Reply to "Comment on "Development Effects of Electrification: Evidence from the Topographic Placement of Hydropower Plants in Brazil"

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Introduction Lipscomb et al. [2013a] developed an instrumental variables strategy that took advantage of topographic requirements for hydropower generation in order to estimate the development effects of electrification. We update the results of that paper based on four considerations, the first three of which are highlighted by Ankel-Peters et al.: (1) The engineering model used to derive the instrument is based on simulated annealing. This means that there is some instability in the estimated constructed grid based on the randomized allocation from alternative grids proposed by the model. We show that this can be eliminated, and the instrument improved, through averaging the predictions of the model over multiple model runs instead of relying on a single run as was done in the original paper. (2) There are different possible definitions of the Amazon region, depending on whether one uses official jurisdictional boundaries or the ecological definition of the Amazon biome. The original paper used the legal Amazon definition in the "engineering model" for construction of the instrument, but the Amazon together with the Pantanal in the controls applied (to proxy for areas with high costs of building, which we wanted to separately control for) in the 2SLS regression specification. We now eliminate this inconsistency by using the Amazon biome as defined by the Brazilian Institute of Renewable and Natural Resources (IBAMA) across all stages of empirical analysis. And instead of controlling for an indicator for Amazon and Pantanal regions as a proxy for high construction costs, we now control directly for the cost of building dams. (3) Lipscomb et al. [2013a] studied the effects of electrification on "Housing values" and the UN "Human Development Index" as their two summary measures of the county's overall development. Ankel-Peters et al. correctly notes that "housing values" is estimated by the Brazilian government research agency IPEA using a hedonic analysis, and electricity access is one of the inputs used in that analysis. This creates potential for bias in any analysis of the effects of electrification on housing values. We therefore replace housing values with log GDP per capita as an alternative summary measure of the development effects of electricity. (4) A recent applied econometrics paper [Lee et al., 2022] shows that a first-stage F-statistic of over 104 is necessary to produce unbiased estimates of standard errors in the second stage of IV estimates. We correct the standard errors according to the *tF* procedure suggested by Lee et al. [2022]. The revised standard errors are larger, and several results from the original paper are no longer statistically significant, although Lee et al. [2023] suggests that this correction is most likely overly conservative. We also correct an error in variable cleaning while producing these revised results.

How do these revisions change the results reported in Lipscomb et al. [2013a]? That 2013 publication had two parts: (a) the effects of electrification on two summary measures of development (Housing values - which we are now replacing with log GDP per capita, and the Human Development Index), and (b) exploration of the mechanisms by which electrification affects county development, by studying effects on education, migration, urbanization, etc. For the first part, our revised results remain similar to the effects of electrification on summary measures of development reported in Lipscomb et al. [2013a]. Electrification has a positive and statistically significant effect on HDI, though the effect is smaller in magnitude than those reported in Lipscomb et al. [2013a]. It also has a positive, statistically significant effect on log(GDP per capita).The original finding that the OLS estimates of the impact of electricity are biased downward relative to the 2SLS estimates also remains. But in the second part of Lipscomb et al. [2013a] – exploration of mechanisms – several of those intermediate "mechanisms variables" now show smaller impacts which are no longer statistically significant after the revisions. For example, effects on education are no longer statistically significant, but this was highlighted as a potential mechanism in Lipscomb et al. [2013a] by which electrification affects development.

This forces us to re-think the mechanisms, and we use our dataset to explore one of the major hypotheses proposed in prominent recent work on the links between electrification and development [Lee, Miguel, and Wolfram, 2020, Fetter and Usmani, 2024]. Lee, Miguel, and Wolfram [2020] hypothesize that that if households and firms face multiple constraints simultaneously (e.g. both a lack of energy and a lack of capital), then perhaps electrification would only improve welfare in already-richer jurisdictions with better access to capital, where households and firms can use the new energy access to invest in businesses and economic activities that improve their incomes. They argue that such development effects may only materialize in the long-run. Since we have unusually long panel data on electrification across Brazil from 1970 to 2000s [Lipscomb et al., 2013b], we show that it would be possible to test their hypothesis by splitting our sample between counties that are above- vs below-median 1970 GDP per capita [Data]. We see that indeed, industrial GDP and employment effects of electrification are larger in the already-richer half of the sample. A full exploration of these mechanisms is beyond the scope of this short note, so we leave it to future research to properly explore the mechanisms by which electrification affects development outcomes, to guide policy on targeting large-scale energy investments.

The Instrument To estimate the development effects of electricity, Lipscomb et al. [2013a] construct an instrument using variation in the cost of producing hydropower based on local geographic characteristics, together with variation in the budget for hydropower across decades. We predict the placement of electricity in an "engineering model" based only on cost factors and the national budget of Brazil for electricity generation, leaving out all demand considerations. The process starts with a probit regression to estimate the relative importance of each geographic factor (Table 1 in Lipscomb et al. [2013a]). This creates a "suitability index" that ranks every location in Brazil in terms of how sensible it is to place a hydropower dam at that location, if one were solely focusing on geographic factors. The underlying parameters for this index were water flow accumulation, average and maximum slope in the river, and an indicator for whether the point was in the Amazon. The Amazon coefficient was large and negative in this probit regression because building infrastructure in the rainforest is costly. Generation plants were therefore relatively unlikely to be placed there by the model. The time variation in the instrument comes from the fact that less and less ideal regions are available as non-electrified potential sites for new hydropower plants in later decades as the lowest cost locations are selected first. The most "suitable" locations that had not yet been electrified receive those plants together with simulated transmission lines and a distribution network which are allocated through simulated annealing.

Instrument Stability The "engineering model" which forms the basis of the instrument relies on simulated annealing which proposes alternative grids for the transmission lines in every decade, and compares the cost of the grid to the cost of alternative randomly generated grids. Cheaper grids are iteratively selected and the process repeats for 80,000 replications for each decade. This was by no means meant to be the best possible estimate of a model of Brazil's electricity grid, and the statistical advances in the field since 2013 have been significant–now machine learning tools offer straightforward alternatives for predicting placement of the grid. However, the simulated annealing algorithm provides us with a prediction of where the grid would be placed in each decade. Because simulated annealing relies on a randomized selection of alternative grids, there is natural variation in the final version of the grid that it achieves after 80,000 replications. There are two main reasons for differences in the final grid between replication seeds: (a) path-dependency – grid points categorized as electrified in 1960 can not be un-electrified in future decades – and (b) natural random

Table 1: Stability over different levels of averaging

	Instrument averaged over .... runs of the simulated annealing				
	5	25	50	100	500
Averaged Instrument	0.352^{***}	0.386^{***}	0.398^{***}	0.396^{***}	0.394^{***}
	[0.070]	[0.073]	[0.074]	[0.073]	[0.074]

Notes: Shown are alternative levels of averaging of the instrument in runs of the preferred specification of the first stage (including the quadratic in hydrographic cost factors interacted with decade as controls as well as county (AMC) fixed effects, and decade fixed effects). All regressions have county size weights. Standard errors are clustered at the AMC level.

variation. Ankel-Peters et al. notes that this leads to instability in the first and second stage estimates. We now run the model 500 times, and there is, as expected, some variation in the first stage coefficient estimates: the 1st percentile coefficient in our preferred specification with fixed effects by AMC and hydrographic cost factors interacted with decade controls is 0.12 while the 99th percentile is 0.32 with the F-stat varying from 14.1 in the first percentile to 23.9 in the 99th percentile.

A natural way to proceed given the instability from simulated annealing is to average across model runs to produce the instrument. There are several benefits to averaging the estimated instruments: first, it improves the "signal" to "noise" ratio: the "signal" is guided by the hydrographic and budget factors from the data and is persistent between runs while the "noise" varies randomly. When the runs are averaged, the "signal" is retained across runs while the "noise" is canceled out. This effect can be seen as the first stage coefficient grows in the number of replications averaged for the creation of the instrument in table 1. Another difference is that the instrument used in the 2013 paper created binary predictions for whether electricity infrastructure would be placed in each point in space, while the new averaged instrument generates a probability for each location in each decade. This improves precision and makes our first stage stronger: in our preferred specification the averaged instrument has a coefficient of 0.39 with an F-Statistic of 28.5. The instrument is stable once you have averaged at least 50 replications of the code as shown in table 1. Additional runs of the instrument have little impact on the estimates past 50. In this comment (and in a follow-on paper Szerman et al.), we use the averaged instrument across 500 runs of the Matlab code. We also use this averaged version of the instrument in all specifications that follow.

The Specification Lipscomb et al. [2013a] include county fixed-effects in their estimation in order to avoid relying on cross-sectional variation for identification which would otherwise introduce bias from differences between the counties. This isolates the identifying variation in the instrument to the cutoff between the last locations that received plants until the budget was exhausted for that decade, and the next most suitable areas that barely missed the threshold and had to wait for another decade for a plant. To mitigate concerns of common, spurious trends between the predicted grid expansion and development indicators, Lipscomb et al. [2013a] directly control for any differential trends by adding geographic-specific time fixed-effects in their 2SLS estimation. Many potential specifications are possible with different geographic characteristics. In their preferred specification, Lipscomb et al. [2013a] include interactions between the Amazon dummy and time-fixed effects in the first and second-stage regressions, which accounts for differential dynamics in Amazon counties.

Changes to the Specification regarding the Amazon Lipscomb et al. [2013a] uses Brazil's Northern Amazon region defined by Brazil's official statistical agency, IBGE as a control in the estimation of the instrument. This area is primarily the area in which electricity transmission was managed by Eletronorte, and followed a separate planning process with more "isolated" generation plants than the rest of the Eletrobras network. Therefore, Lipscomb et al. [2013a] controlled separately for the Legal Amazon in the creation of the instrument–both in the Probit regression and in the cost factors for transmission. In the 2SLS specification we controlled for an indicator for the densely forested Amazon and the Pantanal regions interacted with decade fixed effects, to guard against the possibility that the built-in assumption about the high cost of building in certain areas was not driving the results on the effects of electrification. While either definition of the Amazon – the IBGE definition or the more expansive definition including the Pantanal– is reasonable, Ankel-Peters et al. is correct to point out that the definition should be consistent in both the Matlab code to construct the instrument and the Stata code where we run second stage regressions.

We have revised the specification to control for a single version of the Amazon throughout, the Amazon "biome" as defined by IBAMA, the Brazilian Institute for the Environment and Renewable Natural Resources [IBAMA Brasil]. This is a purely geophysical definition of the Amazon. The biome has been used for policies to protect the Amazon such as the Central Bank's resolution 3545 requiring landholders to comply with titling requirements and environmental regulations in order to receive subsidized credit Assunção et al. [2020]. As the biome is defined on biological and geo-climatic rather than jurisdictional boundaries, it does not have a direct mapping to county borders. We consider any county which touches the Amazon biome as belonging to the Amazon.

Table 2 reports the first-stage results from Lipscomb et al. [2013a] together with the first stage results using the consistent Amazon biome definition together with the averaged instrument. Each row of Table 2 corresponds to a different specification controlling for geographic-specific time trends, in the spirit of Table 10 in Lipscomb et al. [2013a]. We add second stage results for HDI and log GDP per capita to the table so that the reader can observe the extent to which the 2SLS second stage results are stable.

The instrument becomes weak and therefore invalid, in Lipscomb et al. [2013a]'s preferred specification (row

Table 2: First Stage Results: Various Specifications Using the Amazon Biome Definition

	AEJ Printed		Corrected			
			Corrected Biome		HDI	ln(GDP pc)
	(1)	(2)	(3)	(4)	(5)	(6)
Specification	First-Stage	F-Stat	First-Stage	F-Stat	Second Stage	Second Stage
1. Water Flow × budget	0.32***	45.8	0.47***	51.90	0.05***	1.16***
	(0.05)		[0.07]		[0.02]	[0.33]
2. River Gradient × budget	0.32***	47.8	0.46***	49.68	0.05**	1.16***
	(0.05)		[0.07]		[0.02]	[0.33]
3. Amazon FE × budget	0.22***	25.4	0.13**	5.33	0.10	2.58*
	(0.04)		[0.06]		[0.06]	[1.44]
4. Water Flow × budget, Amazon FE × budget	0.22***	24.8	0.13**	5.32	0.10	2.58*
	(0.04)		[0.06]		[0.06]	[1.44]
5. River Gradient × budget, Amazon FE × budget	0.22***	25.3	0.13**	5.22	0.10	2.72*
	(0.04)		[0.06]		[0.06]	[1.51]
6. River Gradient × budget, Water Flow × budget	0.32***	48.6	0.46***	49.78	0.05**	1.16***
	(0.05)		[0.07]		[0.02]	[0.33]
7. River Gradient × budget, Water Flow × budget, Amazon FE × budget	0.22***	24.4	0.13**	5.09	0.10	2.70*
	(0.05)		[0.06]		[0.07]	[1.53]
8. Water Flow × year FE	0.32***	45.6	0.47***	51.85	0.05**	1.15***
	(0.05)		[0.07]		[0.02]	[0.33]
9.Main Preferred Specification in AEJ, 2013: Amazon dummy × year FE	**0.22*****	**24.6**	**0.13****	**5.17**	**0.11***	**2.74***
	(0.04)		[0.06]		[0.06]	[1.51]
10. River Gradient × year FE	0.22***	47.8	0.46***	49.23	0.05**	1.16***
	(0.05)		[0.07]		[0.02]	[0.34]
11. Water Flow × year FE, Amazon FE × year FE	0.22***	23.9	0.13**	5.19	0.11*	2.72*
	(0.05)		[0.06]		[0.06]	[1.49]
12. River Gradient × year FE, Amazon FE × year FE	0.22***	24.9	0.13**	4.92	0.11*	2.95*
	(0.04)		[0.06]		[0.07]	[1.63]
13. Water Flow × year FE, river Gradient × year FE	0.32***	48.3	0.46***	49.35	0.05**	1.16***
	(0.05)		[0.07]		[0.02]	[0.33]
14. River Gradient × year FE, water Flow × year FE, Amazon FE × year FE	0.22***	23.9	0.13**	5.19	0.11*	2.72*
	(0.05)		[0.06]		[0.06]	[1.49]
15. New preferred specification: Quartic suitability rank × year FE	0.24***	29.1	0.39***	28.47	0.06***	0.75*
	(0.04)		[0.07]		[0.02]	[0.39]

Notes: All specifications include AMC60 fixed effects and decade fixed effects in addition to the listed controls. All regressions include county size weights. Standard errors are clustered at the AMC60 level.

9). In fact, the table shows that the instrument becomes weak whenever the specification directly controls for Amazon-specific time fixed-effects (see rows 3,4,5,7,9, 11,12,14), and retains first-stage power when it does not (rows 1,2,6,8,10,13,15). The Amazon-specific trend controls simply absorb too much variation from the instrument. In the specifications with weaker first stages, we find that the second stage coefficients get large–about double–relative to the coefficients with a strong first stage.

Based on these considerations, we re-estimate the 2SLS specification using the averaged instrument and with the following changes. First, we use the Amazon biome definition of the Amazon in all regressions and apply it consistently across all stage of analysis. Second, we replace Lipscomb et al. [2013a]'s preferred specification (shown in row 9 in Table 2) since the first-stage is now weak (coefficient is 0.13 with F-stat of 5.2), with a specification that controls directly for the levels and differential trends in the index of cost factors used in construction of the instrument, reported in row 15. This alternative specification controls directly for time variation related to the estimated geographic suitability of a location for electricity generation. The first stage F-statistic in our new preferred specification is 28.5, higher than the 24.6 reported in Lipscomb et al. [2013a], and still above the commonly used cutoff of 10 suggested by Staiger and Stock [1997]. Because the F-statistic is below the 104 suggested by Lee et al. [2022] we also provide standard errors that have been corrected at the 95% level using their tF procedure (but as shown in Lee et al. [2023], this is most likely overly conservative).

Outcomes Ankel-Peters et al points out that land values from IPEA are estimated based on a hedonic equation which includes electricity access, leading to potential bias in our estimates of the impact of electricity on land values. Reiff and Reis [2016]'s description of how this variable was constructed confirms this. The housing values variable was meant to provide a summary measure of the value of access to electricity through the willingness of households to pay for electricity, which should incorporate their perception of the impact of electricity on their productivity. Given the variable construction issue, we replace housing values with log GDP per capita as an alternative summary measure of development. It is important to acknowledge that this estimate gives larger weight to formal production and leaves out any consumption value that the household assigns to electricity. We winsorize GDP per capita at the 1% and 99% levels given the errors and outliers produced when estimating GDP at the county level.

Data Cleaning Notes The UNDP's calculation of HDI changed between 1990 and 2000, and therefore the HDI variables needed to be adjusted to maintain consistency across the sample. 1990 values were provided for both the old and new methods of calculation of the HDI variables, so we adjusted the HDI variables by creating a ratio of the HDI for 1990 calculated under the old method to the HDI for 1990 calculated under the new method: $\frac{HDIold}{HDInew}$ and multiplying the new 2000 variable by the adjustment ratio. In cases in which the old variable was missing (as is the case for newly created counties), we use the median value of the adjustment ratio for the AMC.[1] Ankel-Peters et al. noted that while we did adjust the HDI component variables, we neglected to adjust the aggregate HDI variable in Lipscomb et al. [2013a]. That is rectified in all regressions in this note.

Results Compared to Lipscomb et al, 2013 In table 3 we compare the results from the original version published in Lipscomb et al. [2013a] to a version in which the coding errors have been resolved but the original specification maintained and the new preferred specification using the quartic of the suitability rank interacted with decade fixed effects as controls. For our main results on HDI and housing values, the coefficient magnitudes are smaller, but electricity continues to have a positive, statistically significant effect on these outcomes. For the various "mechanisms variables", statistical precision is lost in several cases, but the direction of impacts remains the same in nearly all cases. Using the Lee et al. [2022] tF standard errors correction procedure, further statistical precision is lost.

Columns (5) and (6) of table 2 provide the estimates of the impact of electrification for the summary measures of HDI and log GDP per capita. A change from no access to universal access to electricity leads to an increase in HDI of 0.05- 0.06 percentage points and an increase in log GDP per capita of 0.84-1.35 (an impact of approximately 131 percent on GDP per capita)[2].

Column (6) of Table 3 provides the estimates for our key summary measures of development as well as the mechanism variables using our new preferred specification while column (4) provides the IV estimates using the old specification but with the coding changes and averaged instrument. As shown in table 2, the effect of electrification on the summary measures of development remains positive and significant, though smaller in magnitude than originally reported.

The impact of electricity access on HDI components and other poverty measures was shown in table 11 of Lipscomb et al. [2013a], they are now shown in rows 3-15 of table 3. The estimated impact of electrification on HDI components and (reduction in) poverty is positive but smaller in magnitude and not statistically significant in the new specification. Infant mortality does have a large and statistically significant measured impact.

The employment measures of economically active and formal employment remain positive, but the point estimates are smaller and the estimates are only statistically significant for formal employment and rural

[1]This is similar to a suggestion by Ankel-Peters et al, they suggest a population based weighting scheme, we use the median value for the AMC.
[2]$(exp^{(.84)} - 1)$

Table 3: Comparison between 2013 results and Revised Results

	AEJ Printed Results		AEJ Specification, Coding Corrections		New Specification Results	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV
Results from Table 6 in LMB						
Housing Values	0.80***	8.81***	0.31	5.46	0.67**	3.78**
	[0.27]	[3.03]	[0.23]	[4.29]	[0.30]	[1.73]
						(2.11)
Results from Table 7 in LMB						
Human Development Index	0.01	0.11***	−0.00	0.11*	0.00	0.06***
	[0.01]	[0.04]	[0.00]	[0.06]	[0.01]	[0.02]
						(0.02)**
Results from Table 11 in LMB						
HDI: Education	0.03***	0.19***	0.01***	0.12	0.02**	0.04
	[0.01]	[0.06]	[0.00]	[0.07]	[0.01]	[0.04]
						(0.05)
HDI: Longevity	0	−0.01	−0.01*	0.04	−0.01	0.00
	[0.01]	[0.05]	[0.00]	[0.06]	[0.01]	[0.03]
						(0.04)
HDI: Income	−0.03*	0.45**	−0.07***	0.36	−0.08***	0.02
	[0.02]	[0.15]	[0.01]	[0.25]	[0.02]	[0.13]
						(0.16)
Infant Mortality	−7.99***	−11.97	−10.62***	-222.56***	−10.27***	−71.30***
	[2.42]	[18.08]	[2.10]	[83.64]	[2.32]	[15.03]
						(18.34)**
Gross Income PC	−0.01	0.11**	−0.02***	−0.06	−0.02***	−0.04
	[0.01]	[0.05]	[0.00]	[0.07]	[0.01]	[0.04]
						(0.05)
Poverty	−0.76	−42.17***	3.17***	−32.16*	2.92*	−11.05
	[1.39]	[13.84]	[1.17]	[19.42]	[1.77]	[10.72]
						(13.08)
Results from Table 12 in LMB						
Economically Active	0.011*	0.173***	−0.00	0.03	0.00	0.05
	[0.01]	[0.05]	[0.01]	[0.07]	[0.01]	[0.03]
						(0.04)
Formal Employment	0.010*	0.184***	−0.00	0.09	0.00	0.07**
	[0.01]	[0.05]	[0.00]	[0.07]	[0.01]	[0.03]
						(0.04)
Urban Employment	0.01*	0.18***	−0.00	0.10	0.00	0.04
	[0.00]	[0.05]	[0.00]	[0.06]	[0.01]	[0.04]
						(0.05)
Rural Employment	0.01	0.17***	−0.00	-0.01	0.01	0.08**
	[0.01]	[0.05]	[0.01]	[0.07]	[0.01]	[0.03]
						(0.4)**
Results from Table 13 in LMB						
Less than four years education	−0.36	−21.25***	1.44*	-11.66	2.37**	2.37
	[0.90]	[7.75]	[0.75]	[10.45]	[1.11]	[5.88]
						(7.17)
Years in School	0.06	2.02***	−0.11*	0.76	−0.16*	−0.11
	[0.08]	[0.67]	[0.06]	[0.91]	[0.10]	[0.44]
						(0.54)
Human Capital	2.09	11.54	−1.31***	-1.75	−1.05	0.21
	[0.41]	[7.30]	[0.43]	[7.69]	[0.68]	[3.07]
						(3.75)
Results from Table 14 in LMB						
Life Expectancy	−0.44	−1.03	−0.75***	-2.21	−1.01***	−2.69
	[0.32]	[2.39]	[0.28]	[5.76]	[0.31]	[2.07]
						(2.52)
Population Density	−1.11	−23.62	−1.78	-83.62	5.95***	8.42
	[3.74]	[19.20]	[2.95]	[55.60]	[2.19]	[9.47]
						(11.64)
Urban percent of pop	0.01	0.24**	−0.01	0.19	−0.02	−0.01
	[0.01]	[0.11]	[0.01]	[0.17]	[0.02]	[0.11]
						(0.13)

Notes: Standard errors clustered by county reported in brackets, 0.05 tF adjusted standard errors corrected using the Lee et al. [2022] tF procedure reported in parentheses (adjusted by 1.22 based on their table 3A). All regressions have county size weights, county fixed effects decade fixed effects and hydrohat quadratic*decade fixed effects. The changes to the specification are (1) both the instrument and the 2SLS use Amazon Biome as the definition of the Amazon in the control, and (2) the instrument is based on averaging the results of 500 runs of the Matlab simulated annealing. The first stage F-Statistic is 28.5. Note that there are bias concerns related to the housing values variable based on the estimated nature of the original data (see discussion in outcomes section)

Table 4: Heterogeneity by above or below median 1970 GDP pc

Panel A: Income Impact in Below Median GDP pc Counties								
	ln(GDPpc)	HDI	HDI Education	HDI Longevity	HDI Income	ln(IndGDPpc)	Poverty	Pct econ active
Electricity	0.842	0.097***	0.029	−0.007	−0.194	−0.223	4.353	0.014
	[0.593]	[0.032]	[0.072]	[0.040]	[0.207]	[0.765]	[17.911]	[0.054]
Panel B: Income Impact in Above Median GDP pc Counties								
	ln(GDPpc)	HDI	HDI Education	HDI Longevity	HDI Income	ln(IndGDPpc)	Poverty	Pct econ active
Electricity	0.619**	−0.001	0.031	0.008	0.256***	1.063**	−26.495***	0.089***
	[0.272]	[0.031]	[0.019]	[0.032]	[0.094]	[0.516]	[7.173]	[0.023]

Notes: All specifications include AMC60 fixed effects and decade fixed effects in addition to the listed controls. All regressions include county size weights. Standard errors are clustered at the AMC60 level. Panel A is on the below median GDP pc sample (4364 observations) and Panel B is on the counties that were above median GDP pc as of 1970 (4364 observations). The first stage F-Statistic in Panel A is 10.22, the first stage F-Statistic in Panel B is 42.45.

employment. Effects on education and human capital also lose statistical significance.

Rows 15-18 of Table 3 show the effects on a few outcomes that Lipscomb et al. [2013a] was trying to rule out as mechanisms. Effects on life expectancy and population density were statistically insignificant in both the original paper and here. Effects on urbanization also becomes statistically insignificant. Lipscomb et al. [2013a] ran these regressions to examine whether the documented effects of electrification on development outcomes were the result of simple population movements, and we can now say with a bit more confidence that they were not.

The change in statistical significance of several of the "mechanisms variables" implies that we should rethink the arguments put forward in the mechanisms section of Lipscomb et al. [2013a]. There have been some interesting developments in the literature analyzing these connections since that original paper was published. For example, Lee et al. [2020] hypothesize that some areas may not experience substantial benefits from electrification if they are too poor or credit-constrained to purchase the complementary inputs required to take full advantage of energy access. It also may take a long time for the benefits to be realized, as firms and households need time to make those complementary investments.

While a full re-analysis of mechanisms is beyond the scope of this short note, our dataset contains an unusually long panel which from 1970 to 2000s with which the Lee et al. [2020] hypothesis could be explored in future research. In table 4 we re-estimate the effects of electrification splitting the sample between below and above-median GDP per capita counties as of 1970. the effects of electrification indeed look very different in the two sub-samples. Consistent with the Lee, Miguel, and Wolfram [2020] hypothesis, effects on industrial GDP per capita, poverty and percent economically active is large and statistically significant only in wealthier counties. This may be a useful direction for future research to explore, to help improve targeting of electrification projects to areas most equipped to benefit from them.

Key Takeaways Estimating the causal impact of infrastructure services is notoriously difficult because access naturally expands to areas where demand is highest–the central contribution of Lipscomb et al. [2013a] was to show that one can harness the differences in costs together with the budget for electricity over time to create an instrument for electricity access with panel variation. This provides two key improvements in estimation of the impact of electrification: (1) the panel variation in the instrument allows us to include fixed effects to net out time-invariant factors unrelated to electricity that make certain areas more likely to both receive electricity and to have higher income and electricity demand. (2) the remaining identifying variation is from the difference between counties which receive electricity in a given decade and those who are marginally higher cost than the decade cutoff based on geographic factors. It is unlikely that the outcome variables are correlated with this difference in counties around the cutoff for electricity provision in a decade for reasons *other* than increased access to electricity, so the exclusion restriction is satisfied. In this note we update our estimates based on valid concerns raised by Ankel-Peters et al..

The updated results show that the impact of electrification on summary measures of development (HDI and log GDP per capita) remains positive and statistically significant, but the impact on HDI is more muted than found with the original instrument. Mechanisms underlying the links between electrification and development become less clear. Our data is suitable for testing some hypotheses about mechanisms suggested by Lee et al. [2020] and Fetter and Usmani [2024], and these questions should be further investigated in future research.

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