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Measuring firm activity from outer space

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Abstract

To understand how global firm networks operate, we need consistent information on their activities, unbiased by their reporting choices. In this paper, we collect a novel dataset on the light factories emit at night for a large sample of car manufacturing plants. We show that nightlight data can measure activity at such a granular level, using annual firm financial data and high-frequency data related to Covid-19 pandemic production shocks.

Keywords: **multinational firms; nightlight data; global firm networks.**

JEL classifications: **H32, H26, F23.**

1. Introduction

In recent decades, the use of remote sensing data to monitor human activity has dramatically increased. The advantage of this data is that it is consistently available on a global scale and, as a result, allows for studying activity in areas where no reliable statistical data exists. In this paper, we ask whether we can use this data to study the economic activity of *firms*. This would allow us to analyze firm activities outside of developed countries, where the data is harder to obtain, and would help create datasets free of biases coming from different data reporting standards and firm reporting choices.

We hand-collect a novel dataset of car manufacturing plants that belong to the twenty largest car manufacturers in the world that operate in sixty countries. These manufacturers captured 82 per cent of the world car manufacturing market in 2017.[1] We match the information on their footprints with nightlight and subsidiary financial data to study the global distribution of their activities from space. The focus on manufacturing firms allows us to pin down the fixed geographical location of each production site and

1. See: Oica dataset.

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measure nightlight emissions and output related to that particular site. This is not possible for service firms with more mobile capital, such as Alphabet or Amazon. We choose car manufacturers in particular as their production is quite homogeneous across countries, they typically cover a large enough area to be identifiable by the current generation of nightlight data, and they also provide very detailed information on all of their production plants and their history.

To understand whether we can use nightlight data to proxy for such granular firm-level activities, our empirical strategy relies on estimating the relationship between observed nightlight data and reported subsidiary activities. In what follows we refer to a car manufacturing subsidiary as either a subsidiary or a firm and to the manufacturer itself as a parent. We measure firm activities using firm revenues. Following Henderson et al. (2012), we model reported firm activity and measured nightlights as noisy proxies for their true values and estimate the relationship between these observed variables. Under standard orthogonality conditions, and because nightlights are measured with noise, we estimate an upper bound of the elasticity linking true light emissions and true economic activity. We implement this approach by matching remote sensing data with annual subsidiary financial data for the years 2012–2019. Each subsidiary operates in one country but can own multiple plants. For subsidiaries with multiple plants, we aggregate nightlight data across all plants owned by such subsidiaries. We show strong and stable positive correlations between firm activity and nightlight data, in both a cross-section and across time. Nightlight data explains a large, 71 per cent, variation in firm activities across subsidiaries and a much smaller, 10 per cent, variation across time.[2] The magnitude of the estimated coefficients suggests that a 1 per cent change in nightlight emitted by a subsidiary is correlated with a 0.66 per cent change in firm revenue.

We use our estimates to predict firm activity and systematically analyze the role that different fixed effects and observable subsidiary and parent characteristics play in explaining for which firms we make good predictions and for which we do not. We then show that most of the difference between revenue *predicted* using nightlight data and revenue *reported* by firms is driven by country and parent fixed effects. This finding suggests that we can use nightlight data as a proxy for firm activity in cases where we do not observe such activities, but observe other firms that belong to the same parent and other firms that operate in the same country. For example, let us consider a setting in which we observe turnover for Ford-owned subsidiaries operating outside of Mexico and for Toyota-owned subsidiaries operating within Mexico. Our empirical approach enables us to predict turnover for a Ford-owned subsidiary located in Mexico.

We then investigate what type of economic activity is measured by nightlight. To do so, we split the total light emitted into the factory area and the intensity of light emitted by that area to show that both light and area are similarly good predictors of changes in firm activities over time.[3] We take this finding further, by showing that light emissions are *causally* linked with production activities. To do so, we analyze the effects of the Covid-19 pandemic closures of all car manufacturing businesses announced in spring 2020 around the world. For each car manufacturing plant, we collect daily data on nightlight emitted by those factories 12 weeks before and after the Covid-19 pandemic closure dates. These factories constitute our treated group. To control for differences in light emitted at each latitude and longitude at each time of the year, we use as a control group these same factories on those same dates, but in 2019. We find that a complete factory closure results in up to a 14 per cent reduction in nightlight emitted by these factories in the week after the closure. These results suggest that light emission is *causally* linked with production activities, such as the use of labor, but a far larger part of factory light emission comes from fixed infrastructure.

Broadly, we contribute to a large literature that links nightlight data with economic activity, by providing novel evidence on how these links work using small spatial units. While the nightlight is linked to

2. This is similar to the literature that uses small spatial units, such as villages, communes, and countries (Goldblatt et al., 2020; Asher et al., 2021; Gibson and Boe-Gibson 2021).
3. Conceptually, there are two possible sources of changes in nightlight: changes in the area occupied by a factory or the intensity of nightlight production. Our data allow studying both effects separately, similar to Gibson et al. (2017) who did that at the regional level. As such, this paper also offers a technical contribution to the nightlight data literature in explaining where and how the nightlight is generated and whether this matters for how we interpret the nightlight measures (Henderson et al., 2012: 999).

economic activity at the national (Henderson et al., 2012) and sub-national level (Lessmann and Seidel 2017; Bluhm and Krause 2022), there is little evidence on how this data performs at the firm level. Studies that utilize the newest generation of nightlight data still focus on regional applications (Gibson 2021; Gibson et al., 2021). A sub-set of this literature uses nightlight data to specifically study economic shocks due to, for example, war (Li et al., 2017), natural catastrophes (Mohan and Strobl 2017; Fabian et al., 2019), power grid failures (Elvidge et al., 2020b) or the Covid-19 pandemic (e.g., Elvidge et al., 2020a; Bustamante-Calabria et al., 2021; Straka et al., 2021). However, these studies still focus on fairly large spatial units of economic activity like sub-national regions, cities, or larger neighborhoods.[4]

2. Data cleaning and samples used

2.1 Factory land consumption

To identify the factory footprints, we collect the daylight satellite images provided by Google Maps for the years 2012–2019, which we supplement with information from OpenStreetMap and aerial shots of factories from firm websites. The data were collected in two waves. First, factory footprints were tagged based on the 2019 images by multiple research assistants, and the data was cross-validated using a subset of data collected by all the researchers. In the second wave, the annual changes in footprints since 2012 were extracted by one research assistant. As such, the land consumption data cover the period 2012–2019. During this second wave, a second quality check occurred, where 10 per cent of firms with low-quality tagging were excluded. There were two main reasons for this sample selection: first, the resolution of daylight images was not high enough to credibly identify the location of factories; second, we did not find another data source to cross-validate the information collected. For the latter reason, factories in China are more likely to be classified as unreliable, as, for example, Google Street View and Maps or OpenStreetMap data were not available or company websites were inaccessible.

Factory footprints were tagged by research assistants based on a set of predefined rules: only buildings were tagged, parking lots, and racetracks were excluded, and the fence around the production site was used to mark the factory line. If two car manufacturers own the same factory, i.e. they have a joint venture, we copied the same polygon for both firms. In the main analysis, we drop joint ventures to make the exposition as clear as possible but test the robustness of this approach in the Appendix.

2.2 Nightlight data

For each factory area, we also collect nightlight data from the Visible and Infrared Imaging Suite (VIIRS) Day-Night Band (DNB) that sits on board of satellites of the Joint Polar-orbiting Satellite System provided by the Earth Observation Group (EOG). We show an example of a tagged factory and the nightlight emitted by that factory in Figure B1 (see online supplementary material for a color version of this figure). We use two frequencies of nightlight data: daily and annual. We collect *daily data* from the *raw* Nightly DNB Mosaic and Cloud imagery and use the Black Marble (BM) processed *annual data* for years 2012–2019. For both sources, the resolution of the data is 15 arc seconds, which corresponds to an area of approximately 500 m $\times$ 500 m at the Equator. This data measures light emitted by these areas in watt per steradian per square centimeter, nW/cm^2/sr.[5]

4. The older generations of nightlight data have been applied in various economic contexts, such as, for instance, origins of urbanization (Henderson et al., 2017) or its consequences (Bluhm and Krause 2022), the origins of ethnic inequality (Alesina et al., 2016), the growth effects of human capital formation (Gennaioli et al., 2013), rescue conflicts (Berman et al., 2017) favoritism (Hodler and Raschky 2014), trade (Hirte et al., 2020), institutions (Michalopoulos and Papaioannou 2013, 2014) or growth of dictatorships (Martinez 2022). For a detailed review on the use of nightlight data, see Donaldson and Storeygard (2016), or more recently Levin et al. (2020).
5. For more details see: https://eogdata.mines.edu/products/vnl/ and https://blackmarble.gsfc.nasa.gov/.

The BM data that we use for our annual estimates has been filtered to remove sunlit, moonlit, and cloudy pixels. It consistently corrects for the presence of stray lights, snow cover (Han et al., 2022), and allows us to restrict our attention to only the overhead observations to reduce the scope of blurring and shadowing. Further, outliers to discard biomass-burning pixels and to isolate the background have been removed.

We collect the daily data for two time periods: (1) daily images for 12 weeks before and after a factory closes and (2) these same daily images for those same dates but in 2019. For daily data, we have to rely on the raw data. Hence, given the small spatial units that we analyze, we take great care to clean the data to account for potential problems coming from neighboring blooming effects and the volatility of this data with respect to daily changes in cloud coverage. As such, using the VIIRS Cloud Mask we identify cloud-free observations, we calculate light emitted by factories on cloud-free days only and we also exclude days when the sky was cloudy over the 5 km radius around factories.[6] We always control for the cloud coverage in a 10 km radius around each factory. For the event study, we aggregate this data at the weekly level. Further, studies have shown that even the cleaned monthly VIIRS data is relatively volatile. This can occur due to, for example, cyclically local phenomena such as weather or moonlight (Coesfeld et al., 2018). To account for this, we control for the light emitted in a 5 km radius around a factory, excluding the factory itself.

2.3 Financial data

Subsidiary-level data was collected using Orbis unconsolidated information. We collect the following balance sheet information: total assets, fixed assets, employment, profit and loss before tax, and revenue for the years 2012–2019. We winsorize the subsidiary-level financial information at a 5 per cent level to account for outliers.

2.4 Covid-19 closure dates

We hand collect the dates on which car manufacturing factories closed due to Covid-19. Our main source of information was the marklines.com portal followed by news updates provided by just-auto.com. We summarize the Covid-19 factory closure dates in Figure B2 (see online supplementary material for a color version of this figure). Note that the majority of car manufacturing factories in Europe and the US closed around March 17th–20th. Later closure dates come mainly from Asian countries that closed due to part shortages from European and US factories, while January closure dates refer to factories in China. We also collected the official opening dates, when factories announced that they restarted their car production. However, some factories, especially in the US and Europe, reopened earlier, producing face masks or ventilators. We do not have information on when these partial activities restarted, but we can empirically observe when the light emitted by those factories starts increasing again.

2.5 Samples

In this paper, we use two different samples when we analyze the factory and firm-level data. We summarize these in Table 1 and describe them in detail below.

To analyze the impact of Covid-19 shocks on nightlight emitted by factories, we use the raw *factory-level* data. The unit of observation is a factory observed daily. After cleaning, we have data on 684 factories available for analysis. In this sample, we rely on the factory footprints, daily observations of nightlight emitted, and Covid-19 closure dates. This represents a universe of the factories of each of the car manufacturers included in the sample. We plot the location of each factory in the dataset in Fig. 1 and we

6. These criteria mean that we lose approximately 10 per cent of observations.

Table 1. Summary of samples used in the paper.

Subsidiary-level data	
Unit of Observation:	555 subsidiaries (the subsidiaries of the twenty largest car manufacturers in the world owning one or several of the 684 factories)
Used for:	Tables 3 and 4, Tables B1-2, C1-4, D1 (see online supplementary material for a color version of these tables), Figs 2–4, Figures B3–4, D1 and D4 (see online supplementary material for a color version of these figures)
Data structure:	Panel [annual, Table 3 (columns 3–5) and 4 panel B] Cross section [average 2012–2019, Table 3 (columns 1–2) and 4 (panel A)]
Sample period:	2012–2019
Sample restrictions:	No joint ventures and no missing accounting data
Aggregation:	When a firm owns multiple factories, add nightlight and area for those factories
Factory-level data	
Unit of Observation:	684 factories
Used for:	Fig. 1, Fig. 5, Figures B1 and D2-3 (see online supplementary material for a color version of these figures), Table 2, Table 5
Data structure:	Panel: daily light observations
Sample period:	Start date: January 1st, End date: end of May; both for 2019 and 2020
Sample restrictions:	Observations with no cloud coverage in a 5 km radius and availability of controls.
Aggregation:	Daily data in Table 5 (columns 1–2), aggregated at the weekly level in Fig. 5

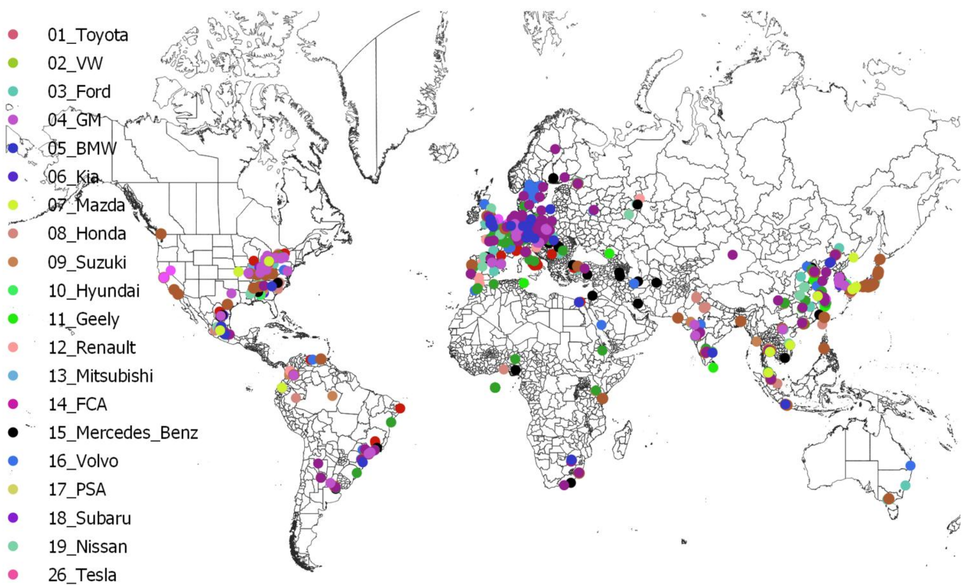

Figure 1. Map of factories around the globe.

use this location to assign country-level characteristics to each factory. The nightlight produced by the factories in this sample and the area they occupy are summarized in Table 2.

Second, we match the factory data with financial information at the *subsidiary-level*. We use the hand-collected names of factory owners from factory websites and Google Places listings to manually identify

Table 2. Descriptive statistics: factory-level data.

Variable	Mean	Median	SD	Min	Max
Total nightlight, N_i, ($nW/cm^2/sr$)	526.319	39.025	11,882.826	0.058	762,000
Factory area (km^2)	0.308	0.170	0.446	0.002	5.948
Average nightlight	334.535	37.408	7,179.882	0.300	482,000
Average nightlight within 5 km radius	309.203	11.565	7,338.668	0.047	510,000
Cloud coverage	0.039	0.004	0.092	0.000	1.398
Number of factories	684				

Note: This table provides summary statistics related to the daily factory-level nightlight data. Total nightlight is the total light emitted by a factory. The area is a factory area in square kilometers, measured by the footprint of the factory buildings. Average nightlight is the total nightlight divided by the factory area. Average nightlight within 5 km radius measures the average light within a 5 km radius around the factory, excluding the factory itself. Cloud cover is constructed from the VIIRS cloud-mask information by averaging the integer cloud-mask category (coded 0–3) within a 10 km radius around each factory. Note that here we only use observations that have no cloud cover within 5 km radius around the factory. The nightlight-related variables come from the VIIRS dataset.

firms in the Orbis dataset. The unit of observation in this analysis is a reporting firm, i.e. a subsidiary of the car manufacturing multinational, such as, for example, Ford Mexico. In our dataset, some subsidiaries own several factories; hence, we aggregate factory areas and the light they emit at the subsidiary level to be able to compare firm financial information with our remote sensing data.[7] This means we have 555 subsidiaries for which we collect financial information.

We obtain the panel data over the years 2012–2019 to track nightlight and firm financials over time. We also average this data across all years into a cross-section. We provide basic summary statistics for light and financial data in the panel and cross-sectional samples in Table B1 (see online supplementary material for a color version of this table).

Private firms in Orbis are not required to report financial data and many choose not to do so for various reasons. Out of the 555 subsidiaries in our dataset, 244 have information for at least one financial variable, mostly revenue. Hence, we use only those 244 subsidiaries to consider correlations between firm activities and nightlight data.

3. Empirical approach

In this section, we outline the econometric model that guides our empirical analysis. As our empirical analysis is conducted in logarithms, we formulate the structural relationship directly in logarithms, consistent with standard approaches that use nighttime lights as a proxy for economic activity. Henderson et al. (2012) provides a canonical discussion.

First, we let firm activity be measured with classical error. We focus on firm revenue as a measure of firm activity. Since true revenue, R^*, is not directly observable and we only have information on what firms *report* to be their revenues, R, we model systematic misreporting as a proportional wedge between reported and true revenues, $R = \lambda R^*$ in levels, and operationalize this relationship in natural logarithms. We define $r \equiv \ln(R)$ and $r^* \equiv \ln(R^*)$ and write:

$$r = \ln(\lambda) + r^* + u_r, \tag{1}$$

where λ captures potential discrepancies between a firm's true economic activity and what it reports, and u_r captures classical noise in logarithm of revenues with variance $\sigma^2_{u_r}$.

7. We discuss the validity of this, and other assumptions, in Section 4.

For symmetry, we assume that nightlight is also measured with classical error. We allow for a proportional wedge between recorded and true nightlight, $N = \varphi N^*$ in levels, and operationalize this relationship in natural logarithms. We define $n \equiv \ln(N)$ and $n^* \equiv \ln(N^*)$ and write:

$$n = \ln(\varphi) + n^* + u_n, \tag{2}$$

where φ measures the gap between actual luminosity and what the satellite data records and u_n is a classical noise in logarithm of nightlights with variance $\sigma^2_{u_n}$.

The structural equation of interest that describes the relationship between the logarithm of true firm revenue and the logarithm of true firm nighttime lights is:

$$n^* = \alpha_0 + \gamma r^* + u \tag{3}$$

where γ is the structural parameter of interest and u is a mean-zero idiosyncratic shock with $var(u) = \sigma^2_u$.

We combine Equations (1)–(3) and add control variables to obtain our baseline estimation equation. In this logarithm formulation, the coefficient of interest corresponds to the inverse structural elasticity of interest, $\beta \equiv 1/\gamma$.

$$\ln(R_i) = \alpha + \beta \times \ln(N_i) + X_i' \delta + \varepsilon_i \tag{4}$$

where i is a subsidiary, $\ln(R_i)$ is the logarithm of revenues; $\ln(N_i)$ is the logarithm of the total light emitted by factories that belong to subsidiary i; X_i' includes a set of control variables which consist of different sets of fixed effects, and ε_i is an idiosyncratic error term. We start our analysis using cross-sectional data and the notation in this exposition reflects that. Consequently, we also first focus on estimations that include parent and country fixed effects. We cluster standard errors at the country level across all specifications to account for within-country correlations between errors assuming error independence across countries.[8]

Under standard orthogonality conditions, the estimand in the baseline regression is $\widehat{\beta} = 1/\gamma$. However, in the presence of classical noise in the measured nightlight and in the relationship between true nightlight and revenues, OLS estimates suffer from attenuation bias. This means that $\widehat{\beta}$ should be interpreted as an upper bound estimate of γ.[9]

In an empirical specification without any control variables, the error term ε_i is a composite of errors u, u_n, and u_r and level shifters related to measurement of nightlight, revenues and the relationship between nightlight and revenues, φ, λ, and γ respectively. The slope of $\widehat{\beta}$ will be affected by any components of the error term that are correlated with $\ln(N_i)$.[10] Including various sets of fixed effects across specifications will remove the concerns about level shifters in the error term, however, if any components of the error term vary within fixed-effects groups the concern about bias will remain. In what follows we discuss each source of potential error that could bias our estimates of $\widehat{\beta}$ in turn.

3.1 Measurement errors in revenues

Many commonly discussed sources of revenue errors, such as, differences in accounting methods (e.g. cash versus accrual), financial reporting standards (e.g. GAAP versus IFRS), exchange-rate fluctuations, and segment-level reporting practices are likely to be persistent within countries and parent firms over

8. An alternative would be to cluster at the parent level to account for within-parent correlations between errors. In Table D1 (see online supplementary material for a color version of this table), we show that this alternative clustering does not affect the main inference.
9. In Supplementary Appendix A, we derive the probability limit of $\widehat{\beta}$ in the log specification and show precisely how σ^2_u and $\sigma^2_{u_n}$ generate attenuation toward zero relative to the true β.
10. Note that if uncorrelated with $\ln(N_i)$, u, u_n, and u_r will inflate residual variance and affect precision rather than biasing $\widehat{\beta}$.

the sample period. Such time-invariant components will be absorbed in our empirical specifications by including country and parent fixed effects and hence will not affect the estimated slope of $\widehat{\beta}$.

While tax planning reasons, such as earnings management to adjust revenue figures or deliberate misreporting of revenues to deceive stakeholders, may also generate a mismatch between reported and actual revenue (Hanlon and Heitzman 2010), many of these are likely to be constant over time across parent firms and be absorbed by parent fixed effects. However, *changes* in tax incentives or enforcement may induce subsidiaries to adjust reporting practices over time (Suárez Serrato 2018; Bilicka et al., 2022). If these covary with nightlights, they could bias the estimate of $\widehat{\beta}$ even when country and parent fixed effects are included. Further, revenue reporting wedge may vary systematically with scale of operations. For example, if larger and more capable subsidiaries have greater ability or incentives to engage in misreporting or tax-planning activities (Hines and Rice 1994; Bilicka and Scur 2024), then the wedge can be correlated with observed nightlights even after conditioning on country and parent fixed effects. To mitigate these two concerns, we (1) include time-varying country fixed effects, (2) exploit the panel structure of the data to include subsidiary and year fixed effects,[11] and (3) examine whether the estimated relationship differs across the subsidiary size distribution in specifications without subsidiary fixed effects.[12]

3.2 Measurement errors in nightlight

While it is standard in the nightlight literature to assume that $\varphi = 1$, factories occupy relatively small geographical units of data which may raise concerns that nightlight data in our context is more severely affected by measurement error related to blooming and light emission location accuracy.

We mitigate this potential bias in the data in three distinct ways. First, we use Black Marble data, which improves cleaning procedures and the consistency of measurement angles with respect to the standard VIRS data provided by Elvidge et al. (2021). Second, in all estimations, we directly control for blooming by including nightlight emitted by the 5 km surrounding area radius excluding the factory itself. Third, we document empirically that nightlight captures firm activities across the size distribution of factories well, but we acknowledge that the limitation of using nightlight data to proxy for firm activity is that the factory must occupy a large enough site.

3.3 Is γ constant across firms?

Another concern with our approach is that to recover $\widehat{\beta}$ in specifications without subsidiary fixed effects, we assume that γ is constant across all firms. However, it is possible that the relationship between revenues and nightlight is firm specific and reflects differences in how different *types* of firms operate. For example, firms may emit different amounts of nightlight across the production chain; upstream firms that assemble cars might emit more light than downstream firms that focus on high-tech R&D or components production, despite having the same revenue. Another example is that car manufacturers differ in how frequent the supplies are; factories that belong to those that use just-in-time production may emit more light as they rely on more frequent supplies than firms that stock up in bulk. Further, car manufacturers differ in how they assemble their cars, some produce all components in one place, while others rely on frequent supplies across their factories. Empirically, we show that our estimates do not substantially vary across some of the observable firm and factory characteristics in Section 4.2 to assuage concerns about this assumption.

11. Note that we write Equation (4) as a static one, without subscripts *t*. We compare results from empirical estimations that include different fixed effects in detail in Section 4.1.
12. There is another potential source of bias with this approach that stems from proxying for firm activities using turnover. In the context of the manufacturing industry, turnover typically refers to the total sales or revenue generated by a company, which we believe summarizes the activity of firms most comprehensively. Still, in Section 5.1, we show that our results do not differ if we use other firm activity measures such as assets, or employment, which suggests this approach does not introduce additional bias.

3.4 Independence of error terms

A critical assumption underpinning the validity of our empirical approach is the independence of the error terms u_r, u_n, and u from each other and from the nightlight data. In our context, there are at least two potential threats that could invalidate this assumption. First, the share of R&D spending in a factory may affect the true relationship between nightlight and revenues (as described above) *and* it may also give firms differential capabilities to report biased revenues. The literature on profit shifting indicates that firms that have a lot of intangible, hard to value, capital are more likely to shift profits across borders and are more responsive to reforms that intend to curb such shifting (Davies et al., 2018; Liu et al., 2020). Second, a combination of higher corporate tax rates and higher capital allowances may make it more profitable to focus on activities related to tangible assets and as we already pointed out firms with more tangible assets may have a different relationship between true revenues and nightlight.

While this assumption cannot be empirically tested, our split sample analysis helps us validate its plausibility. We show that across upstream and downstream firms producing different products, with different business line types, our estimates of the relationship between nightlight and revenues remain the same.

4. Is nightlight correlated with reported firm activities?

To quantify the correlations between nightlight and firm activity, we match our factory-level data with subsidiary-level annual financial information. The unit of observation in this part of the analysis is a reporting firm, i.e. a subsidiary of the car manufacturing multinational, such as, for example, Ford Mexico.

We summarize the correlation between nightlight and firm activity, proxied by revenues, in Table 3. In Columns 1–2, we present results using a cross-sectional variation, while in Columns 3–5, we use panel-level data. We start by presenting raw correlations in columns 1 and 3, then include country and parent fixed effects in columns 2 and 4, and finally firm and year fixed effects in column 5. We show that across estimates in columns 1–4, a 1 per cent difference in nightlight emissions between subsidiaries is associated with around 0.8 per cent difference in subsidiary revenues. Estimates from column (5) suggest that

Table 3. Correlations between firm activities and nightlight.

	Cross section		Panel		
	(1) ln(turnover)	(2) ln(turnover)	(3) ln(turnover)	(4) ln(turnover)	(5) ln(turnover)
$ln(N_i)$	0.790***	0.786***	0.812***	0.791***	0.664***
	(0.060)	(0.049)	(0.054)	(0.038)	(0.146)
Country FE		✓		✓	
Parent FE		✓		✓	
Firm FE					✓
Year FE					✓
Observations	320	309	1,496	1,484	1,455
R^2	0.374	0.711	0.477	0.699	0.959
R^2 **within**	0.374	0.485	0.477	0.507	0.0990

Note: The table presents the correlation between firm activities, measured by the logarithm of firm turnover, and nightlight emissions using the Black Marble cleaning procedure. In columns 1 and 2, we show results for collapsed cross-sectional averages for 2012–2019. In columns 3–5, we show results using annual data for 2012–2019. $ln(N_i)$ is the logarithm of the total light emitted by a factory. In all specifications we control for the nightlight emitted within a 5 km radius around the factory excluding the factory itself. Robust standard errors are clustered at the country level. R^2 within refers to the within variation, as indicated by the fixed effects included.
***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

a 1 per cent difference in nightlight emissions between subsidiaries is associated with around 0.66 per cent difference in subsidiary revenues.[13]

Under the assumptions discussed in Section 3, we use point estimates in columns (4) and (5) to calculate the upper bound of the structural elasticity of interest that describes the true relationship between nightlight and firm activities. We find these upper bounds to be $1/0.664 \approx 1.5$ and $1/0.791 \approx 1.3$. In Henderson et al. (2012) the range of country-level elasticities under various assumptions about the signal-to-noise ratio is between 1.03 and 1.72. This puts our estimates well within the range of the existing country-level evidence.[14]

Including country and parent fixed effects in the estimations relative to raw correlations does not change the magnitude of the estimated relationship between nightlight and turnover in either cross-sectional or panel data. This suggests that these time-invariant country-level or parent-level sources of error are unlikely to be correlated with nightlight and hence do not bias the estimate of $\widehat{\beta}$. While estimates that include subsidiary and year fixed effects are smaller, this is consistent with within estimators potentially increasing attenuation bias by reducing the signal-to-noise ratio in observed nightlight. However, the difference in magnitude between specifications with and without subsidiary fixed effects is not statistically significant which suggests that even these time-invariant subsidiary-level differences are not likely to be strongly correlated with nightlight and, consequently, do not substantially bias the estimates of $\widehat{\beta}$.

To assess whether nightlight data is a good proxy for economic activity, we consider how much variation in reported revenue can be explained by nightlight data. This is useful if we want to use nightlight data to predict firm activity for firms that do not report such activity. We treat this discussion as descriptive evidence on in-sample explanatory power of our specifications rather than a forecasting exercise, and to evaluate that we report R^2 and within-R^2 in Table 3.[15] We find that nightlight itself explains a large amount of the overall variation in firm activity, 37–48 per cent. However, this predictive power is driven by the ability of light to explain the cross-sectional variation. Comparing the overall and within R^2 in the panel estimates in column 5, we find that nightlight data explains 10 per cent of the variation in firm activities over time. Relative to prior literature, such as Henderson et al. (2012) who find R^2 of about 21 per cent at the country level, this is arguably still very high. Further, a comparison of R^2 between columns 3 and 4 reveals that parent and country fixed effects capture a large degree of variation in our data.[16]

If we want to use nightlight data to predict firm activity for firms that do not report such activity, our results suggest that panel data estimates that include subsidiary fixed effects allow us to explain the largest variation in firm activity. However, using those estimates is problematic, since in most cases when a firm does not report its activity it does so consistently across years, which means that the firm fixed effects estimates cannot be used. Results from columns (2) and (4) of Table 3 suggest that for both the cross-sectional and panel data, our estimates that use country and parent fixed effects have very similar and high predictive power. In that case, we can use those estimates to predict firm activity when a firm is missing information but other firms of the same parent company and other firms that operate in the

13. We visualize all of the estimates from Table 3 in Figure B4 (see online supplementary material for a color version of this figure) plotting nightlight and turnover net of different fixed effects. We also include fit lines to validate the appropriateness of the linear specifications.
14. To keep the quality of the nightlight data high, we implement a set of stringent cleaning procedures, which involve many choices of which observations to keep. In Figure D1 (see online supplementary material for a color version of this figure), we show that the magnitudes of our estimates are not driven by these choices nor are they sensitive to including additional control variables.
15. Another way to evaluate the predictive power of various specification is to use out-of-sample cross-validation. We explored this using Monte Carlo procedure but do not include the results in the paper due to small sample of firms. These results are available upon request.
16. Note that adding year fixed effects to column 4 does not change the magnitude of the coefficient estimate nor does it materially increase the R^2. Also, removing the neighboring pixels controls from these estimates does not affect the R^2 materially; they change from 37 per cent to 36 per cent in column 1 and from 48 per cent to 47 per cent in column 3 with no changes across other columns.

same country are not. This is a much less demanding requirement on what data we need to make our predictions than in the context of the firm-fixed effect estimates.

4.1 Differences between predicted and reported firm activities

While our estimates from Table 3 suggest that the correlation between nightlight and firm activities is strong, it is worth examining what types of firms we make good predictions for and what types of firms our approach cannot capture well. To do that, we use estimates from Table 3 to generate predictions for firm activity based on nightlight data. We then plot these *predicted* revenues against *reported* revenues in Fig. 2 and include a 45-degree line for reference. We follow the structure of Table 3 and plot different predicted revenues that *include* varying sets of fixed effects on the horizontal axis across five panels and in each case use the reported revenues on the vertical axis. Panels (a) and (b) use cross-sectional data, while panels (c)–(e) use panel data. This is different from a common approach in the literature, such as Henderson et al. (2012), where fixed effects are partialed out from both the nightlight and turnover data. Instead, we include the fixed effects in the predictions of turnover to demonstrate their additional predictive power against the reported turnover.

Evidence from panels (a) and (c) which use predicted revenues from regressions without any fixed effect suggests that for the majority of firms we are able to predict revenue well. However, there exist some outliers for which we are not able to do so. For dots arranged alongside the 45-degree line, our nightlight data is a perfect proxy for firm activities. For dots above the 45-degree line, we predict less activity than firms reported in the financial data, while for dots below the 45-degree line, we predict more activity than reported in the financial data. In panels (a) and (c), we see a large mass of subsidiaries in the bottom right-hand side corner which have a lot of activity when measured using nightlight data, but they report to have relatively small revenue in the financial data.

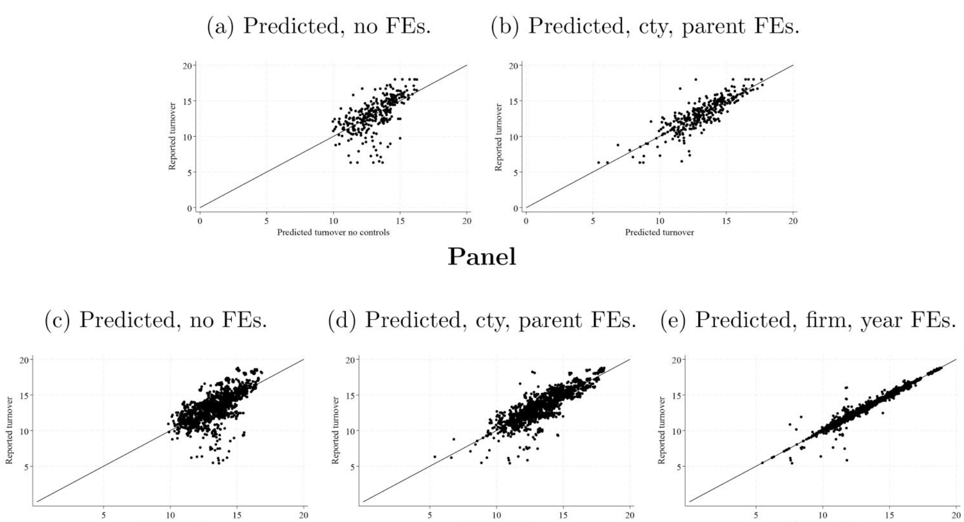

Figure 2. The correlations between predicted and reported firm activities.

Note: This figure presents the relationship between predicted and reported turnover. Each dot corresponds to a single subsidiary. All variables are in logs. In all panels, we include reported turnover on the vertical axis. On the horizontal axis, we include predicted turnover. To calculate predicted turnover we regress firm turnover on nightlight and take predicted values from regressions that include varying sets of fixed effects. In panels (a) and (c), we include no fixed effects, in panels (b) and (d), we include country and parent FEs, and in panel (e) we include firm and year FEs. In panels (a) and (b), we use cross sectional data, and in panels (c)–(e) we use panel data. The solid black line is a 45 degree line. Panels (a)–(e) use predicted values from regressions that correspond directly to columns 1–5 in Table 3.

We then proceed to use the information generated by panels (a) and (c) to understand the characteristics of the firms for which we make large prediction errors, i.e. those firms in the bottom right-hand side corner of these graphs. To do that, we calculate the size of the prediction error as the difference between the reported and predicted revenue and compare characteristics of the firms in the top decile of the distribution of that prediction error to those firms in deciles 4–7 for which we make very small or no prediction errors. The results in Table C3 (see online supplementary material for a color version of this table) show that we are much more likely to make larger prediction errors for firms in urban areas with a lot of buildup around them, as well as for firms in countries with higher GDP per capita and higher corporate tax rates.

Including country and parent fixed effects in predicted turnover in panels (b) and (d) reveals that most of these outliers are likely due to differences between countries and between car manufacturers in how well we can predict firm activities. In fact, in Tables C1 and C2 (see online supplementary material for a color version of these tables) we show that it is indeed the case that for some countries, such as Canada or Mexico, and some car manufacturers, such as Mitsubishi or Subaru, we systematically make large errors. This is why it is crucial to include these fixed effects when we want to predict firm activity based on nightlight data.

While country and parent fixed effects account for static differences between countries and parent firms, a concern that evidence from Table C3 (see online supplementary material for a color version of this table) highlights is that differences between firm activities and nightlight can be heterogeneous across firms of different sizes, making different products in differently urbanized locations, or across places that experience changes over time in country characteristics, such as GDP per capita or tax rates. Evidence from panel (e) implies that once we include firm and year fixed effects that account for those concerns in our predictions of firm turnover, our predictive approach works almost perfectly. However, as discussed above, using firm fixed effect estimates to make predictions for firms with missing data may not be feasible. Consequently, in what follows we discuss how firm heterogeneity and changes over time within countries affect our estimates of the relationship between nightlight and firm activity in estimations that do not include firm-fixed effects.

4.2 Heterogeneity across firms

A potential concern with estimating the average relationship between firm activities and nightlight without including firm fixed effects is that firms differ in their nightlight emission across their size or position in the supply chain. In particular, factories that source car parts may be very different from car assemblies or bus assemblies in terms of size, the type of operations that they have, and the relationship between firm activities and nightlight. If that is the case, this affects the validity of assumptions discussed in Section 3. In Fig. 3, we show five sets of empirical results that analyze the heterogeneity in the relationship between nightlight and firm activity across firm characteristics. In each of those specifications, we control for country and parent fixed effects; hence, the variation we are exploring here is the one arising from differences in the firm characteristics within parent firms and within countries.

In panel (a), we show results from quantile regressions in which we estimate the relationship between nightlight and firm revenues across the distribution of revenues. In panel (b), we show the heterogeneity across firm size distribution measured by quantiles of the distribution of the area that the firm occupies. In panel (c), we explore heterogeneity in our baseline estimates for firms that are responsible for different types of products, such as parts, engines, cars, or trucks. In panel (d), we show heterogeneity in our baseline estimates across different stages of production. In panel (e), we split firms according to whether they are more likely to be an upstream assembly factory or a downstream parts factory. We proxy for upstreamness with the presence of a large assembly parking lot in at least one factory that belongs to that subsidiary.

The dashed vertical lines in each panel indicate the baseline estimates from column 2 in Table 3 which these coefficients are directly comparable to. The results suggest that our estimates are quite consistently

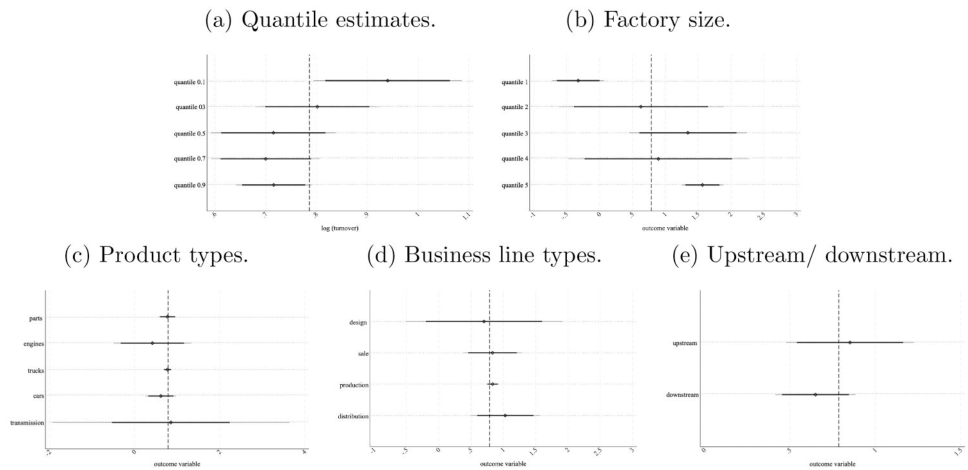

Figure 3. Distributional effects.

Note: This figure presents the correlation between firm activities measured by turnover and nightlight emissions across the distribution of firm and factory characteristics. Each dot plots a coefficient from estimating a regression of logarithm of turnover on $ln(N_i)$, where the latter variable is defined as the logarithm of the total light emitted by a factory. In panel (a), we estimate quantile regressions, where the quantiles are indicated on the vertical axis. In panel (b), we consider heterogeneity across factory footprints as measured by the quantiles of the distribution of the average area that each factory occupies. The quantiles are indicated on the vertical axis, with Q1 corresponding to smallest firms and Q5 to the largest ones. In panel (c), we consider heterogeneity across subsidiaries that produce different types of products, as indicated on the vertical axis. In panel (d), we consider heterogeneity across subsidiaries that engage in different stages of productions process, as indicated on the vertical axis. In panel (e), we compare upstream and downstream firms, where upstream is proxied by the presence of an assembly parking lot. Information in panels (b) and (e) comes from factory footprints data; information in panels (a), (c), and (d) comes from Orbis subsidiary dataset information on primary business line and main products and services. We estimate each of these coefficients for the collapsed cross-sectional averages for 2012–2019 and include country and parent fixed effects. We cluster at the country level. The vertical line in each plot indicates the corresponding baseline average coefficient from column 2 in Table 3.

predicting a very similar relationship between nightlight and firm revenues across different firm sizes and for different product and business line types. Panel (a) results confirm that nightlight is similarly correlated with revenues across the distribution of observed revenues and that our results are not driven by outliers. While the magnitude of the correlation is higher at the 10th percentile of revenue distribution, it is not significantly different from the correlation at the median.

Consistent with our discussion in Section 3, nightlight does not predict firm activity for subsidiaries that occupy a small area well. This is evident in panel (b), where nightlight is not correlated with firm activity for firms in the smallest quantile of the area distribution. Evidence from panels (c) and (d) shows that our estimates are similar across firms that are responsible for different product types across different stages of production. In panel (e), we find the magnitude of the coefficient for downstream firms to be slightly smaller than for upstream firms, suggesting that nightlight may be less precise in proxying for activities of downstream firms. However, the estimated coefficients are not statistically significantly different between upstream and downstream firms. In all, evidence from this figure allows us to conclude that nightlight data is a good predictor of firm activities across different firm characteristics and the size of the potential bias that could be introduced by assuming no heterogeneity in how nightlight predicts firm activities across firms is relatively small even in the estimations that only control for country and parent fixed effects.

4.3 Heterogeneity across time and countries

Another potential concern in using predictions from regressions of nightlight on firm revenue controlling only for country and parent fixed effects is that the relationship between nightlight and firm activities could change within countries over time due to evolving institutional conditions or reporting

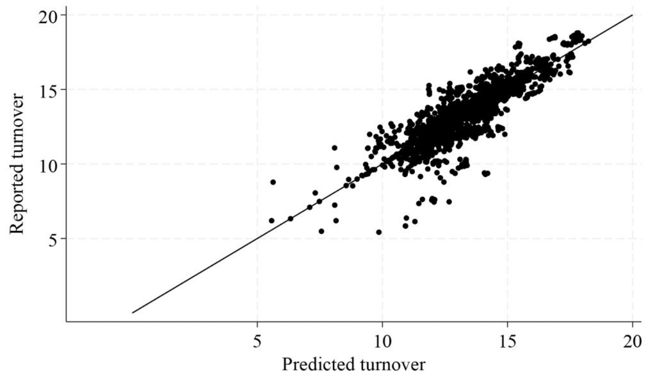

Figure 4. The correlations between predicted and reported firm activities.

Note: This figure presents the relationship between predicted and reported turnover in the panel data. Each dot corresponds to a single subsidiary. All variables are in logs. We include reported turnover on the vertical axis. On the horizontal axis, we include predicted turnover. To calculate predicted turnover we regress firm turnover on nightlight and take predicted values from regressions that includes parent and time-varying country fixed effects. The solid black line is a 45 degree line.

incentives. This could bias the estimates of $\widehat{\beta}$ if these time-varying components of the error term are correlated with nightlights conditional on fixed effects. To assess the importance of such time-varying factors, we estimate specifications that include time-varying country fixed effects and compare coefficient estimates from those regressions to the baseline.

We present results in Fig. 4. If changes within countries over time were correlated with nightlight and could bias the estimates of β, we would expect the difference between Fig. 4 and panel (d) in Fig. 2 to be large. We do not find that which suggests that changes over time at the country level are not likely to bias our estimates. We report coefficient estimates from this regression in column 1 of Table C4 (see online supplementary material for a color version of this table) and find the magnitude of the correlation to be 0.773, which is very similar to the estimates using country and parent fixed effects only.[17]

5. What type of economic activity is measured by nightlight?

In our main estimation, we focus on revenue as our central measure of firm activity. In what follows, we examine what light can explicitly measure and consequently what firm activity empirically is. We do so in three steps. First, we consider correlations between nightlight and different measures of firm activity. Second, we examine how extensive and intensive margin changes in nightlight shape the overall relationship between total nightlight and firm activity. Third, we utilize factory closures due to the outbreak of Covid-19 to provide causal evidence and disentangle the effects coming from fixed structures and from employment movement.

5.1 Different measures of firm activity

We start by replicating our estimates from columns 2 and 5 in Table 3 for total assets, employment, fixed assets, and profit and loss before tax. We present these estimates in columns 1–4 of Table 4, where panel A results correspond to those in column 2 in Table 3, while panel B results correspond to those in column 5 in Table 3. Comparing these estimates to our baseline shows that nightlight is also positively correlated with

17. For completeness, we also include results that control for the time-varying country and firm controls, such as for example, GDP, GDP per capita, tax rates, degree of urbanization near factory, and number of plants that belong to a firm and find the correlation coefficient between nightlight and firm activity not to be affected.

Table 4. Correlations between firm activities and nightlight: alternative firm activity measures and comparison between intensity of nightlight and land use.

Panel A: cross sectional data	**(1) ln(assets)**	**(2) ln(employment)**	**(3) ln(fixed assets)**	**(4) ln(profits)**	**(5) ln(turnover)**
$ln(N_i)$	0.706***	0.640***	0.732***	0.777***	
	(0.047)	(0.049)	(0.117)	(0.070)	
$ln(N_i/area_i)$					0.743***
					(0.227)
$ln(area_i)$					0.812***
					(0.083)
Country FE	✓	✓	✓	✓	✓
Parent FE	✓	✓	✓	✓	✓
Observations	263	268	267	197	309
R^2	0.766	0.703	0.719	0.721	0.711
R^2 **within**	0.543	0.488	0.415	0.429	0.486
Panel B: panel data					
$ln(N_i)$	0.317***	0.412**	0.270***	0.626**	
	(0.075)	(0.176)	(0.092)	(0.256)	
$ln(N_i/area_i)$					0.612***
					(0.170)
$ln(area_i)$					0.848**
					(0.280)
Firm FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Observations	1,379	1,008	1,378	1,035	1,455
R^2	0.984	0.953	0.974	0.901	0.958
R^2 **within**	0.0660	0.0449	0.0190	0.0197	0.0977

Note: The table presents the correlation between different measures of firm activities and nightlight emissions in columns 1–4. In column 5, we show the correlation between turnover, land use, and land-use intensity. In panel A, we show results for collapsed cross-sectional averages for 2012–2019 and include country and parent fixed effects. In panel B, we show results using annual data for 2012–2019 and include year and firm fixed effects. The dependent variable is the logarithm of total assets in column (1), the logarithm of the number of employees in column (2), the logarithm of fixed assets in column (3), the logarithm of profit and loss before taxes in column (4), and the logarithm of turnover in column (5). $ln(N_i)$ is the logarithm of the total light emitted by a factory, $ln(area_i)$ is the area, and $ln(N_i/area_i)$ is the average light emitted per area. Robust standard errors are clustered at the country level. R^2 within refers to the within country and parent variation in panel A and within firm variation in panel B.
***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

other measures of firm activity. The magnitudes of the correlations presented here are smaller than the ones presented for revenue, both in the case of cross-sectional and panel estimates, but nightlight explains a similar amount of variation in each of those firm activity measures as it does for revenue. We find weak evidence that nightlight predicts employment and profits with lower accuracy, as we observe larger standard errors for those variables in panel B. This could suggest that nightlight is mainly a good proxy for assets. However, these estimates do not allow us to directly disentangle what nightlight measures, given a very strong correlation between financial variables in both the cross-sectional and panel data (Table B2, see online supplementary material for a color version of this table). We turn to this in the next subsection.

5.2 Extensive versus intensive margin

Previous literature has shown that nightlight emitted by regions or countries is a good proxy for overall regional productivity. Conceptually, there are two possible sources of change in nightlight emissions. The first is the increase in the intensity of light per pixel; the second is the increase in the share of pixels with positive light emission in the observed area. Light is a good proxy for economic activity because both of these sources are good indicators of economic development. Higher land use (extensive margin) and more intensive land use (intensive margin) are commonly associated with higher productivity. The distinction between these two sources is key to understanding what drives changes in GDP.

To disentangle the effects of land use from land use intensity in the context of firm-level activity, we use our subsidiary-level data and measure the land occupied by factories *and* the intensity of light produced by that given area to estimate the following model, which closely follows Equation (4).[18]

$$ln(R_i) = \alpha + \beta_1 \times ln(area_i) + \beta_2 \times ln(N_i/area_i) + X_i^{\prime}\delta + \varepsilon_i \tag{5}$$

where i is a subsidiary, $ln(area_i)$ is the logarithm of the area that all factories belonging to that subsidiary occupy and $ln(N_i/area_i)$ is the logarithm of the average light emitted by those factories per area they occupy. $ln(R_i)$ is the logarithm of revenues. $X_i^{\prime}$ is a set of subsidiary-level control variables. We summarize the results in column 5 of Table 4, where panel A reports results using cross-sectional data and panel B uses panel data.[19] These results correspond directly to estimates from columns 2 and 5 in Table 3. We find that both land use and land use intensity are similarly good predictors of differences in revenue between firms in a cross section and for changes in revenue in a panel. The magnitude of the coefficient suggests that a 1 per cent increase in the area occupied by a factory increases revenue by 0.8 per cent and a 1 per cent increase in light intensity increases revenue by 0.6 per cent, but these estimates are not statistically significantly different from one another. Further, their magnitude is similar to what we show in Table 3. This evidence suggests that nightlight has the ability to proxy almost equally well for both the area and the intensity of nightlight when we use granular firm-level data.

5.3 Is nightlight causally linked with the use of capital?

To quantify how much nightlight is emitted because of the existence of capital stock on the ground and how much is emitted due to its utilization, we use a difference-in-differences design combined with an event-study specification. In the spring of 2020, almost all the car manufacturers around the globe closed their factories to prevent the spread of the Covid-19 pandemic. This shock offers a unique setting to examine the effects of short-term changes in activity in, and around, these factories on the emission of nightlight. Any change in the light emitted by factories around these events is likely related to changes in short-run production activities, such as running machines, employees coming to work, or transporting final and intermediate products. We use this shock to measure how a complete shutdown of a factory affects the amount of light emitted by that factory.

5.3.1 Estimation approach

We use the difference-in-differences approach to investigate the effect of Covid-19 factory closures on nightlight emitted by those factories. In the baseline model, we compare nightlight emitted by factories 3 weeks before and after the factory closures, up until the official factory reopening date. We do not have traditional control and treated groups as almost all factories around the world closed due to Covid-19 at some point during 2020. Instead, we construct our control group using the same factory on the same day in 2019. This approach controls for the fact that light is different at each latitude and longitude at each time of the year, i.e. for the systematic influence of the so-called “stray lights.” As such, in our estimations,

18. Note that the land occupied by a region is typically fixed over time. Consequently, with the traditionally used log-log estimates and panel data, findings based on nightlight intensity are conceptually identical to estimates based on the sum of light.
19. In Figure B3 (see online supplementary material for a color version of this figure), we show the variation in the intensity of nightlight and the area that we utilize in our exercise.

we compare the effects of factory closure on light emitted in 2020 relative to light emitted by the same factory on the same day in 2019.

We further account for the potential blooming light effect coming from the nearby neighborhood areas. Because we are dealing with very small units of observations relative to previous literature, the concern we have is that lights emitted by our factories may be contaminated by the effect of the blooming lights coming from the nearby neighborhood areas. To make sure that we pick up the effect of factory closure, rather than nearby lockdown and stay-at-home orders that countries have introduced in response to Covid-19, we control for the average nightlight emission within a 5 km radius around each production site excluding the factory itself.

Our identification strategy relies on the assumption that in the absence of Covid-19 closure, the amount of light emitted by factories would evolve in the same way in 2020 as in 2019 during non-cloudy nights. Consequently, to verify the plausibility of this assumption *and* to estimate the dynamic effect of closure on nightlight emitted by these factories, we use an event study design. For this, we aggregate our nightlight data on a weekly basis. We use a week before Covid-19 closure as a benchmark and normalize all coefficients to zero in that week. Hence, we estimate the following equation:

$$ln(N_{i,w}) = \alpha + \sum_{\kappa=-3}^{3} \delta_\kappa I(2020) \times 1[w=\kappa] + X'_{iw}\gamma + \psi_i + \mu_w + \varepsilon_{i,w} \tag{6}$$

where, i is a factory, w is weeks. $ln(N_{i,w})$ is light emitted by a factory in each week; $\sum_{\kappa=-3}^{3} 1[w=\kappa]$ is a series of week dummies that equal to one in each of the κ weeks away from the closure date, with the dummy variable corresponding to $\kappa=-1$ as the omitted category. $I(2020)$ is a dummy variable that equals 1 in 2020 and zero in 2019. X'_{iw} includes the cloud cover in the 10 km radius and the nightlight emission in the 5 km radius around the factory excluding the factory itself; ψ_i is the factory-specific fixed effect, μ_w are fixed effects for each week relative to Covid-19 week, and $\varepsilon_{i,w}$ is the error term. We estimate the model using 3 weeks before and after each factory closure, binning coefficients at those endpoints, and do not report them. The coefficients of interest are the δ_κ, which measure the average difference in lights in each week relative to the week before the Covid-19 closure in 2020 relative to 2019.

We also estimate a more general difference-in-differences specification in which we use the daily data and keep only observations 3 weeks before and after Covid-19 closure to correspond directly to the event study plots from Equation (6).

$$ln(N_{i,d}) = \alpha + \beta \times post_d \times I(2020) + X'_{id}\delta + \psi_i + \mu_w + \varepsilon_{i,d} \tag{7}$$

where $post_d$ is a dummy equal to 1 starting the day the factory closed due to Covid-19, while all the remaining variables are defined as in Equation (6). Here, the parameter of interest is β, which captures the average effect of the Covid-19 closure on factory lights relative to the control group.

5.3.2 Summary of results

We plot the coefficients from the dynamic estimation in Fig. 5, with the corresponding coefficient magnitudes reported in column 3 in Table 5. Blue hollow diamonds correspond to coefficient estimates, while vertical lines are 95 per cent confidence intervals. We find that in the week that factories close due to Covid-19, there is an almost 14 per cent reduction in nightlight emitted by those factories. This effect persists in week 1 and gradually declines. By week 3, a lot of factories in our sample started carrying out government orders for masks or ventilators production and ground activity started picking back up. Further, we find no difference in the light emitted by the affected factories relative to 2019 before the Covid-19 closure, which suggests the identifying assumption is plausible and the estimated effect can be interpreted as the causal effect of closure.

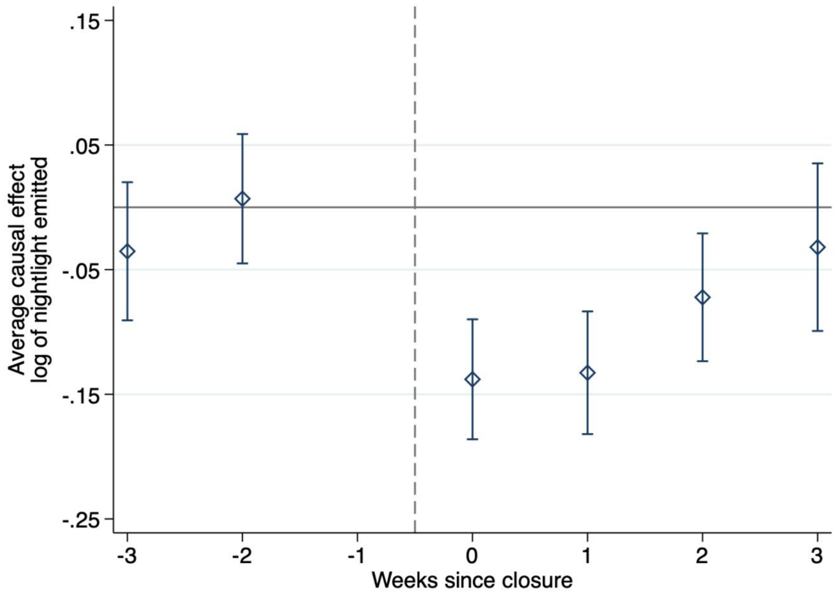

Figure 5. Dynamic plot: the effect of factory closures on nightlight emitted.

Note: In this figure, we plot the coefficient estimates of the effect of Covid-19 closure on light emitted by factories using a dynamic specification. Hollow diamonds represent coefficient estimates, δ_κ, from a regression that takes the form $ln(N_{i,w}) = \alpha + \sum_{\kappa=-3}^{3} \delta_\kappa I(2020) \times 1[w=\kappa] + X'_{iw}\gamma + \psi_i + \mu_w + \varepsilon_{i,w}$, where, i is a factory, w is weeks. $ln(N_{i,w})$ is light emitted by a factory in each week; $\sum_{\kappa=-3}^{3} 1[w=\kappa]$ is a series of week dummies that equal one in each of the κ weeks away from the closure date, with the dummy variable corresponding to $\kappa = -1$ as the omitted category. $I(2020)$ is a dummy variable that equals 1 in 2020 and zero in 2019. X'_{iw} includes the cloud cover in the 10km radius around the factory, excluding the factory itself; ψ_i is the factory-specific fixed effect, μ_w are week relative to Covid fixed effects and $\varepsilon_{i,w}$ is the error term. δ_κ coefficients, plotted as hollow diamonds here, represent the difference in nightlight emitted in each week relative to the closure week, relative to year 2019. The vertical lines represent the 95% confidence intervals. Standard errors are clustered at the factory level. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

The magnitude of the estimated effect suggests that production activity has a significant, but small effect on the light emitted by production sites. Note that factory closures were accompanied by an almost 100 per cent reduction in the labor force. Hence, an almost 14 per cent reduction in nightlight emitted indicates that lights are mostly related to the existing production facilities, rather than short-run operational activities. As such, firm activity that our correlational estimates from Section 4 proxy for is largely capital on the ground, not necessarily the utilization of such capital.

In columns 1 and 2 in Table 5, we report the average effect of the factory closure across 3 weeks after the factory closed down. The magnitude of the effect in column 2 suggests a 7 per cent reduction in nightlight emitted across those 3 weeks. This effect is statistically significant at 1 per cent level. This is consistent with the event study result, in which the effect gradually declines following the official closure dates. The magnitude of the effect in column 1, where we do not control for the logarithm of nightlight in the surrounding 5 km radius, is larger than in our preferred specification. One potential explanation is that this coefficient captures not only changes in the nightlight emitted by the factories but also changes in nightlight emitted by the nearby residential areas. This is likely since the Covid-19 lockdown also affected the residential areas with stay-at-home orders. Consequently, we believe that the difference in the estimated magnitudes between specifications in columns 1 and 2 is not related to the measurement error.[20]

20. We test the robustness of these estimates thoroughly in Figures D2 and D3 (see online supplementary material for a color version of these figures). We show that our results do not change when we adjust the window around the Covid-19 closure dates from 3 weeks all the way to 12 weeks. We also show that our results are robust to different cleaning procedure choices, similar to the firm-level results from Section 4.

Table 5. Nightlight and firm activity: Covid-19 production shock.

	(1) $ln(N_{i,d})$	(2) $ln(N_{i,d})$	(3) $ln(N_{i,w})$
$post_d$	0.009	−0.020	
	(0.034)	(0.031)	
$I(2020)$	−0.026	−0.029	
	(0.023)	(0.018)	
$post_d \times I(2020)$	−0.095***	−0.069***	
	(0.026)	(0.021)	
$I(2020) \times$ **week = −3**			−0.035
			(0.028)
$I(2020) \times$ **week = −2**			0.007
			(0.026)
$I(2020) \times$ **week = 0**			−0.138***
			(0.025)
$I(2020) \times$ **week = 1**			−0.133***
			(0.025)
$I(2020) \times$ **week = 2**			−0.072***
			(0.026)
$I(2020) \times$ **week = 3**			−0.032
			(0.034)
Cloud cover	✓	✓	✓
ln(light 5 km ring)		✓	✓
Factory FE	✓	✓	✓
Week FE	✓	✓	✓
Observations	9,740	9,735	9,813
R^2	0.806	0.901	0.843
R^2 **within**	0.0281	0.503	0.00985

Note: This table reports coefficient estimates from regressions looking at the effects of Covid-19 production shock on factory nightlight emissions. The unit of observation in all columns is a factory and the dependent variable is $ln(N_{i,d})$, the total light emitted by each factory. Columns 1 and 2 use daily data and show results using $post_d$ dummy, which is equal to one after a factory closed down due to the Covid-19 pandemic. $I(2020)$ is equal to 1 in year 2020 and zero in 2019. Column 3 presents the difference-in-differences coefficient estimates for each week relative to the Covid-19 week, as plotted in Fig. 5. All estimates include factory and week fixed effects. The sample is restricted to observations with no cloud coverage in a 5 km radius and observations between the first of January and the official time of reopening of factories. ln(light 5 km ring) measures the mean light within a 5 km radius around the factory excluding the factory itself. All estimates control for the percentage of cloud coverage within a 10 km radius. Robust standard errors clustered at the factory level are in parentheses. R^2 within refers to within-firm variation.

***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Finally, the magnitudes of the effect we estimate in Table 5 can be directly related to the panel estimates from Table 3. The Covid result suggests that a reduction in labor by 100 per cent (while holding ground capital fixed) reduces nightlight by 14 per cent at most, and 7 per cent on average, which implies an elasticity in the range of 0.07–0.14. In the panel data, if we put nightlight on the left-hand side and estimate the relationship between nightlight and firm employment holding fixed assets constant, we obtain an elasticity estimate of 0.09 with a P-value of .062, which is within the range of the causal estimates obtained here.

6. Conclusions and discussion

Our results offer a new dataset and a methodology that can be used to track firm-level activities more consistently, especially in places where information on such activities does not exist or is not systematically reported. We provide a nuanced perspective on what nightlight data can measure and how we should go about using it to quantify the activity of smaller geographical units. We leave collecting such data for other industries with a large ground presence for future research and we acknowledge that such data collected on a larger scale would allow researchers to understand the activities of large firms that operate across different countries with different reporting standards.

While our paper shows that nightlight can be used to measure firm activities consistently, our approach has limitations. To be able to use nightlight data to proxy for factory operations, the factory needs to have a large footprint. We recognize and empirically demonstrate that the current generation of nightlight data can capture factories that occupy spaces that fill at least one of the 500mx500m pixels very well, while our approach will not suit smaller factories. Consequently, nightlight data is likely to be a good measure of firm activity for industries such as aerospace, shipbuilding, heavy machinery, railway, defense, electronics, oil and gas, and renewable energy equipment manufacturing, as they all share similar footprints to car manufacturing. Other industries with characteristics that lend themselves to using our approach are those that require large-scale facilities, advanced machinery, complex supply chains, significant R&D investment, and a skilled workforce, making their operational and physical infrastructure comparable to that of the automotive industry. However, as the data quality and resolution get better in the future, researchers should be able to apply our approach to a wider range of manufacturing industries with smaller footprints.

The dataset collected for this paper offers researchers a unique ability to measure the allocation of subsidiary activities across parent firms with international presence without having access to subsidiary-level data. While calls for more transparency of multinational (MNC) activities across jurisdictions have been answered by mandatory public country-by-country reporting (CBCR) for the largest firms, this data is not publicly available for all firms yet, offers information aggregated at the jurisdiction level, and is still subject to strategic disclosure of MNCs who aggregate the information themselves. In contrast, our data can be obtained across all subsidiaries of MNCs consistently and can be constructed even if a particular MNC is not reporting any financial information in a given country, but other MNCs are. One interesting direction of study could be to compare the measures of predicted revenue we derive to those reported in the CBCRs.[21] This may be a useful source of information in the context of understanding the implications of the introduction of global minimum taxes that rely on our ability to measure the scale and location of firm activities across jurisdictions accurately.

Our study also suggests a range of potential applications beyond this example. First, developing countries have long been recognized to have different economic geography than developed countries. For example, they have a much higher agglomeration of economic activities in very few cities, which often results in large disparities in development between urban centers and rural areas. However, the underlying drivers and consequences of these differences remain poorly understood due to the limited availability of comprehensive data. Our findings provide a path to address this gap by examining, for instance, the effects of large-scale private investments on urbanization, trade, migration, globalization, or, more generally, economic geography. Second, our research highlights the potential value of remote sensing data for monitoring economic growth in small-scale economic units during times of crisis. Finally, our findings represent a promising initial step toward tracking firm-level activity with remote sensing data, especially given that higher-resolution data is likely to come from the new satellites being installed.[22]

Supplementary material

Supplementary material is available at *Journal of Economic Geography* online.

21. The EU Tax Observatory compiles a list of publicly available country-by-country reports here: https://www.taxobservatory.eu/repository/the-cbcr-company-database-explorer/ None of the car manufacturers on our list have this information available, but with the raising disclosure pressure from governments and the OECD, these data will likely become public soon.
22. One example of such a high-resolution data source is Woldview-3, https://worldview3.digitalglobe.com.

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