (0.069) [0.20]	103.736	199.067	22984
Agriculture ... Working (0 1)?	-0.062*** (0.018)	-0.036 (0.041) [0.21]	103.801	0.418	22987
Manu / Other Formal ... Working (0 1)?	0.087*** (0.022)	0.168*** (0.047) [0.01]	103.801	0.562	22987
Other, Informal ... Working (0 1)?	-0.093*** (0.022)	-0.183*** (0.044) [0.00]	103.801	0.466	22987
Year FE	Yes	Yes			
Individual FE	Yes	Yes			

Notes: In Panel A, we report the results of household-level panel regressions with household and year fixed effects, while in Panel B, we report the results of individual-level panel regressions with individual and year fixed effects. Each cell reports estimates of β from a separate regression, with the dependent variable listed in the row heading. For Panel A, controls include household size and month of survey indicators. The farm and non-farm profit variables are only defined for households that reported profits, and we first winsorize these variables to account for outliers. In Panel B, we additionally control for individual age and years of schooling. Total hours worked and the working-by-sector indicators are defined only if the individual reported working. Dependent variable means are reported in levels. The "KBP" column reports the Kleibergen and Paap (2006) Wald Rank F statistic. Robust standard errors, clustered at the village level, are reported in parentheses. */**/*** denotes significant at the 10% / 5% / 1% levels. Two-stage false-discovery rate (FDR) sharpened q-values are reported in brackets below the standard errors (Benjamini et al., 2006; Anderson, 2008).

Table 3: Road Quality and District-Level Manufacturing Outcomes

	FELS	IV-GMM		Stats	
Panel A: Firm Counts	**(1)**	**(2)**	**KBP**	$\overline{Y}$	N
Log Number of Opened Firms	0.079 (0.061)	1.206*** (0.204) [0.00]	120.988	6.049	3400
Log Number of Closed Firms	-0.024 (0.072)	0.125 (0.287) [0.18]	71.660	6.088	3200
Percent Δ Number of Firms	0.012 (0.022)	0.303*** (0.088) [0.01]	129.589	-0.031	3356
Panel B: Production					
Log Output	0.197 (0.147)	1.654*** (0.507) [0.01]	120.988	1582.368	3400
Log Value Added	0.149 (0.142)	1.401*** (0.479) [0.01]	120.988	572.269	3400
Log Total Employment	-0.153* (0.082)	0.405 (0.255) [0.05]	120.988	13657.688	3400
Log Output per Worker	0.351*** (0.106)	1.243*** (0.351) [0.01]	120.988	0.111	3400
Year FE	Yes	Yes			
District FE	Yes	Yes			

Notes: We report the results of district-level panel regressions of the dependent variable on road quality. Each cell reports β from a separate regression, with the dependent variable listed in the row heading. All regressions include district and year fixed effects. Robust standard errors, clustered at the district level, are reported in parentheses. Dependent variable means are reported in levels. The "KBP" column reports the Kleibergen and Paap (2006) Wald Rank F statistic. */**/*** denotes significant at the 10% / 5% / 1% levels. Two-stage false-discovery rate (FDR) sharpened q-values are reported in brackets below the standard errors (Benjamini et al., 2006; Anderson, 2008).

Table 4: Road Quality and Firm-Level Manufacturing Outcomes

	FELS	IV-GMM		Stats	
	(1)	(2)	KBP	$\overline{Y}$	N
Log Output	-0.045 (0.045)	-0.155 (0.126) [0.55]	19.433	19.937	278471
Log Value Added	-0.067 (0.052)	-0.118 (0.151) [0.66]	19.414	7.371	278408
Log Total Labor	-0.009 (0.013)	-0.082* (0.042) [0.40]	19.407	164.249	278586
Log Output per Worker	-0.035 (0.043)	-0.073 (0.113) [0.66]	19.434	0.073	278326
Year FE	Yes	Yes			
District FE	Yes	Yes			
Firm FE	Yes	Yes			

Notes: We report the results of firm-level panel regressions of the dependent variable on road quality. Each cell reports β from a separate regression, with the dependent variable listed in the row heading. All regressions include firm and year fixed effects (and implicitly also district fixed effects). Robust standard errors, clustered at the district level, are reported in parentheses. Dependent variable means are reported in levels. The "KBP" column reports the Kleibergen and Paap (2006) Wald Rank F statistic. */**/*** denotes significant at the 10% / 5% / 1% levels. Two-stage false-discovery rate (FDR) sharpened q-values are reported in brackets below the standard errors (Benjamini et al., 2006; Anderson, 2008).

Table 5: Road Quality and Community Prices

	FELS	IV-GMM		Stats	
	(1)	**(2)**	**KBP**	$\overline{Y}$	N
Log Factory Wage	-0.055 (0.182)	0.452 (0.400) [0.30]	14.011	3842.123	226
Log Farm Wage	0.041 (0.114)	0.112 (0.148) [0.33]	82.897	3766.165	339
Log Perishables Price	-0.313*** (0.082)	-1.007*** (0.199) [0.02]	126.974	76.494	914
Log Non-Perishables Price	0.060 (0.079)	0.169 (0.213) [0.33]	132.303	135.353	923
Log Land Value (Hedonic FE)	0.405*** (0.134)	0.459** (0.214) [0.13]	131.156	3790.753	579
Log Rent (Hedonic FE)	0.139** (0.062)	0.516*** (0.117) [0.02]	136.268	3838.634	901
Year FE	Yes	Yes			
Village FE	Yes	Yes			

Notes: We report the results of community-level panel regressions of the dependent variable on road quality (both in logs). Each cell reports β from a separate regression, with the dependent variable listed in the row heading. Log Farm Wage is not available in 1993. Dependent variable means are reported in levels. The "KBP" column reports the Kleibergen and Paap (2006) Wald Rank F statistic. Robust standard errors, clustered at the community level, are reported in parentheses. */**/*** denotes significant at the 10% / 5% / 1% levels. Two-stage false-discovery rate (FDR) sharpened q-values are reported in brackets below the standard errors (Benjamini et al., 2006; Anderson, 2008).

Table 6: Road Quality and District-Level Migration Outcomes

	FELS	IV-GMM		Stats	
	(1)	(2)	KBP	$\overline{Y}$	N
Percent Δ Population (2000-1990)	0.032	0.101	10.413	0.137	198
	(0.039)	(0.083)			
		[0.13]			
Log Total Recent Migrants (Kabu)	0.919***	2.206***	10.403	43514.102	198
	(0.149)	(0.398)			
		[0.03]			
Log Total Recent Migrants (Prov)	0.951***	2.835***	10.403	20453.100	198
	(0.226)	(0.619)			
		[0.03]			
Province FE	Yes	Yes			

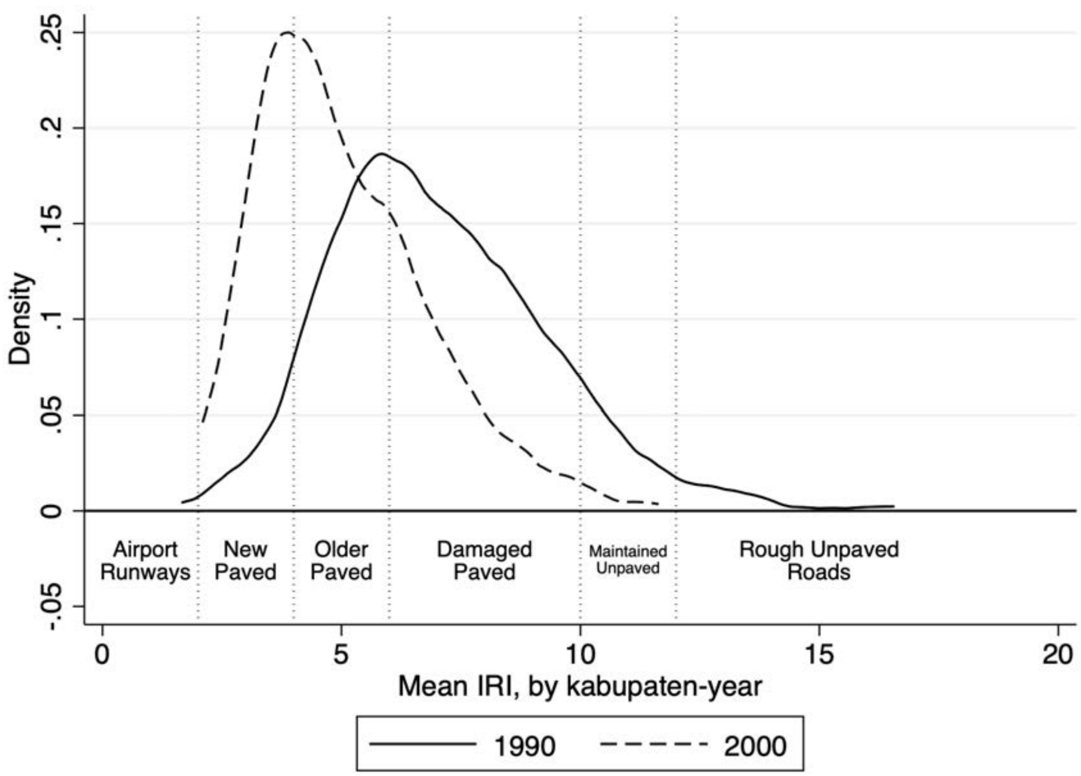
Notes: We report the results of cross-sectional regressions of the dependent variable on changes in road roughness. Each cell reports estimates of β from a separate regression, with the dependent variable listed in the row heading. All regressions include province fixed effects. For the percent change in population regression, we control for 1990 non-oil GDRP (in logs). For the migration regressions, we also include controls for the logs of 1990 population. Robust standard errors, clustered at the province level, are reported in parentheses. Dependent variable means are reported in levels. The "KBP" column reports the Kleibergen and Paap (2006) Wald Rank F statistic. */**/*** denotes significant at the 10% / 5% / 1% levels. Two-stage false-discovery rate (FDR) sharpened q-values are reported in brackets below the standard errors (Benjamini et al., 2006; Anderson, 2008).

Table 7: Local Household Welfare Effect: Decomposition

	Mean	95% C.I.	Share Negative
Overall Δ Welfare	0.095	[-0.206, 0.401]	0.326
Wage Income from Labor Effect	0.148	[0.079, 0.218]	0.000
Total Farm and Non-farm Business Profit Effect	0.002	[0.001, 0.004]	0.019
Housing Prices Effect	-0.009	[-0.016, -0.002]	0.199
Perishable Food Prices Effect	0.096	[0.064, 0.127]	0.000
Non-Perishable Food Prices Effect	-0.078	[-0.234, 0.079]	0.807
Non-Food Prices Effect	-0.065	[-0.195, 0.066]	0.807

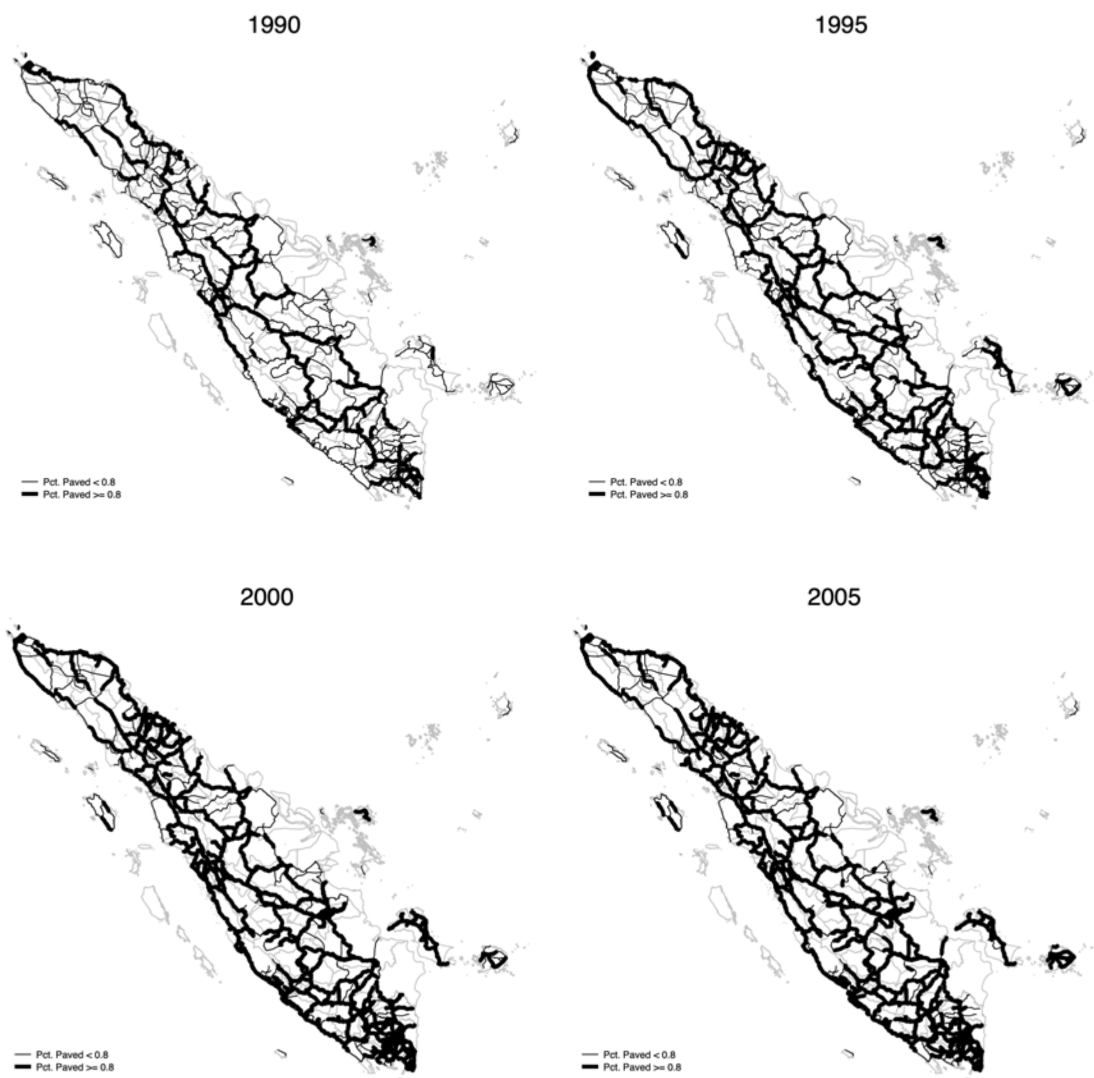
Notes: This table reports a decomposition of the estimated elasticity of road quality to welfare, based on equation (2). To construct confidence intervals, we use a parametric bootstrap procedure (Horowitz, 2001). In each bootstrap, we take a random sample (with replacement) from the 6,567 households in the IFLS-1, and we redraw each road quality elasticity parameter from a normal distribution, with a mean equal to that elasticity's point estimate and the standard deviation equal to that elasticity's standard error. We repeat this bootstrapping exercise for 1,000 replications.

Figure 1: Changes in the Distribution of Road Roughness



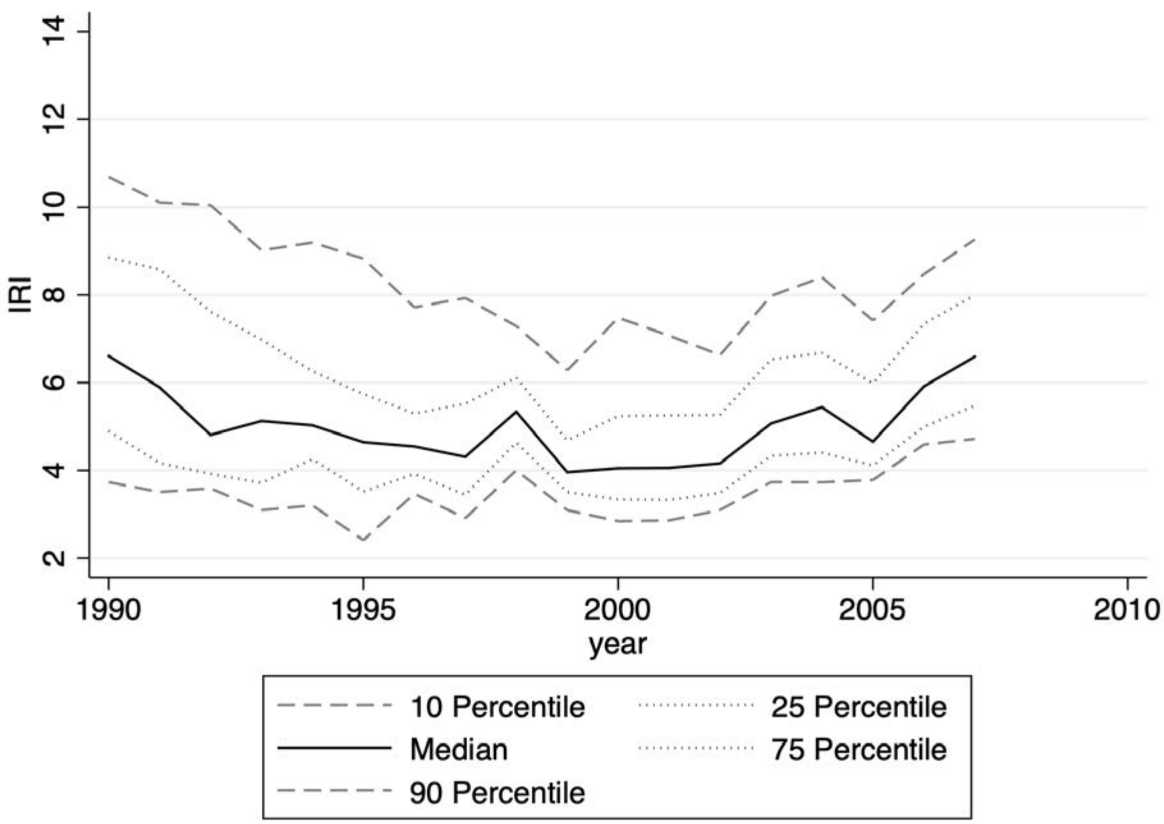
Notes: Authors' calculations using IRMS data. The mapping between IRI and pavement quality classifications is from Sayers and Karamihas (1998).

Figure 2: Evolution of Paved Surfaces on Sumatra's Road Network



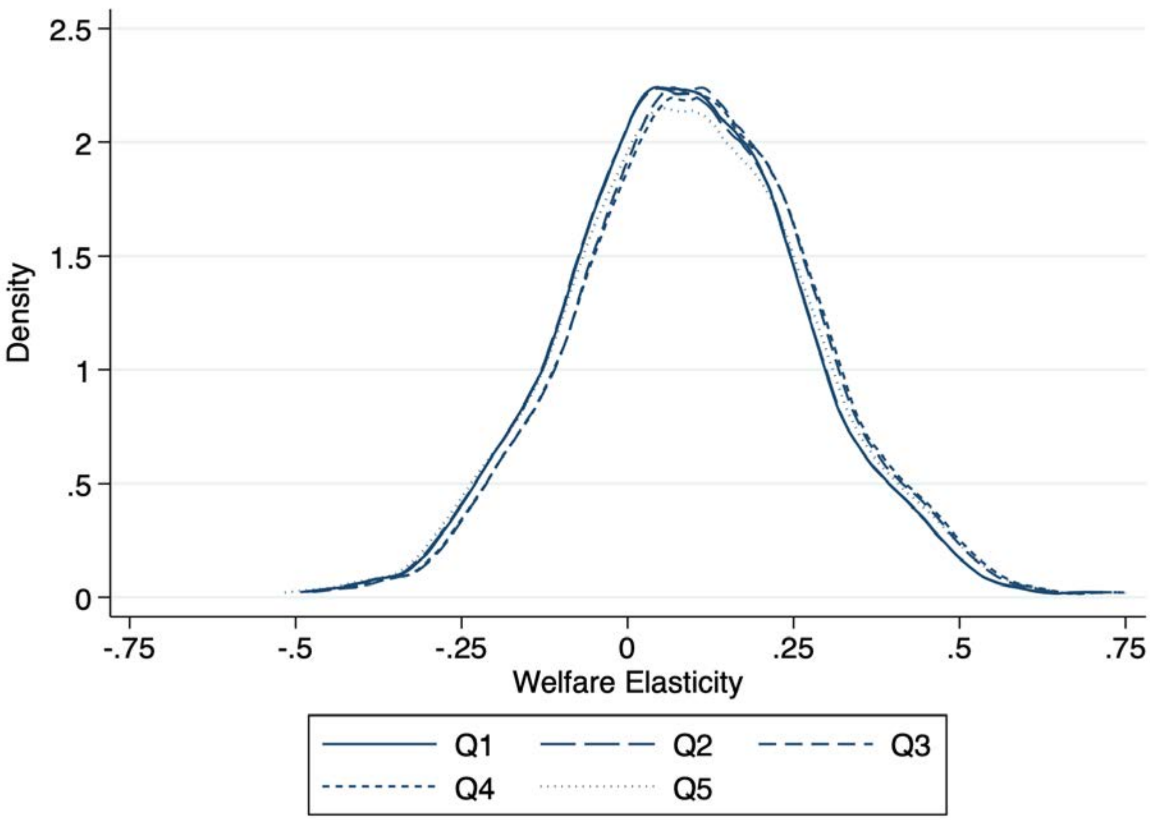
Notes: Authors' calculations using IRMS data. Thick black lines correspond to road sections that are 80 percent paved or greater, while thin black lines correspond to road sections that are less than 80 percent paved.

Figure 3: Changes in the Distribution of Road Roughness



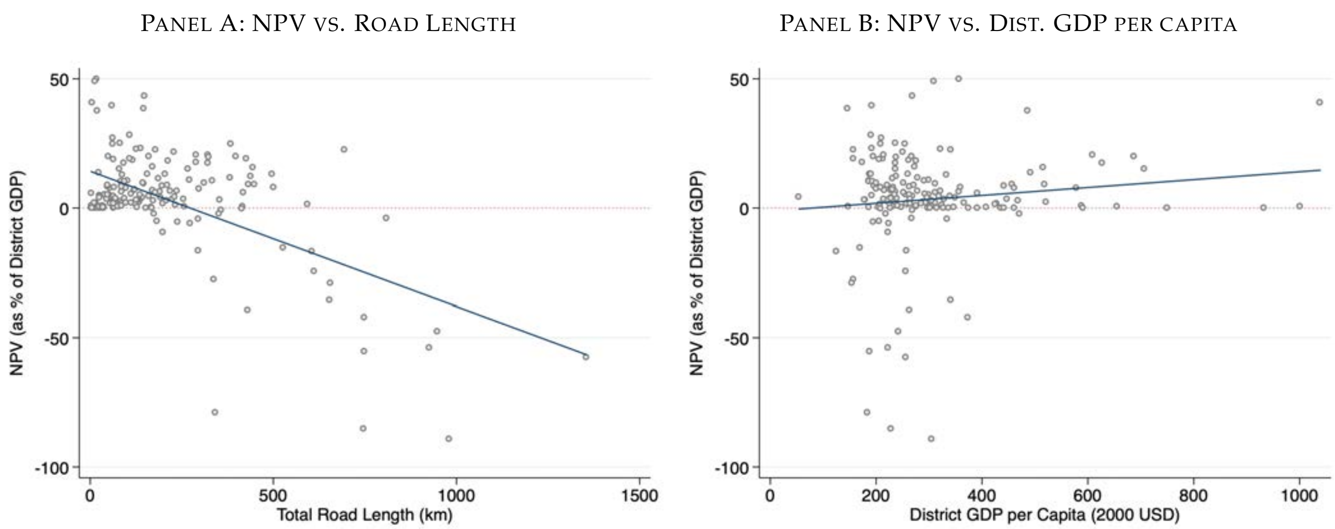
Notes: Authors' calculations using IRMS data. The percentiles plotted summarize the distribution of road roughness across different segments in the data.

Figure 4: Distribution of Welfare Gains: By Initial Consumption Quintile



Notes: This figure reports kernel density estimates of the distribution of welfare effects of road quality improvements across households, based on equation (2). The data are based on 1,000 bootstrap replications, following (Horowitz, 2001). Separate kernel density estimates are made for each quintile of the distribution of total household consumption expenditures. Q1 denotes the lowest expenditure quintile, while Q5 denotes the highest expenditure quintile.

Figure 5: Scatterplots of the Net Present Value of District Road Upgrades



Notes: This figure presents scatterplots of the net present value of a counterfactual district road upgrading program (as a percentage of district GDP in 1990) against the total road length in the district (Panel A) and district GDP per capita in 1990 (Panel B).

Online Appendix

Gertler, P., Gonzalez-Navarro, M., Gračner, T. and Rothenberg, A. (2024): "Road Maintenance and Local Economic Development: Evidence from Indonesia's Highways"

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A Additional Tables and Figures

Table A.1: Road Quality and Travel Times

	FELS	Stats	
Panel A: IFLS Travel Times	**(1)**	$\overline{Y}$	N
Log Travel Time to Nearest Provincial Capital	-0.179**	169.363	856
	(0.073)		
Log Travel Time to Nearest District Capital	-0.122	46.664	834
	(0.140)		
Panel B: Roughness-Based Travel Times			
Log Travel Time to Nearest Provincial Capital	-0.513***	73.656	888
	(0.031)		
Log Travel Time to Nearest District Capital	-0.488***	21.256	888
	(0.041)		
Year FE	Yes		
Village FE	Yes		

Notes: We report the results of community-level panel regressions of the dependent variable on road quality (both in logs). Each cell reports β from a separate regression, with the dependent variable listed in the row heading. Panel A uses measures of travel times derived from the IFLS survey responses, while Panel B uses measures of travel times derived from the road roughness network data. To construct these roughness-based travel times, we use the mapping between roughness and speed reported in Appendix B. All regressions include community and year fixed effects. Dependent variable means are reported in levels. Robust standard errors, clustered at the community level, are reported in parentheses. */**/*** denotes significant at the 10% / 5% / 1% levels.

Table A.2: Comparing Provincial Investment with Official Total Budget Figures

Panel A: Total Province Investment vs. Total DPU Budget

	(1)	(2)	(3)
Log Official Total Budget	0.072		
	(0.243)		
L.Log Official Total Budget		0.597**	
		(0.233)	
L2.Log Official Total Budget			0.056
			(0.205)
N	60	60	60
F Stat	1.09	12.19	17.12
Adjusted R^2	-0.02	0.38	0.23

Panel B: Total Province Investment vs. Total DPU Improvement Budget

	(1)	(2)	(3)
Log Official Improvement Budget	-0.003		
	(0.195)		
L.Log Official Improvement Budget		0.574**	
		(0.238)	
L2.Log Official Improvement Budget			-0.070
			(0.191)
N	60	60	60
F Stat	1.12	13.73	20.60
Adjusted R^2	-0.02	0.39	0.23
Year FE	Yes	Yes	Yes

Notes: This table reports the results of regressions of the log total number of kilometers upgraded in province p in year t (the dependent variable, measured from IRMS data) against the total official budget for roads in the province in year t (column 1), year $t-1$ (column 2) and year $t-2$ (column 3). The sample only includes data from the years 1993, 1994, 1996, and 2000, as these were the only years where historical budget data were available. All columns include year fixed effects. Robust standard errors are reported in parentheses. */**/*** denotes significant at the 10% / 5% / 1% levels.

Table A.3: First Stage Relationship across Different Units of Analysis

	Km Post-Year		**District-Year**	**IFLS Indiv-Year**	**IFLS HH-Year**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Log Weighted Budget, National Roads	0.017***		-0.018***	-0.023***	-0.023***
	(0.003)		(0.002)	(0.003)	(0.003)
Log Weighted Budget, Provincial Roads		0.029***	0.044***	0.053***	0.054***
		(0.002)	(0.003)	(0.004)	(0.004)
N	259549	259549	3599	49772	23511
Regression F Stat	30.91	158.44	112.14	111.38	136.11
Adjusted R^2	0.28	0.29	0.71	0.81	0.79
Adjusted R^2 (within)	0.00	0.01	0.08	0.25	0.26
District FE	Yes	Yes	Yes	.	.
Individual FE	.	.	.	Yes	.
Household FE	.	.	.	.	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the first stage relationship between average road quality and our budget proxies. Columns 1-2 measure road quality directly using the kilometer-post interval data from the IRMS. Column 3 aggregates this kilometer-post interval data to district-years, while columns 4 and 5 report district-year specifications corresponding to the IFLS panel analysis. Robust standard errors are reported in parentheses, clustered at the district level in columns 1-3 and the village level in columns 4 and 5. */**/*** denotes significant at the 10% / 5% / 1% levels.

Table A.4: Provincial Investment and Lags of Economic Activity: More Variables

	Avg Night Light Intensity		% with No Night Lights		Log Manuf. Output		Log Manuf. Employment	
Panel A: DV: Log Wt. Km, National Roads	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Lag (t-1)	0.719	-1.189	-0.655	0.398	-0.308	-0.130	0.544	-0.183
	(0.991)	(2.039)	(1.289)	(1.879)	(0.222)	(0.281)	(0.389)	(0.467)
Lag (t-2)		1.065		1.107		-0.311		0.507
		(1.777)		(1.688)		(0.248)		(0.368)
Lag (t-3)		1.035		-0.944		-0.007		0.460
		(1.556)		(1.614)		(0.117)		(0.453)
N	255	221	255	221	289	289	289	289
F Stat	0.53	0.52	0.26	0.21	1.93	1.16	1.96	1.52
Adjusted R^2	0.60	0.61	0.60	0.60	0.58	0.59	0.59	0.59
Adjusted R^2 (within)	-0.00	-0.01	-0.00	-0.01	0.00	0.01	0.01	0.01
Panel B: DV: Log Wt. Km, Provincial Roads	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Lag (t-1)	-1.805*	-1.274	1.952	1.434	0.312	0.378	-0.054	-0.105
	(0.947)	(2.254)	(1.282)	(2.102)	(0.203)	(0.245)	(0.325)	(0.488)
Lag (t-2)		0.531		1.331		0.075		0.114
		(1.616)		(1.920)		(0.198)		(0.452)
Lag (t-3)		-1.561		-0.175		-0.209*		-0.049
		(1.671)		(1.888)		(0.109)		(0.422)
N	255	221	255	221	289	289	289	289
F Stat	3.63	0.68	2.32	0.37	2.37	1.83	0.03	0.03
Adjusted R^2	0.55	0.56	0.55	0.55	0.56	0.56	0.56	0.55
Adjusted R^2 (within)	0.01	-0.00	0.00	-0.01	0.00	0.00	-0.00	-0.01
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the relationship between our budget proxies for national roads (Panel A) and provincial roads (Panel B) and lagged measures of economic activity. Columns 1-2 use time-varying measures of average provincial light intensity, columns 3-4 use time-varying measures of the percent of the provinces with no night time lights, columns 5-6 use log manufacturing output at the province level (from the SI), and columns 7-8 use log manufacturing employment at the province level (from the SI). Robust standard errors are reported in parentheses. */**/*** denotes significant at the 10% / 5% / 1% levels.

Table A.5: Effects of Road Quality on Consumption, Income, and Employment: Non-Movers

	FELS	IV-GMM		Stats	
Panel A: Household-Level Outcomes	**(1)**	**(2)**	**KBP**	$\overline{Y}$	N
Log HH Per-Capita Consumption Expenditures	0.120***	0.279***	129.090	11.079	23508
	(0.042)	(0.086)			
Log Farm Profits	0.002	0.003	99.229	18.426	7159
	(0.001)	(0.002)			
Log Non-Farm Profits	0.001	0.012*	94.488	18.696	4724
	(0.002)	(0.006)			
Panel B: Individual-Level Outcomes					
Log Total Earnings	0.076**	0.231***	90.775	1.437	17652
	(0.033)	(0.066)			
Log Total Hours Worked	-0.002	-0.069	103.736	199.067	22984
	(0.038)	(0.070)			
Any Employment (0 1)?	-0.025	0.058	104.087	0.702	36330
	(0.021)	(0.038)			
Agriculture ... Working (0 1)?	-0.062***	-0.036	103.801	0.418	22987
	(0.018)	(0.041)			
Manu / Other Formal ... Working (0 1)?	0.087***	0.168***	103.801	0.966	22987
	(0.022)	(0.047)			
Other, Informal ... Working (0 1)?	-0.093***	-0.183***	103.801	0.466	22987
	(0.022)	(0.044)			
Year FE	Yes	Yes			
Individual FE	Yes	Yes			

Note: We report the results of individual-level panel regressions with individual and survey-wave fixed effects. This table is identical to Table 2 except that we only estimate effects on non-moving individuals and households. Robust standard errors, clustered at the village level, are reported in parentheses. */**/*** denotes significant at the 10% / 5% / 1% levels.

Table A.6: Road Quality and Working in Other Informal Jobs

	FELS	IV-GMM
	(1)	**(2)**
Log Road Quality	-0.093***	-0.183***
	(0.022)	(0.044)
N	22,989	22,987
N Clusters	324	324
Adjusted R^2	0.538	0.538
Adjusted R^2 (within)	0.018	0.018
Kleibergen-Paap (KBP) Wald Rank F Stat		103.8
Under Id. Test (KP Rank LM Stat)		79.3
... p-Value		0.00
AR Wald Test (Weak IV Robust Inf.)		10.5
... p-Value		0.00
Sargan-Hansen J Test		1.6
... p-Value		0.21
Year FE	Yes	Yes
Individual FE	Yes	Yes

Notes: We report the results of district-level panel regressions of the dependent variable on road quality. Each cell reports β from a separate regression, with the dependent variable listed in the row heading. All regressions include district and year fixed effects. Robust standard errors, clustered at the village level, are reported in parentheses. Dependent variable means are reported in levels. The "KBP" column reports the Kleibergen and Paap (2006) Wald Rank F statistic. */**/*** denotes significant at the 10% / 5% / 1% levels. Two-stage false-discovery rate (FDR) sharpened q-values are reported in brackets below the standard errors (Benjamini et al., 2006; Anderson, 2008).

Table A.7: Effects of Road Quality on Log Number of Opened Firms, by Industry

	IV-GMM **(1)**
Log Number of Opened Firms	1.206*** (0.204)
... 31. Food and Beverages	0.574*** (0.131)
... 32. Textiles	0.278** (0.114)
... 33. Wood Products	0.244** (0.111)
... 34. Paper Products	0.129*** (0.038)
... 35. Chemical Products	0.114* (0.058)
... 36. Ceramics & Glass	0.217*** (0.066)
... 38. Metal & Machines	0.083 (0.070)
... 39. Other Products	0.046 (0.029)
Year FE	Yes
District FE	Yes

Note: This table reports the coefficients from regressions of the number of opened firms, by industry, on road quality (both in logs). The first row replicates the specification from Table 3, row 1, but we report effects separately by 2-digit industry in subsequent rows. All regressions include district and year fixed effects. Robust standard errors, clustered at the district level, are reported in parentheses. */**/*** denotes significant at the 10% / 5% / 1% levels.

Table A.8: Hedonic Regressions

	DV: Log Rent	DV: Log Land Value
	(1)	(2)
Type of dwelling: Single Unit, Single Level	0.077**	-0.010
	(0.033)	(0.092)
Type of dwelling: Single Unit, Multi Level	0.179***	0.070
	(0.033)	(0.103)
Type of dwelling: Duplex	0.111***	0.098
	(0.036)	(0.107)
Type of dwelling: Multi Unit, Single Level	0.112***	0.026
	(0.037)	(0.142)
House is surrounded by human and animal waste	0.015	-0.052
	(0.017)	(0.033)
House is surrounded by piles of trash	0.012	-0.017
	(0.015)	(0.030)
House is surrounded by stagnant water	-0.027*	-0.008
	(0.015)	(0.036)
There is a stable under / next to house	-0.017	0.049*
	(0.012)	(0.025)
House has sufficient ventilation	0.023**	0.066**
	(0.011)	(0.028)
Owned house	0.064***	0.043
	(0.012)	(0.047)
House rented/contracted	-0.083***	0.029
	(0.019)	(0.147)
Yard is moderately sized	0.037***	0.108***
	(0.010)	(0.025)
Room number in the house	-0.063***	0.080***
	(0.003)	(0.006)
Ceramic floor	0.210***	0.264***
	(0.022)	(0.064)
Tiled floor	0.093***	0.244***
	(0.021)	(0.052)
Cement floor	0.013	0.156***
	(0.017)	(0.046)
Lumber floor	0.009	0.147**
	(0.028)	(0.062)
Bamboo floor	-0.040	0.247**
	(0.049)	(0.118)
Masonry outer wall	0.166***	0.203***
	(0.018)	(0.045)
Lumber outer wall	0.048**	0.177***
	(0.019)	(0.043)
Concrete roof	0.183***	0.176
	(0.061)	(0.257)
Wooden roof	0.107**	0.059
	(0.048)	(0.110)
Metal roof	0.054*	0.054
	(0.028)	(0.065)
Tiled roof	0.095***	-0.021
	(0.029)	(0.067)
Asbestos roof	0.104***	-0.227*
	(0.037)	(0.120)
Electricity in the house	0.083***	0.132***
	(0.020)	(0.045)
Piped water used for cooking	-0.007	-0.053
	(0.024)	(0.091)
Pump/Well water used for cooking	-0.069***	0.107
	(0.023)	(0.079)
Well/Spring/Rain water used for cooking	-0.133***	0.055
	(0.024)	(0.072)
River water used for cooking	-0.079**	-0.132
	(0.039)	(0.091)
Purchased water used for cooking	-0.064	-0.038
	(0.105)	(0.317)
Inside water source	0.027**	0.077**
	(0.011)	(0.030)
Own toilet	0.085***	0.094***
	(0.012)	(0.028)
Drainage ditch (flowing)	0.044***	0.041
	(0.010)	(0.026)
Drainage ditch (stagnant)	-0.008	-0.000
	(0.018)	(0.036)
Trash collected by sanitation service	0.071***	
	(0.016)	
N	25567	9671
Regression F-Stat	30.731	16.256
Adj. R^2	0.399	0.308
Adj. R^2 (Within)	0.057	0.078
Community × Wave FE	Yes	Yes

Note: All regressions include community × survey wave fixed effects. Robust standard errors, clustered at the village level, are reported in parentheses. */**/*** denotes significant at the 10% / 5% / 1% levels.

Table A.9: Road Quality and Community Prices: Median Values vs. Hedonic Estimates

	FELS	IV-GMM		Stats	
	(1)	**(2)**	**KBP**	$\overline{Y}$	N
Log Land Value (Hedonic FE)	0.405***	0.459**	131.156	3790.753	579
	(0.134)	(0.214)			
Log Rent (Hedonic FE)	0.139**	0.516***	136.268	3838.634	901
	(0.062)	(0.117)			
Median Log Land Value	0.478**	0.433	125.957	3856.951	751
	(0.197)	(0.301)			
Median Log Rent	0.090	0.531***	134.552	3852.859	922
	(0.072)	(0.139)			
Year FE	Yes	Yes			
Village FE	Yes	Yes			

Notes: We report the results of community-level panel regressions of the dependent variable on road quality (both in logs). Each cell reports β from a separate regression, with the dependent variable listed in the row heading. Dependent variable means are reported in levels. The "KBP" column reports the Kleibergen and Paap (2006) Wald Rank F statistic. Robust standard errors, clustered at the community level, are reported in parentheses. */**/*** denotes significant at the 10% / 5% / 1% levels.

Table A.10: Effects of Road Quality: Robustness to New IVs and Controls

	FELS	Baseline IVs	IV Refinements				
Panel A: Roughness-Based Travel Times	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Log Travel Time to Nearest Provincial Capital	-0.513***	-1.079***	-1.075***	-1.074***	-1.070***	-0.969***	-0.960***
	(0.031)	(0.082)	(0.083)	(0.082)	(0.083)	(0.073)	(0.074)
N	888	888	888	888	888	888	888
Kleibergen-Paap Wald Rank *F* Stat		120.39	115.65	115.67	111.71	146.67	151.66
Panel B: Household-Level Outcomes							
Log HH Per-Capita Consumption Expenditures	0.120***	0.279***	0.289***	0.297***	0.309***	0.357***	0.371***
	(0.042)	(0.086)	(0.087)	(0.086)	(0.087)	(0.099)	(0.102)
N	23508	23508	23508	23508	23508	23508	23508
Kleibergen-Paap Wald Rank *F* Stat		129.09	122.59	123.53	118.54	136.99	138.47
Panel C: Individual-Level Outcomes							
Log Total Earnings	0.076**	0.236***	0.235***	0.251***	0.255***	0.231***	0.234***
	(0.033)	(0.066)	(0.067)	(0.068)	(0.069)	(0.068)	(0.069)
N	17654	17652	17652	17652	17652	17652	17652
Kleibergen-Paap Wald Rank *F* Stat		90.78	86.67	87.46	84.54	110.33	106.88
Manu / Other Formal ... Working (0 1)?	0.087***	0.168***	0.160***	0.171***	0.164***	0.185***	0.179***
	(0.022)	(0.047)	(0.047)	(0.047)	(0.047)	(0.048)	(0.049)
N	22989	22987	22987	22987	22987	22987	22987
Kleibergen-Paap Wald Rank *F* Stat		103.80	99.22	99.50	96.18	118.67	116.34
Other, Informal ... Working (0 1)?	-0.093***	-0.183***	-0.176***	-0.187***	-0.179***	-0.206***	-0.199***
	(0.022)	(0.044)	(0.044)	(0.044)	(0.045)	(0.046)	(0.047)
N	22989	22987	22987	22987	22987	22987	22987
Kleibergen-Paap Wald Rank *F* Stat		103.80	99.22	99.50	96.18	118.67	116.34
Panel D: District-Level Manufacturing Outcomes							
Log Number of Opened Firms	0.079	1.206***	1.267***	1.220***	1.283***	1.260***	1.330***
	(0.061)	(0.204)	(0.208)	(0.204)	(0.209)	(0.213)	(0.218)
N	3400	3400	3400	3400	3400	3400	3400
Kleibergen-Paap Wald Rank *F* Stat		120.99	116.49	118.73	114.48	109.01	103.01
Log Output per Worker	0.351***	1.243***	1.022***	1.397***	1.148***	0.963***	0.732**
	(0.106)	(0.351)	(0.361)	(0.368)	(0.377)	(0.353)	(0.364)
N	3400	3400	3400	3400	3400	3400	3400
Kleibergen-Paap Wald Rank *F* Stat		120.99	116.49	118.73	114.48	109.01	103.01
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cross-Sectional FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Leave-One Out Budget IV	.	.	Yes	.	Yes	.	Yes
IV w/ Budget Residuals	.	.	.	Yes	Yes	.	.
Controlling for Lagged Province Growth	.	.	.	.	.	Yes	Yes

Notes: We report the results of panel regressions of different dependent variables on road quality (both in logs). All regressions include year fixed effects and cross-sectional fixed effects (community FE in Panel A; household FE in Panel B; individual FE in Panel C; and district FE in panels D). Cluster robust standard errors are reported in parentheses, with community-level clustering in Panels A, B, and C and district-level clustering in Panel D. */**/*** denotes significant at the 10% / 5% / 1% levels.

Table A.11: Effects of Road Quality: Robustness to Controls and Sample Splits

		IV-GMM								
	FELS	**Baseline**	**No Oil / Gas**	**No Mining**	**FDI**	**PLN**	**TV**	**Schools**	**Health Facil.**	**Only 1998+**
Panel A: Roughness-Based Travel Times	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**
Log Travel Time to Nearest Provincial Capital	-0.513***	-1.079***	-1.167***	-1.095***	-1.104***	-1.052***	-1.129***	-1.050***	-1.124***	-1.407***
	(0.031)	(0.082)	(0.098)	(0.090)	(0.085)	(0.082)	(0.091)	(0.080)	(0.088)	(0.166)
N	888	888	744	820	888	888	888	888	888	446
Kleibergen-Paap Wald Rank F Stat		120.39	98.33	107.12	122.28	111.94	109.20	122.71	106.16	41.21
Panel B: Household-Level Outcomes										
Log HH Per-Capita Consumption Expenditures	0.120***	0.279***	0.243***	0.234***	0.263***	0.354***	0.260***	0.258***	0.251***	0.427***
	(0.042)	(0.086)	(0.090)	(0.088)	(0.087)	(0.087)	(0.090)	(0.082)	(0.089)	(0.104)
N	23508	23508	19341	21422	23508	23508	23508	23508	23508	12120
Kleibergen-Paap Wald Rank F Stat		129.09	107.07	118.25	131.25	114.07	120.75	128.19	110.40	76.83
Panel C: Individual-Level Employment Outcomes										
Log Total Earnings	0.076**	0.236***	0.244***	0.221***	0.230***	0.253***	0.197***	0.207***	0.264***	0.440***
	(0.033)	(0.066)	(0.071)	(0.066)	(0.066)	(0.070)	(0.071)	(0.064)	(0.073)	(0.115)
N	17654	17652	14976	16226	17652	17652	17652	17652	17652	7768
Kleibergen-Paap Wald Rank F Stat		90.78	79.47	83.57	90.57	79.15	81.76	94.94	83.66	47.70
Manu / Other Formal ... Working (0 1)?	0.087***	0.168***	0.141***	0.146***	0.166***	0.195***	0.161***	0.142***	0.168***	0.120*
	(0.022)	(0.047)	(0.050)	(0.048)	(0.047)	(0.046)	(0.050)	(0.045)	(0.049)	(0.066)
N	22989	22987	19185	20764	22987	22987	22987	22987	22987	10524
Kleibergen-Paap Wald Rank F Stat		103.80	91.20	95.37	103.38	90.03	88.69	105.34	91.68	48.70
Other, Informal ... Working (0 1)?	-0.093***	-0.183***	-0.164***	-0.160***	-0.181***	-0.213***	-0.177***	-0.157***	-0.193***	-0.142**
	(0.022)	(0.044)	(0.047)	(0.045)	(0.044)	(0.044)	(0.047)	(0.042)	(0.047)	(0.064)
N	22989	22987	19185	20764	22987	22987	22987	22987	22987	10524
Kleibergen-Paap Wald Rank F Stat		103.80	91.20	95.37	103.38	90.03	88.69	105.34	91.68	48.70
Panel D: District-Level Manufacturing Outcomes										
Log Number of Opened Firms	0.079	1.206***	1.193***	1.196***	1.208***	1.312***	1.222***	1.147***	1.222***	1.623***
	(0.061)	(0.204)	(0.217)	(0.216)	(0.204)	(0.225)	(0.217)	(0.205)	(0.216)	(0.371)
N	3400	3400	2822	2839	3400	3396	3396	3396	3396	1800
Kleibergen-Paap Wald Rank F Stat		120.99	109.94	120.90	123.20	99.35	95.05	116.03	114.16	68.15
Log Output per Worker	0.351***	1.243***	1.323***	1.378***	1.189***	1.261***	1.252***	1.198***	1.226***	0.826*
	(0.106)	(0.351)	(0.385)	(0.385)	(0.354)	(0.361)	(0.374)	(0.359)	(0.357)	(0.422)
N	3400	3400	2822	2839	3400	3396	3396	3396	3396	1800
Kleibergen-Paap Wald Rank F Stat		120.99	109.94	120.90	123.20	99.35	95.05	116.03	114.16	68.15
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cross-Section FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: We report the results of panel regressions of different dependent variables on road quality (both in logs). All regressions include year fixed effects and cross-sectional fixed effects (community FE in Panel A; household FE in Panel B; individual FE in Panel C; and district FE in panels D). Cluster robust standard errors are reported in parentheses, with community-level clustering in Panels A, B, and C and district-level clustering in Panel D. */**/*** denotes significant at the 10% / 5% / 1% levels.

Table A.12: Effects of Road Quality: Controlling for Economic Activity in Adjacent Provinces

	FELS	IV-GMM		
Panel A: Roughness-Based Travel Times	**(1)**	**(2)**	**(3)**	**(4)**
Log Travel Time to Nearest Provincial Capital	-0.513***	-1.079***	-1.185***	-1.097***
	(0.031)	(0.082)	(0.117)	(0.106)
N	888	888	888	888
Kleibergen-Paap Wald Rank *F* Stat		120.39	52.59	74.38
Panel B: Household-Level Outcomes				
Log HH Per-Capita Consumption Expenditures	0.120***	0.279***	0.326***	0.220*
	(0.042)	(0.086)	(0.118)	(0.126)
N	23508	23508	23508	23508
Kleibergen-Paap Wald Rank *F* Stat		129.09	65.68	70.66
Panel C: Individual-Level Outcomes				
Log Total Earnings	0.076**	0.236***	0.458***	0.333***
	(0.033)	(0.066)	(0.089)	(0.082)
N	17654	17652	17647	17647
Kleibergen-Paap Wald Rank *F* Stat		90.78	80.38	69.32
Manu / Other Formal ... Working (0 1)?	0.087***	0.168***	0.210***	0.242***
	(0.022)	(0.047)	(0.053)	(0.063)
N	22989	22987	22980	22980
Kleibergen-Paap Wald Rank *F* Stat		103.80	87.96	74.12
Other, Informal ... Working (0 1)?	-0.093***	-0.183***	-0.219***	-0.236***
	(0.022)	(0.044)	(0.051)	(0.061)
N	22989	22987	22980	22980
Kleibergen-Paap Wald Rank *F* Stat		103.80	87.96	74.12
Panel D: District-Level Manufacturing Outcomes				
Log Number of Opened Firms	0.079	1.206***	1.417***	1.152***
	(0.061)	(0.204)	(0.271)	(0.203)
N	3400	3400	3400	3400
Kleibergen-Paap Wald Rank *F* Stat		120.99	75.12	115.23
Log Output per Worker	0.351***	1.243***	1.230***	1.076***
	(0.106)	(0.351)	(0.441)	(0.349)
N	3400	3400	3400	3400
Kleibergen-Paap Wald Rank *F* Stat		120.99	75.12	115.23
Year FE	Yes	Yes	Yes	Yes
Cross-Sectional FE	Yes	Yes	Yes	Yes
Controls for Lags of GDP in Adjacent Provinces	.	.	Yes	.
Controls for Lags of Population in Adjacent Provinces	.	.	.	Yes

Notes: We report the results of panel regressions of different dependent variables on road quality (both in logs). All regressions include year fixed effects and cross-sectional fixed effects (community FE in Panel A; household FE in Panel B; individual FE in Panel C; and district FE in panels D). Cluster robust standard errors are reported in parentheses, with community-level clustering in Panels A, B, and C and district-level clustering in Panel D. */**/*** denotes significant at the 10% / 5% / 1% levels.

Table A.13: Effects of Road Quality: Adding Island and Province Trends

	FELS	IV-GMM		
Panel A: Roughness-Based Travel Times	**(1)**	**(2)**	**(3)**	**(4)**
Log Travel Time to Nearest Provincial Capital	-0.513***	-1.079***	-1.604***	-0.931***
	(0.031)	(0.082)	(0.216)	(0.243)
N	888	888	888	888
Kleibergen-Paap Wald Rank *F* Stat		120.39	37.00	14.58
Panel B: Household-Level Outcomes				
Log HH Per-Capita Consumption Expenditures	0.120***	0.279***	0.249***	0.248***
	(0.042)	(0.086)	(0.088)	(0.082)
N	23508	23508	24522	24522
Kleibergen-Paap Wald Rank *F* Stat		129.09	121.68	137.80
Panel C: Individual-Level Outcomes				
Log Total Earnings	0.076**	0.236***	0.269***	0.269***
	(0.033)	(0.066)	(0.069)	(0.076)
N	17654	17652	17652	17652
Kleibergen-Paap Wald Rank *F* Stat		90.78	90.88	78.55
Manu / Other Formal ... Working (0 1)?	0.087***	0.168***	0.168***	0.177***
	(0.022)	(0.047)	(0.047)	(0.051)
N	22989	22987	22987	22987
Kleibergen-Paap Wald Rank *F* Stat		103.80	103.83	92.51
Other, Informal ... Working (0 1)?	-0.093***	-0.183***	-0.185***	-0.192***
	(0.022)	(0.044)	(0.044)	(0.047)
N	22989	22987	22987	22987
Kleibergen-Paap Wald Rank *F* Stat		103.80	103.83	92.51
Panel D: District-Level Manufacturing Outcomes				
Log Number of Opened Firms	0.079	1.206***	1.161***	1.208***
	(0.061)	(0.204)	(0.356)	(0.306)
N	3400	3400	3400	3400
Kleibergen-Paap Wald Rank *F* Stat		120.99	45.53	63.42
Log Output per Worker	0.351***	1.243***	0.286	0.213
	(0.106)	(0.351)	(0.425)	(0.388)
N	3400	3400	3400	3400
Kleibergen-Paap Wald Rank *F* Stat		120.99	45.53	63.42
Year FE	Yes	Yes	Yes	Yes
Cross-Sectional FE	Yes	Yes	Yes	Yes
Island Trends	.	.	Yes	.
Province Trends	.	.	.	Yes

Notes: We report the results of panel regressions of different dependent variables on road quality (both in logs). All regressions include year fixed effects and cross-sectional fixed effects (community FE in Panel A; household FE in Panel B; individual FE in Panel C; and district FE in panels D). Cluster robust standard errors are reported in parentheses, with community-level clustering in Panels A, B, and C and district-level clustering in Panel D. */**/*** denotes significant at the 10% / 5% / 1% levels.

Table A.14: Effects of Road Quality: Wild Bootstrap

	FELS	IV-GMM	H_0: β (**Log Road Quality**) $= 0$ Baseline Clustering	District Wild Cluster	Province Wild Cluster
Panel A: Roughness-Based Travel Times	**(1)**	**(2)**	p-value	p-value	p-value
Log Travel Time to Nearest Provincial Capital	-0.513***	-1.079***	0.000	0.000	0.003
	(0.031)	(0.082)			
N	888	888	888	888	888
Kleibergen-Paap Wald Rank F Stat		120.39			
Panel B: Household-Level Outcomes					
Log HH Per-Capita Consumption Expenditures	0.120***	0.279***	0.001	0.007	0.299
	(0.042)	(0.086)			
N	23508	23508	24522	24522	24522
Kleibergen-Paap Wald Rank F Stat		129.09			
Panel C: Individual-Level Outcomes					
Log Total Earnings	0.076**	0.236***	0.000	0.000	0.010
	(0.033)	(0.066)			
N	17654	17652	17923	17923	17923
Kleibergen-Paap Wald Rank F Stat		90.78			
Manu / Other Formal ... Working (0 1)?	0.087***	0.168***	0.000	0.000	0.046
	(0.022)	(0.047)			
N	22989	22987	23347	23347	23347
Kleibergen-Paap Wald Rank F Stat		103.80			
Other, Informal ... Working (0 1)?	-0.093***	-0.183***	0.000	0.000	0.016
	(0.022)	(0.044)			
N	22989	22987	23347	23347	23347
Kleibergen-Paap Wald Rank F Stat		103.80			
Panel D: District-Level Manufacturing Outcomes					
Log Number of Opened Firms	0.079	1.206***	0.000	0.000	0.006
	(0.061)	(0.204)			
N	3400	3400	3400	3400	3400
Kleibergen-Paap Wald Rank F Stat		120.99			
Log Output per Worker	0.351***	1.243***	0.000	0.000	0.179
	(0.106)	(0.351)			
N	3400	3400	3400	3400	3400
Kleibergen-Paap Wald Rank F Stat		120.99			
Year FE	Yes	Yes	Yes	Yes	Yes
Cross-Sectional FE	Yes	Yes	Yes	Yes	Yes

Notes: We report the results of panel regressions of different dependent variables on road quality (both in logs). All regressions include year fixed effects and cross-sectional fixed effects. Column 3 reports the p-value of a chi-square test of the significance of the β coefficient on log road quality from the baseline clustering specification in column 2. Column 4 reports a Wild Bootstrapped version of this test, with clustering at the district level. Column 5 reports a Wild Bootstrapped version of this test, with clustering at the province level. Both columns 4 and 5 use 999 replications. */**/*** denotes significant at the 10% / 5% / 1% levels.

Table A.15: Road Quality and District-Level Manufacturing Outcomes: Nearby Districts

	IV-GMM	
	Local	$d \leq 50$
Panel A: Firm Counts	**(1)**	**(2)**
Log Number of Opened Firms	1.206***	-0.082
	(0.204)	(0.656)
Log Number of Closed Firms	0.125	-0.054
	(0.287)	(0.868)
Percent Δ Number of Firms	0.303***	0.087*
	(0.088)	(0.046)
Panel B: Production		
Log Output	1.654***	0.513
	(0.507)	(0.481)
Log Value Added	1.401***	0.708
	(0.479)	(0.452)
Log Total Employment	0.405	0.273*
	(0.255)	(0.147)
Log Output per Worker	1.243***	0.309
	(0.351)	(0.365)
Year FE	Yes	Yes
District FE	Yes	Yes

Notes: We report the results of district-level panel regressions of the dependent variable on road quality. Column 1 replicates the results from Table 3, while column 2 defines outcomes based on firm counts and production for all districts within 50 km of the given district. All regressions include district and year fixed effects. Robust standard errors, clustered at the district level, are reported in parentheses. */**/*** denotes significant at the 10% / 5% / 1% levels.

Table A.16: Estimating the Housing Expenditure Share

	Actual Rent			Imputed Rent		
Expenditure Quintile	**Mean**	**Median**	N	**Mean**	**Median**	N
1	0.116	0.092	526	0.119	0.088	5287
2	0.108	0.088	666	0.095	0.071	5872
3	0.097	0.081	733	0.091	0.061	6025
4	0.089	0.071	872	0.092	0.059	6030
5	0.080	0.057	734	0.124	0.065	6240

Notes: Authors' calculations, using IFLS-1 data.

Table A.17: Expenditure Shares of Non-Housing Consumption

	Mean			Median			
Expenditure Quintile	**Perishable**	**Non-Perishable**	**Non-Food**	**Perishable**	**Non-Perishable**	**Non-Food**	N
1	0.102	0.579	0.319	0.083	0.591	0.294	1392
2	0.106	0.545	0.349	0.093	0.551	0.331	1438
3	0.117	0.505	0.378	0.106	0.511	0.365	1439
4	0.112	0.455	0.433	0.106	0.450	0.424	1440
5	0.096	0.350	0.553	0.082	0.331	0.569	1442

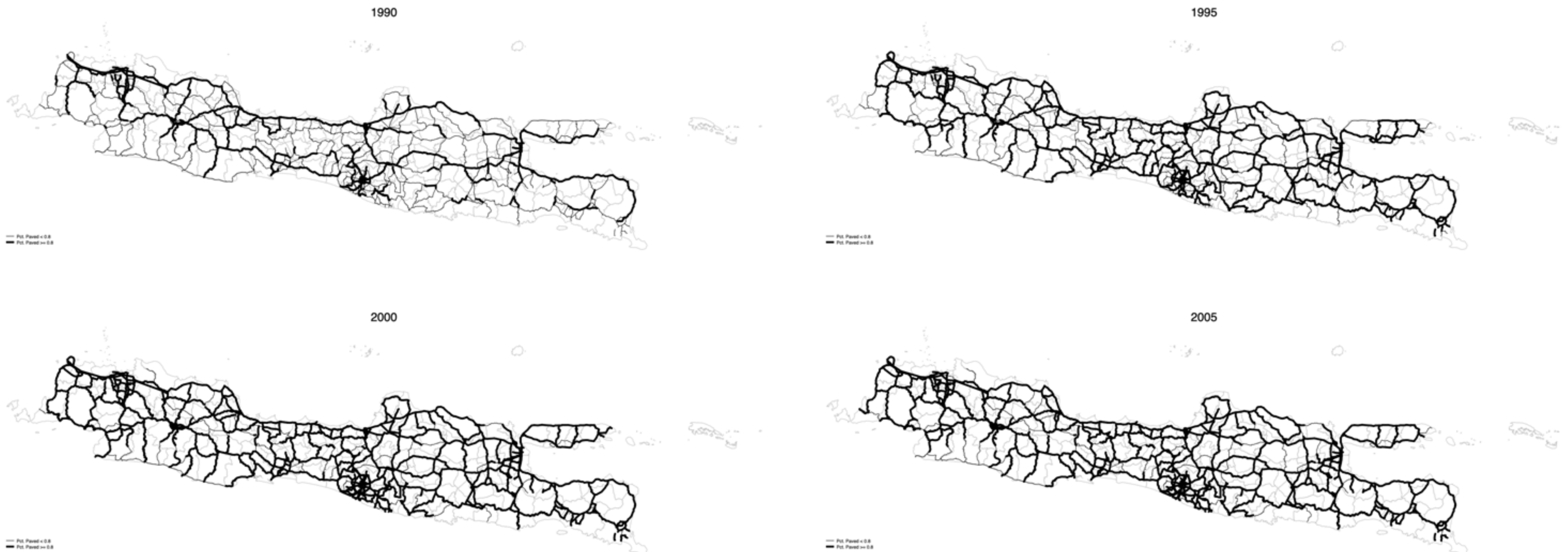
Notes: Authors' calculations, using IFLS-1 data.

Table A.18: Estimating Road Deterioration Profiles

	Unpaved Roads	Paved Roads
	(1)	**(2)**
Years Since Last Upgrade	0.120	0.063
	(0.002)***	(0.000)***
N	51,198	743,569
Adjusted R^2	0.67	0.66
F Statistic	454.8	3496.1
Road Segment FE	Yes	Yes
Year FE	Yes	Yes

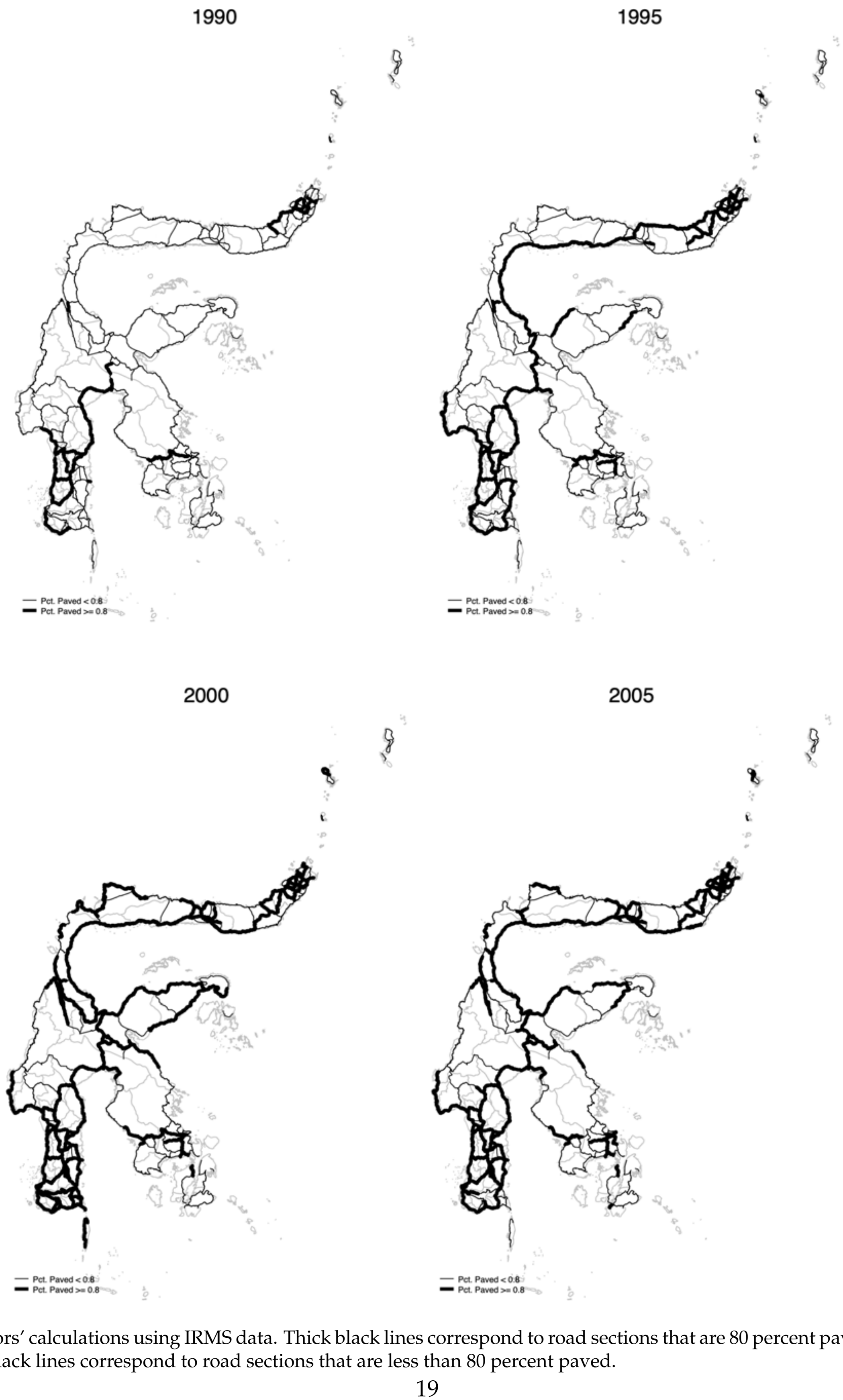
Notes: Robust standard errors, clustered at the road segment level, are reported in parentheses. */**/*** denotes significant at the 10% / 5% / 1% levels.

Figure A.1: Evolution of Paved Surfaces on Java's Road Network



Notes: Authors' calculations using IRMS data. Thick black lines correspond to road sections that are 80 percent paved or greater, while thin black lines correspond to road sections that are less than 80 percent paved.

Figure A.2: Evolution of Paved Surfaces on Sulawesi's Road Network



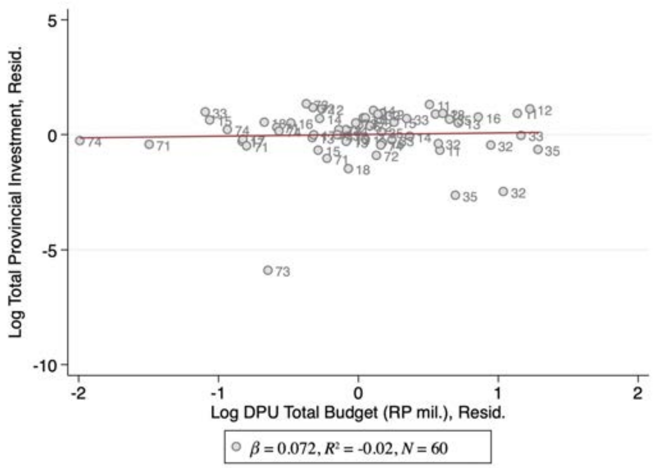
Notes: Authors' calculations using IRMS data. Thick black lines correspond to road sections that are 80 percent paved or greater, while thin black lines correspond to road sections that are less than 80 percent paved.

Figure A.3: IFLS Villages

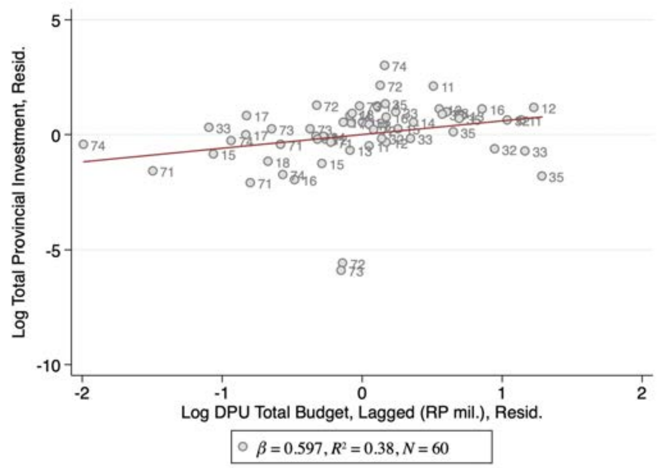
Notes: This figure displays a map of the original IFLS villages from wave 1 of the IFLS in 1993. The black polygons correspond to IFLS-1 communities, while the grey polygons display the locations of other communities in Indonesia.

Figure A.4: DPU Total Budget vs Total KM Upgraded

(A) CONTEMPORANEOUS

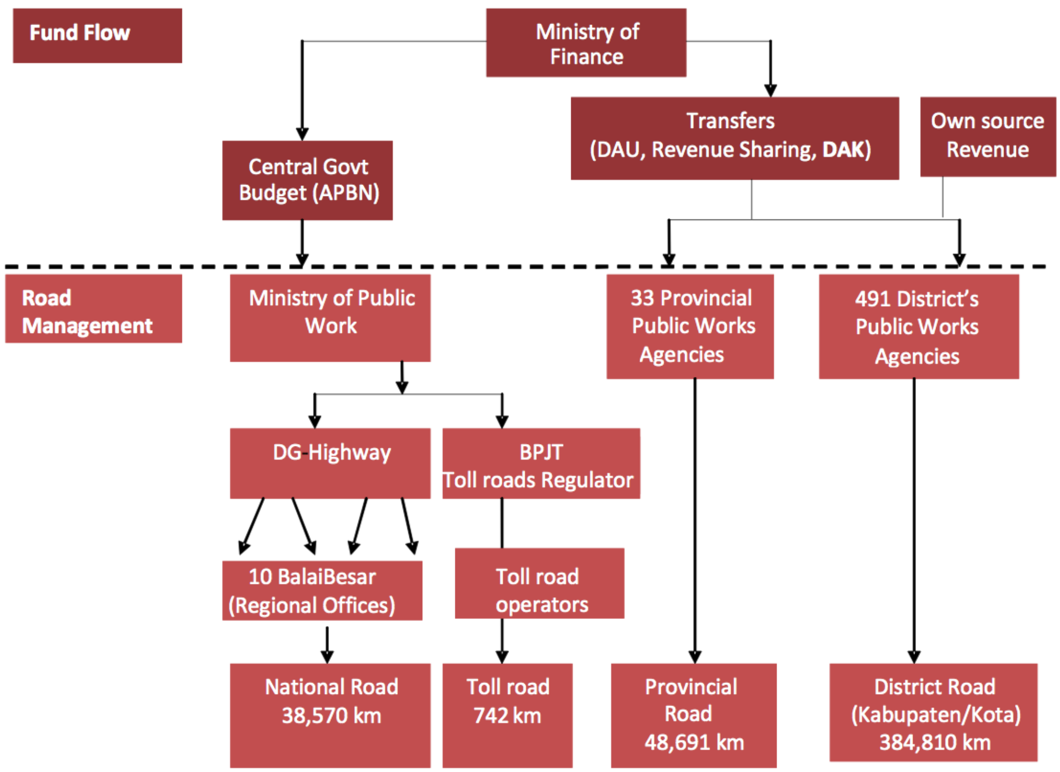


(B) 1 YEAR LAG



Notes: This figure reports residual-on-residual plots of the regression relationships estimated in Appendix Table A.2, Panel A, Columns 1 and 2.

Figure A.5: Institutional Arrangements for Indonesia's Road Sector



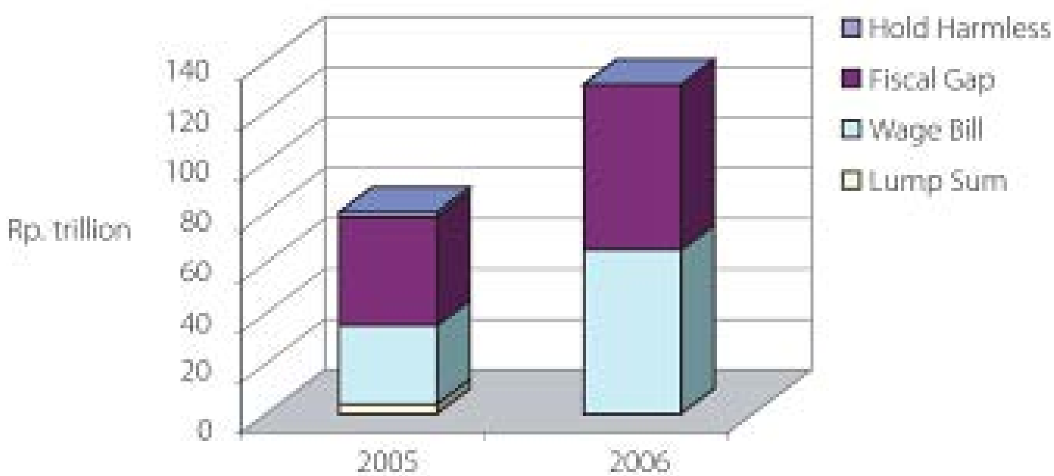
Source: World Bank (2012).

Figure A.6: The evolution of technical criteria in the DAK formula for roads and their respective weights

No.	Technical criteria	Description	2008	2009	2010	2011
1	Length of road	Length of road which is legally acknowledge through the decree of the head of local government	30%	25%	25%	25%
2	Road condition	Length of road with non-stable condition	30%	40%	35%	25%
3	Good road performance		20%			
4	Accessibility	Defined by the length of road divided by total area			20%	10%
5	Mobility	Length of road per 1000 population in the province/kabupaten			20%	10%
6	Ownership/concern by LG	Determined by the percentage of original APBD allocated to the road sector		20%		10%
7	Reporting	Consistency in submitting of quarterly report, physical progress, financial progress	20%	15%		20%

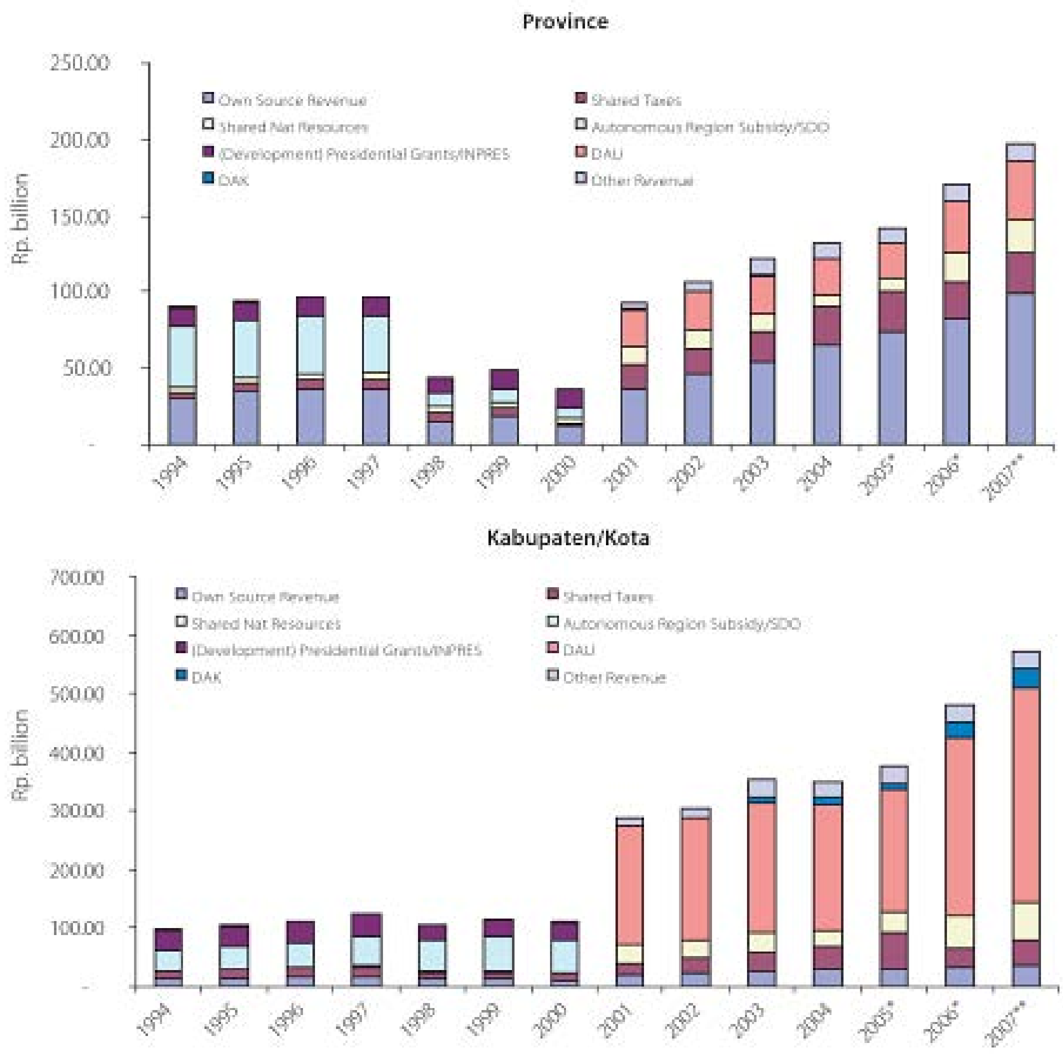
Source: World Bank (2012).

Figure A.7: Changes in DAU composition over time



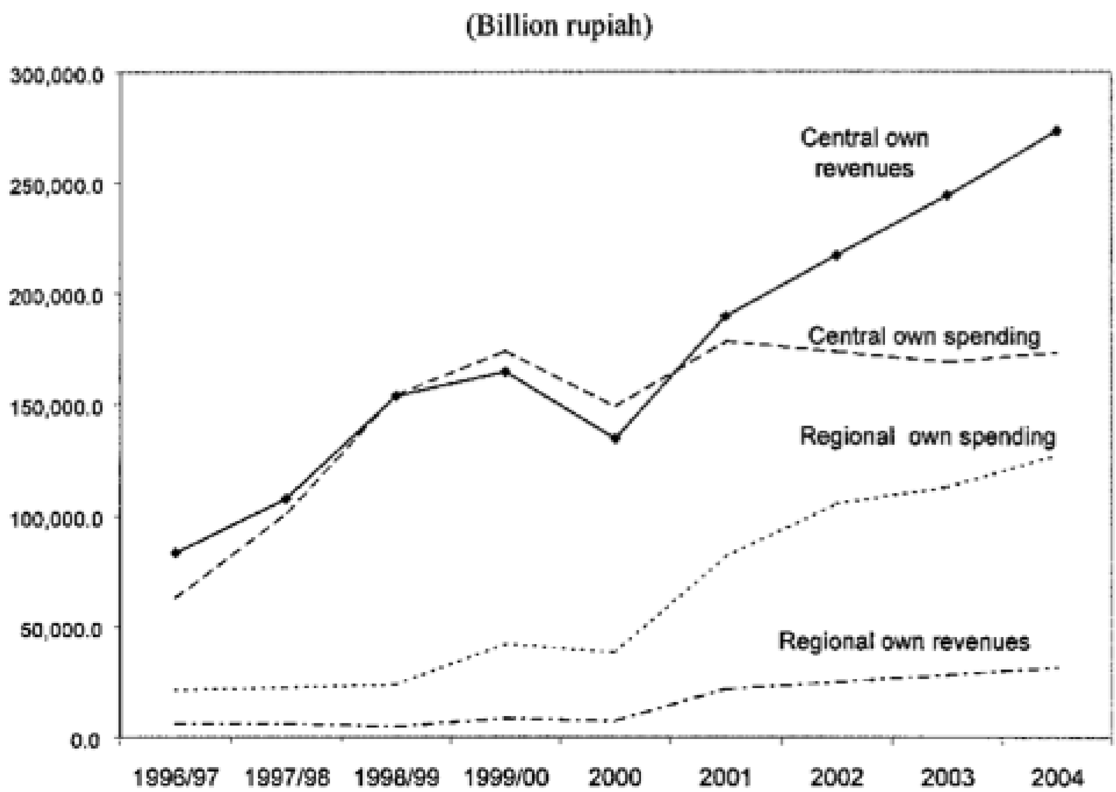
Notes: World Bank staff calculations, from World Bank (2008).

Figure A.8: Sub-national Revenue over Time



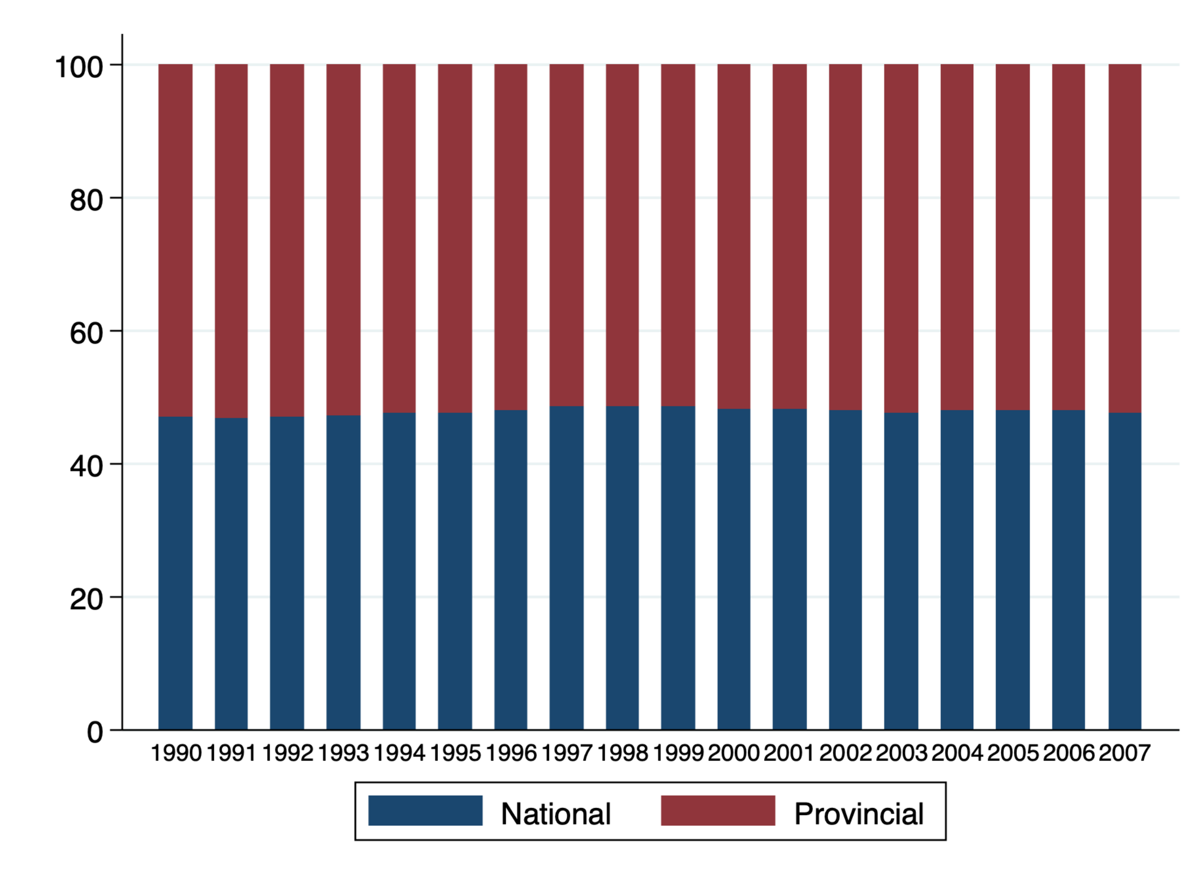
Notes: World Bank staff calculations, from World Bank (2008).

Figure A.9: Growth in Revenues for Road Maintence



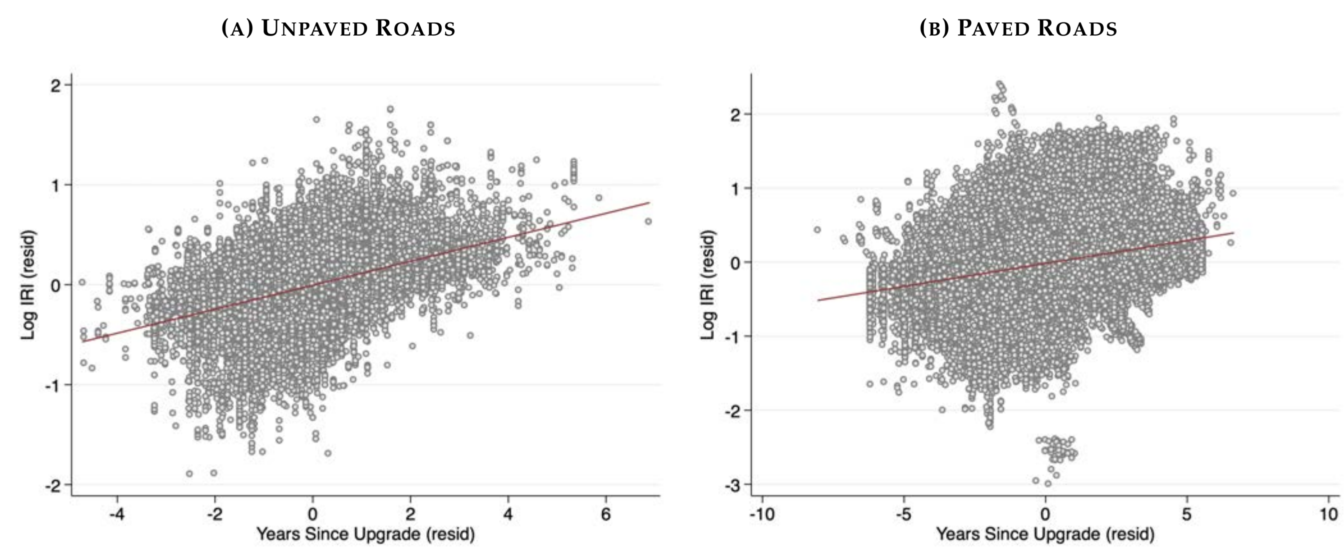
Notes: From Ahmad and Mansoor (2002), in billions of rupiah.

Figure A.10: Shares of Total Road Length by Administrative Authority



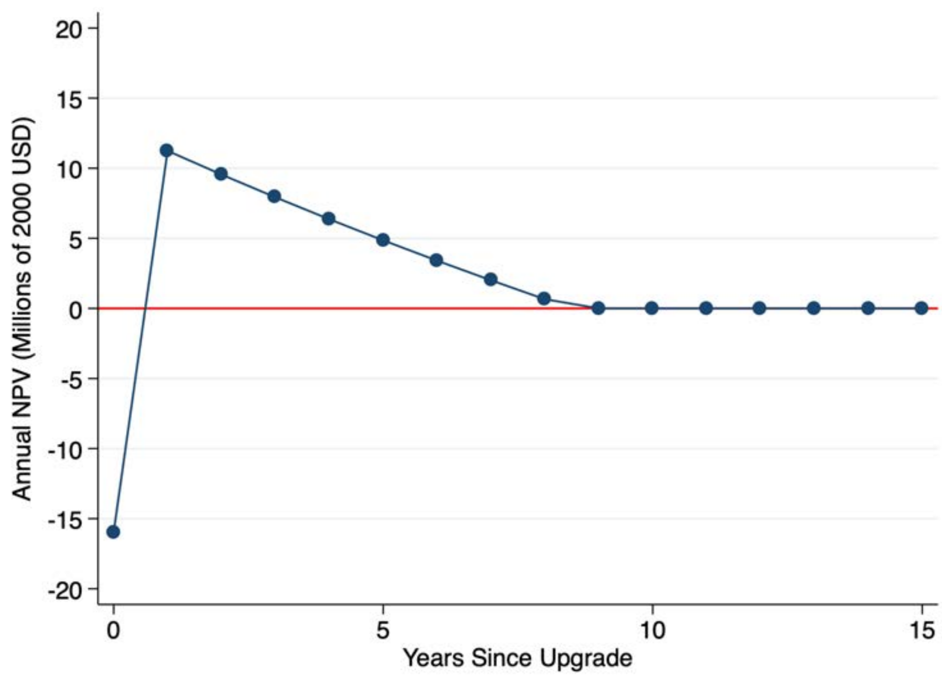
Notes: This figure plots the share of national and provincial roads in total roads covered by IRMS data. The figure is restricted to road quality observations taken from Java, Sumatra, and Sulawesi.

Figure A.11: Road Roughness and Years Since Upgrade: Residual-on-Residual Plots



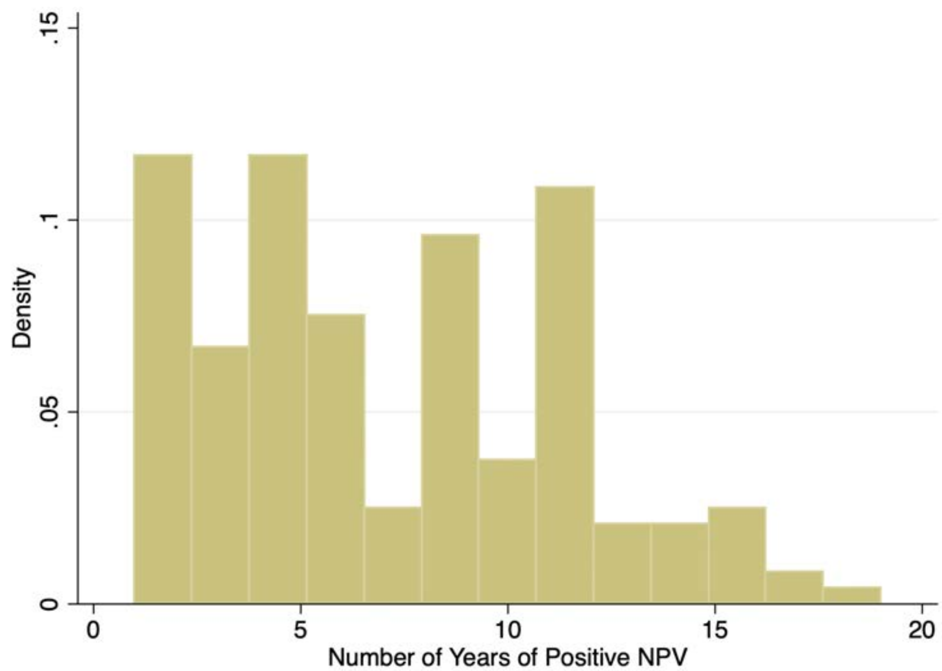
Notes: This figure reports residual-on-residual plots of the regression relationships estimated in Appendix Table A.18.

Figure A.12: Annual Benefits vs. Costs of Upgrading Roads: District Labuhan Batu



Notes: This figure presents an example of the annual benefits minus the costs of upgrading roads for District Labuhan Batu in North Sumatra. The entire cost of the upgrade is borne in the first year, while the benefits accrue for 9 years before deterioration reduces road conditions back to their previous levels.

Figure A.13: Distribution of Years that NPV is Positive



Notes: This figure reports a histogram of the number of years in which the net present value of the counterfactual district road upgrading program is positive. The median district with positive NPV has 6 years of positive benefits before road deterioration reduces road quality back to its previous levels.

B Data Appendix

B.1 Road Quality Data

Data on the quality of Indonesia's highway networks were produced by DPU as part of Indonesia's Integrated Road Management System (IRMS). This appendix section begins by providing some background on road management in Indonesia, describing the road classification system and discussing IRMS coverage. It then discusses the measures of road quality that are collected in IRMS and how they are measured. We then discuss how the road network data were created.

B.1.1 Background on Road Management

Indonesia's national road network is currently managed and maintained by the Department of Public Works (*Kementerian Pekerjaan Umum dan Perumahan Rakyat*, or Kementerian PUPR), specifically by the Directorate General of Highways (*Direktorat Jenderal Bina Marga*). According to Law No. 38, 2004, roads are classified into four different types, primarily based on their function for users. Arterial roads (*jalan arteri*) serve as the major transportation linkages between urban areas, and are characterized by longer distances, higher speeds, and limited access. Speeds are meant to be a minimum of 60 km/h, and width should be at least 11 meters to accommodate larger traffic volumes. Collector roads (*jalan kolektor*) serve "collector or distributor transportation" and are characterized by medium distance travel with medium speeds. Collector roads are subdivided into primary collector roads (*jalan kolektor primer*), which should have a minimum speed of 20 km/h and width of 9 meters, and secondary collector roads, which should have a minimum speed of 20 km/h and width of 9 meters. Local roads (*jalan lokal*) and Neighborhood Roads (*jalan lingkungan*) serve local areas at lower speeds, and are characterized by unlimited access.

Roads can also be classified by their management authority, or "status" (*wewenang penyelenggaraan*). Generally, arterial and primary collector roads are managed by the national government (specifically by Kementerian PUPR). Secondary and tertiary collector roads are managed by provincial governments, while local and neighborhood roads are managed by the kabupaten, kecamatan, and desa governments. Table B.1 describes the road classification system, minimum speed and width guidelines, and management authorities.

Table B.2 depicts the coverage of the IRMS dataset by road function and managing authority, as measured by counts of the number of kilometer-post observations that appear in the entire dataset. Most of the observations, and indeed most of the road network, is made up by collector roads (K1-K3), though the category with the next largest coverage consists of arterial roads. Local and neighborhood roads are not very well surveyed in the IRMS dataset.

B.1.2 Measures of Road Quality

There are a number of different devices that transport engineers have developed to collect measurements of road quality, and there are several different measures of road quality. The most widely used measure of road roughness, and the measure used in this study, is the international roughness index (IRI), developed by the World Bank in the 1980s. IRI is constructed as a filtered ratio of a standard vehicle's accumulated suspension motion (in meters), divided by the distance travelled by the vehicle during measurement (in kilometers). Expressed in units of slope (m/km), IRI is a characteristic of a vehicle's longitudinal profile. Importantly, since it is a measure of a physical quantity, IRI is standardized, as opposed to other subjective measures of ride quality. Figure 1 shows the relationship between different ranges of IRI and surface type; generally, larger roughness levels correspond to worse surfaces, but the mapping is not one-to-one.

Bennett et al. (2007) distinguish between several different types of devices for measuring road roughness and provide a good overview of their relative strengths and weaknesses. Over the course of its existence, Indonesia's IRMS has largely made use of two different types of measuring devices.[46] Before 1999, roads were surveyed using devices like the ROMDAS, which estimate IRI indirectly. The ROMDAS machine is a bump integrator, which first must be calibrated and second, estimates IRI from correlation equations. It is very useful for measuring roughness on bumpy roads and can record high levels of IRI, but the device must be calibrated manually, and measurement error can occur if the device is miscalibrated.

The ROMDAS device is also portable, meaning that it can be used inside different vehicles (each of which would require unique calibrations). The portability contrasts with devices like the high-speed laser profilometer, which is essentially a separate vehicle reserved entirely for the purposes of measuring road quality. The device uses lasers and optical techniques to scan the road as it is traversed and create measures of surface profiles. These instruments are very accurate, but are much more expensive. Moreover, they might become mis-calibrated on extremely rough roads. Indonesia started using the high speed laser profilometer for collecting its road quality data in 1999, licensing vehicles from the Australian government.

Road width and surface type are more straightforward variables to measure, involving visual inspection and simple measurement. In Figure 2 and Appendix Figures A.1 and A.2, we categorize a kilometer-post interval as being unpaved if it is either an earth, gravel, or sand road, or if it was given a granular base (crushed stone) treatment, a first step in the process of paving.

B.1.3 Creation of Road Network Data

Using GIS shapefiles of the road network provided by Kementerian PUPR, we have georeferenced the kilometer post observations of road quality, in order to capture the evolution of Indonesia's transportation network over space and time. This proved to be a challenging exercise, because the identifiers for each road-link-interval observation were not consistent over time, and because the identifiers in the shapefile and in the linearly referenced dataset were often different, even though both did refer to exactly the same link.

Once the IRMS interval data was successfully merged to the regional network shapefiles, we converted the GIS database of road links into a weighted graph of arcs and nodes, as commonly used in the transportation literature. Nodes represent locations (such as ports, cities, or the centroids of kabupatens, my unit of analysis), arcs represent the possibility of traveling between two nodes, and weights represent the cost of moving goods along a given arc. Weights were constructed according to the IRMS data on road quality, and for simplicity, the cost of moving along each road was assumed to be the same, no

[46] I am very grateful for the extensive discussions I've had with Glen Stringer about IRMS; this section of the appendix benefits highly from our conversations.

matter which way you were traveling.[47]

For computational reasons, we have used a simplified representation of Indonesia's road network, where the number of nodes and links was small enough for network algorithms to operate on it using a desktop computer.[48] Table B.3 depicts the number of network arcs, the total distance of the network, and merge statistics for the kilometer-post observations. Merge statistics are pretty good for arterial and collector roads, but the quality of merges falls substantially for local and neighborhood roads, due most likely to poor shapefile coverage for that type of road network.

The interval observations were not matched directly to their exact locations in the network, because we had no knowledge of the exact location of the kilometer posts. To deal with this, I first aggregated the kilometer-post interval observations to the road-link level by constructing distance-weighted averages of the road quality variables. Each network arc-year observation was then assigned the value of this average road quality variable that corresponds to its road link.[49]

B.1.4 Roughness, Speed, and Ride Quality

One effect that rough roads have on vehicles is that they require the driver to travel at lower speeds. When faced with potholes, ragged pavement, or poor surfaces, drivers slow down, and this reduction in speed increases travel time and hence the cost of travel. Of course, there is not a one-to-one relationship between road roughness and speed, because drivers choose the speed at which they travel, and different preferences for smoothness of the ride or the desired arrival time might induce different choices of speed.

Yu et al. (2006) explore the relationship between *jolt*, or the "jerk" experienced by road users, and subjective measures of ride quality and road roughness at different speeds.[50] Using survey data in which users were asked to rate the quality of particular rides, the authors find that people experience greater discomfort while traveling at higher speeds on rough roads, but lowering speed on rough roads can reduce discomfort. The authors provide a mapping between subjective measures of ride quality and roughness at different speeds, and this mapping can be used to infer the maximum speed that one can travel in order to achieve a ride of a certain quality, given pavement roughness. Table B.4 reproduces this mapping. Because travel times were unreasonably long for high quality rides given Indonesia's rough roads, and because the subjective quality measures were chosen by Western drivers, we have focused on the poor ride quality speed thresholds in our empirical work.

Given the maximum speed that one can travel on roads of different roughness levels, it is straightforward to calculate travel times for each network arc, the primary measure of transport costs used in this study. Note that the travel times on road sections were computed using the detailed kilometer-

[47] Another tedious issue involved the construction of junction points where the road links intersected. The shapefiles were originally stored as MapInfo files, an older shapefile format that required conversion for use with Arcview. In the process of conversion, information on where the roads crossed was lost, requiring painstaking editing. The shapefiles were also not designed to be used in any network analysis, so much care had to be taken to make them usable.

[48] The road lines were straightened using the "Generalize" command from ET Geotools, which employs the Ramer-Douglas-Peucker algorithm for reducing the number of points that represents a line.

[49] In some cases, when a network arc had no data for a particular year, I assigned the network arc the average value of road quality for arcs with the same function. This was done because constructing the transport cost variables involved a search over the entire network, and if certain network arcs were coded as missing, this could distort the search substantially. Overall, imputation amounted to no more than 5 percent of network arc observations in any given year.

[50] *Jolt* is officially defined as the vector that specifies the time-derivative of acceleration; in other words, the third derivative of the vertical displacement of vehicle to time t.

post interval roughness data. These were then aggregated to the network arcs using distance-weighted averages.

B.2 Administrative Boundaries

Administrative boundary shapefiles were constructed by BPS for use during the 2000 Household Census. These shapefiles contain the polygon boundaries of all provinces, kabupatens, kecamatans, and desas for the entire extent of the Indonesian archipelago. However, after the fall of Suharto and a massive decentralization program, many new kabupatens were created, splitting existing kabupatens into new ones. For instance, in 1990 there were 290 kabupatens and kotas, but by 2003, there were 416 kabupatens and kotas. The fact that administrative boundaries are not fixed over time create difficulties for the analysis.

Because of the need for a geographic unit of analysis that was consistently defined over time, we use kabupaten borders as they were defined in 1990. BPS provided the administrative boundary shapefile for 2000, as well as a correspondence table between kabupaten codes in 2000 and kabupaten codes from 1990 to the present. This information was processed using ArcView to create the 1990 shapefiles that form the basis of the analysis. Throughout the paper, all survey data were appropriately merged back to the 1990 kabupaten definitions.

Table B.1: Indonesia's Road Classification System

Function	Code	Minimum Speed	Minimum Width	Management Authority
Arterial	A	60 km/h	11 m	National
Collector-1	K1	40 km/h	9 m	National
Collector-2	K2	20 km/h	9 m	Provincial
Collector-3	K3	20 km/h	9 m	Provincial
Local	L	20 km/h	7.5 m	Kabupaten & Desa
Neighborhood	Z	15 km/h	6.5 m	Kabupaten & Desa

Source: Departemen Pekerjaan Umum, 2008

Table B.2: Road Function and Managing Authority, Kilometer-Post Observations, 1990-2007

	Road Function			Managing Authority		
	Code	**Number of Obs.**	**Share of Total**	**Code**	**Number of Obs.**	**Share of Total**
	A	52,917	0.17	**N**	93,808	0.30
	K1	40,889	0.13	**P**	132,649	0.42
	K2	121,386	0.39	**K**	15,862	0.05
Java	**K3**	10,714	0.03	**S**	72,068	0.23
	L	15,862	0.05			
	Z	72,619	0.23			
	Total	314,387	1.00	**Total**	314,387	1.00
	A	103,160	0.20	**N**	202,915	0.39
	K1	99,782	0.19	**P**	263,409	0.50
	K2	235,750	0.45	**K**	11,391	0.02
Sumatra	**K3**	27,632	0.05	**S**	45,680	0.09
	L	11,391	0.02			
	Z	45,680	0.09			
	Total	523,395	1.00	**Total**	523,395	1.00
	A	54,496	0.21	**N**	143,147	0.54
	K1	87,728	0.33	**P**	72,198	0.27
	K2	71,234	0.27	**K**	18,232	0.07
Sulawesi	**K3**	1,887	0.01	**S**	29,371	0.11
	L	18,232	0.07			
	Z	29,371	0.11			
	Total	262,948	1.00	**Total**	262,948	1.00

Source: IRMS and authors' calculations. Data come from kilometer-post observations. Standard deviations in parentheses.

Table B.3: Number of Network Arcs, Distances, and Merge Statistics (by road function)

		Road Function						
		A	**K1**	**K2**	**K3**	**L**	**Z**	**Miss**
	# of Arcs	1168	889	2618	309	315	37	.
	# of Road IDs	220	129	354	43	72	6	.
	Total Distance	2944.91	1970.65	5832.59	750.39	663.44	92.16	.
	Link-Years Merged	16538	13685	38719	3876	4689	14572	3015
Java	**Link-Years Unmerged**	1838	735	1842	45	971	21772	157
	% Merged	0.90	0.95	0.95	0.99	0.83	0.40	0.95
	Arc-Years Merged	20,844	16002	46350	5562	5670	666	.
	Arc-Years Unmerged	180	0	774	0	0	0	.
	% Merged	0.99	1.00	0.98	1.00	1.00	1.00	.
	# of Arcs	1485	1205	2975	453	277	22	41
	# of Road IDs	207	165	412	87	66	6	13
	Total Distance	4964.69	4469.43	11551.28	1492.97	571.67	56.44	147.56
	Link-Years Merged	24755	20035	49171	6808	2603	8730	1406
Sumatra	**Link-Years Unmerged**	718	373	537	52	394	9722	12
	% Merged	0.97	0.98	0.99	0.99	0.87	0.47	0.99
	Arc-Years Merged	26730	21690	51876	7830	4986	396	0
	Arc-Years Unmerged	0	0	1674	324	0	0	738
	% Merged	1.00	1.00	0.97	0.96	1.00	1.00	0.00
	# of Arcs	1624	2319	2051	15	391	.	45
	# of Road IDs	113	116	150	4	44	.	1
	Total Distance	2836.96	3805.92	4369.33	28.35	732.96	.	70.34
	Link-Years Merged	24006	24006	34711	30911	551	5670	5674
Sulawesi	**Link-Years Unmerged**	25	356	410	339	9	118	4755
	% Merged	1.00	0.99	0.99	0.99	0.98	0.98	0.54
	Arc-Years Merged	25794	35694	33660	270	7038	.	0
	Arc-Years Unmerged	3438	6048	3258	0	0	.	810
	% Merged	0.88	0.86	0.91	1.00	1.00		0.00

Source: IRMS and authors' calculations. Missing function information is attributable to poorly coded shapefiles. Arc-Years could be unmerged potentially because there were no surveys done on that particular link; statistics are computed assuming a balanced panel. Road IDs are defined in the shapefile, while Link IDs are defined from the IRMS data.

Table B.4: Roughness and Ride-Quality Speed Limits

Max Speed	Good	Fair	Mediocre	Poor
120 km/h	IRI $\in [0.00, 1.49]$	IRI $\in [0.00, 1.89]$	IRI $\in [0.00, 2.70]$	IRI $\in [0.00, 3.24]$
100 km/h	IRI $\in [1.49, 1.79]$	IRI $\in [1.89, 2.27]$	IRI $\in [2.70, 3.24]$	IRI $\in [3.24, 4.05]$
80 km/h	IRI $\in [1.79, 2.24]$	IRI $\in [2.27, 2.84]$	IRI $\in [3.24, 4.05]$	IRI $\in [4.05, 4.63]$
70 km/h	IRI $\in [2.24, 2.57]$	IRI $\in [2.84, 3.25]$	IRI $\in [4.05, 4.63]$	IRI $\in [4.63, 5.40]$
60 km/h	IRI $\in [2.57, 2.99]$	IRI $\in [3.25, 3.79]$	IRI $\in [4.63, 5.40]$	IRI $\in [5.40, 6.25]$
50 km/h	IRI $\in [2.99, 3.59]$	IRI $\in [3.79, 4.54]$	IRI $\in [5.40, 6.25]$	IRI $\in [6.25, 8.08]$
40 km/h	IRI $\in [3.59, 4.49]$	IRI $\in [4.54, 5.69]$	IRI $\in [6.25, 8.08]$	IRI $\in [8.08, 10.80]$
30 km/h	IRI $\in [4.49, 5.99]$	IRI $\in [5.69, 7.59]$	IRI $\in [8.08, 10.80]$	IRI $\in [10.80, 16.16]$
20 km/h	IRI $\in [5.99, 8.99]$	IRI $\in [7.59, 11.39]$	IRI $\in [10.80, 16.16]$	IRI $\in [16.16, 32.32]$
10 km/h	IRI $\in [8.99, \infty)$	IRI $\in [11.39, \infty)$	IRI $\in [16.16, \infty)$	IRI $\in [32.32, \infty)$

Source: Authors' calculations and Yu et al. (2006), Table 2. IRI denotes the international roughness index, measured in m/km. Ride quality levels are subjective and measured on a 5-point scale ("Very Good", "Good", "Fair", "Mediocre", and "Poor").

C Welfare Decomposition Appendix

In this section, we provide an approximation for how a marginal improvement in road quality impacts welfare for incumbent households. To do so, we combine reduced form estimates of the effects of road quality on outcomes in different types of locations, presented in Section 6, with a simple model of household utility maximization.

Our simple framework clarifies the ways that a small change in road quality impacts households through its effects on different sources of income and on different prices. To present this framework, we proceed with a bit of notation. Let i index households and let $U_i = U(\mathbf{c}_i, H_i)$ denote household i's utility function, which is defined over a vector of consumer goods, $\mathbf{c}_i = (c_{i1}, c_{i2}, ..., c_{iK})'$, and housing, H_i.

Ignoring subscripts, the general household's utility maximization problem is given by:

$$\max_{\mathbf{c},H} U(\mathbf{c}, H) \quad \text{s.t.} \quad Y = \sum_{k=1}^{K} p_k c_{ik} + p_H H$$

where p_k denotes the consumer price of good k, p_H represents housing prices, and Y collects the household's total income from all potential sources.

Specializing to a Cobb-Douglas functional form, we can write:

$$\max_{\mathbf{c}_i,H_i} \left(\prod_{k=1}^{K} c_{ik}^{\alpha_k}\right) H_i^{\alpha} \quad \text{s.t.} \quad Y_i = \sum_{k=1}^{K} p_k c_{ik} + p_H H_i \tag{C.1}$$

where $\{\alpha_k\}_{k=1}^{K}$ and α are constants, and where we assume that $1 = \sum_{k=1}^{K} \alpha_k + \alpha$. Given (C.1), it is straightforward to show that the household's indirect utility function is given by:

$$V = \left(\prod_{k=1}^{K} \alpha_k^{\alpha_k}\right) \cdot \alpha^{\alpha} \cdot Y \cdot \left(\prod_{k=1}^{K} p_k^{-\alpha_k}\right) \cdot p_H^{-\alpha} \tag{C.2}$$

Taking logs of (C.2), we have:

$$\ln V = \underbrace{\left(\sum_{k=1}^{K} \alpha_k \ln \alpha_k\right) + \alpha \ln \alpha}_{\text{cons}} + \ln Y - \sum_{k=1}^{K} \alpha_k \ln p_k - \alpha \ln p_H$$

$$\ln V = \text{cons} + \ln Y - \sum_{k=1}^{K} \alpha_k \ln p_k - \alpha \ln p_H$$

Totally differentiating this expression with respect to road quality, A, we obtain:

$$\frac{\partial \ln V}{\partial \ln A} = \underbrace{\frac{\partial \ln Y}{\partial \ln A}}_{(I)} - \underbrace{\sum_{k=1}^{K} \alpha_k \frac{\partial \ln p_k}{\partial \ln A}}_{(II)} - \alpha \underbrace{\frac{\partial \ln P_H}{\partial \ln A}}_{(III)}$$

This expression tells us that changing road quality impacts the household's welfare through its effect on total income (I), through its impact on consumer prices (II), and through its impact on housing prices (III).

To make progress on the first term, (I), we note that Y is the total wage income that the household earns from a variety of sectors, plus the total net profits from farm and non-farm business income:

$$Y = Y_L + \pi$$

where Y_L measures labor income and π measures the household's combined farm and non-farm business profits. Using this, we can rewrite (A) as follows:

$$\begin{aligned}
\frac{\partial \ln Y}{\partial \ln A} &= \frac{1}{Y}\left[\frac{\partial Y_L}{\partial \ln A} + \frac{\partial \pi}{\partial \ln A}\right] \\
&= \frac{1}{Y}\left[\frac{Y_L}{Y_L}\frac{\partial Y_L}{\partial \ln A} + \frac{\pi}{\pi}\frac{\partial \pi}{\partial \ln A}\right] \\
&= \frac{1}{Y}\left[Y_L\left(\frac{1}{Y_L}\frac{\partial Y_L}{\partial \ln A}\right) + \pi\left(\frac{1}{\pi}\frac{\partial \pi}{\partial \ln A}\right)\right] \\
&= \frac{1}{Y}\left[Y_L\left(\frac{\partial \ln Y_L}{\partial \ln A}\right) + \pi\left(\frac{\partial \ln \pi}{\partial \ln A}\right)\right]
\end{aligned}$$

where we multiply by all derivatives by 1 in the second line, and we use the relationship that

$$\frac{\partial y}{\partial \ln x} \cdot \frac{1}{y} = \frac{\partial \ln y}{\partial \ln x}$$

in the third line.[51]

The second term, (II), is just a weighted sum of the elasticities of prices with respect to road quality across goods, where the weights are equal to expenditure shares. The third term, (III), is the elasticity of housing prices with respect to road quality, multiplied by α, the share of the consumer's income spent on housing.

Let $\mathcal{E}_{y,x}$ denote the elasticity of y with respect to x, and define $\theta_{Y_L} \equiv Y_L/Y$ and $\theta_\pi \equiv \pi/Y$ as the share of labor income and total profits in total income, respectively. Given this notation, we can write our expression for how welfare changes for a marginal improvement in road quality as follows:

$$\mathcal{E}_{V,A} = \theta_{Y_L}\mathcal{E}_{Y_L,A} + \theta_\pi \mathcal{E}_{\pi,A} - \sum_{j=1}^{J} \alpha_j \mathcal{E}_{p_j,A} - \alpha \mathcal{E}_{p_H,A} \tag{C.3}$$

[51]To prove this, let $z = \exp y$, so that $\ln z = y$. Then, we have:

$$\begin{aligned}
\frac{\partial y}{\partial \ln x} &= \frac{\partial \ln z}{\partial \ln x} = \frac{\partial z}{\partial x} \cdot \frac{x}{z} \\
\implies \frac{\partial y}{\partial \ln x} &= \frac{\partial \exp y}{\partial x} \cdot \frac{x}{\exp y} \\
\implies \frac{\partial y}{\partial \ln x} &= \exp y \cdot \frac{\partial y}{\partial x} \cdot \frac{x}{\exp y} \\
\implies \frac{\partial y}{\partial \ln x} &= \frac{\partial y}{\partial x} \cdot x \\
\implies \frac{\partial y}{\partial \ln x} \cdot \frac{1}{y} &= \frac{\partial y}{\partial x} \cdot \frac{x}{y}
\end{aligned}$$

This is identical to equation (2) in the text.