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Revisiting the relationship between economic freedom and development to account for statistical deception by autocratic regimes☆

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ABSTRACT

The literature connecting economic freedom indexes to income levels and growth generally points in the direction of a positive association. In this paper, we argue that this finding is a conservative one as the data is biased against finding *any* effects. The bias emerges as a result of the tendency of dictatorial regimes to overstate their GDP level. Dictatorships also tend to have lower scores of economic freedom. This downwardly biases any estimations of the relation between income and economic freedom. In this paper, we use recent corrections to GDP numbers – based on nighttime light intensity – to estimate the bias. We find that the true effects of economic freedom on income levels are between 1.1 and 1.62 times greater than commonly estimated. For economic growth, the bias is far smaller. Finally, we find suggestive evidence that the relationship between changes in economic freedom and economic growth is being underestimated as a result of the lies of dictators regarding GDP.

1. Introduction

The index of economic freedom most commonly used – that of Canada's Fraser Institute – was first made public in 1996 (Gwartney and Lawson, 2003; Lawson, 2019). Prior to its creation, many economists argued that secure property rights, free trade, limited regulation, and sound money mattered to economic growth. However, without any measurements of economic freedom, empirical applications were limited to case studies that had near laboratory conditions such as Hong Kong versus China, North Korea versus South Korea, Taiwan versus China etc. While the comparative evidence was quite strong (Rabushka, 1973; Grier and Tullock, 1989; Friedman and Friedman, 1980; Young, 1995), generalizability was an issue. Indeed, one could nitpick factors that limited the ability to generalize claims that economic freedom mattered to development (Schiffer, 1991; Gulati, 1992; Chang, 1993). The creation of an economic freedom index was meant to enable generalizability. While there were some proto-types of indexes pre-1996, the Fraser Institute's index has emerged as the dominant one (Gwartney and Lawson, 2023).[3]

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[3] There is also the Heritage Foundation's index but it has a degree of arbitrariness that is absent from the more data-driven index of the Fraser Institute. Additionally, there is an issue of transparency and consistency. For example, Cuba and North Korea are included in the Heritage Foundation's index despite the absence of key indicators. Moreover, the available literature suggests no sizable differences in key results between the Fraser Institute and the Heritage Foundation (Ram, 2014).

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Since 1996, hundreds of articles established (directly and indirectly) a connection between income levels and income growth to economic freedom. Early results showed strong associations between economic freedom and income levels (Easton and Walker, 1997; De Haan and Sturm, 2000). Changes in economic freedom were also found to be associated with faster economic growth (Sturm and De Haan, 2001; Grier and Grier, 2021; Alexandre et al., 2022). However, levels of economic freedom appeared less relevant with regard to income growth which led to some debates (Doucouliagos and Ulubasoglu, 2006).[4] Surveys of these early empirical results and subsequent surveys of later contributions to the literature (De Haan et al., 2006; Hall and Lawson, 2014; Lawson, 2022) tend to confirm this pattern: **(a)** strong association between economic freedom and income levels; **(b)** a strong association between changes in economic freedom and economic growth and; **(c)** a debated relationship between levels of economic freedom and economic growth.

These appear like strong results. In reality, and this is the claim we advance in this paper, they are *weak* results. More precisely, they are weak because there is a bias in the income data that goes *against* finding a positive association. Correcting for this bias shows that the relationship between economic freedom and economic development is stronger than generally depicted. The origins of the bias are found in the lower quality of GDP statistics amongst dictatorial and autocratic regimes. Because these regimes tend to inflate their GDP numbers relative to democracies, over-estimations are disproportionately concentrated within that subgroup of regimes. This one-sided set of GDP measurement errors then affects the estimation of the relationship between economic freedom and income measures[5] through the well-documented relationship between political and economic liberties (Lawson and Clark, 2010; Rode and Gwartney, 2012; Bjørnskov, 2018). As such, if the errors of GDP are concentrated on the "unfree" end of the spectrum of political regimes, it means that they are also concentrated on the "unfree" end of the spectrum of economic freedom. Ergo, the bias that we discuss.

We use a set of corrected GDP figures for 1992 to 2013 produced by Martinez (2022) to see if the three key empirical results of the economic freedom literature discussed above are affected by unadjusted GDP data. Each of our regression specifications is designed to speak to one of these three key empirical results by comparing coefficients obtained using adjusted and unadjusted GDP data. Throughout the paper, we present them in the form of ratios of the adjusted to unadjusted regression coefficients. For the relation between income and economic freedom levels, we find that the ratios are between 1.1 and 1.62. This suggests that the current literature noticeably understates the strength of the association between both variables. However, the relationship between levels of economic freedom and income growth appears is more modestly affected by swapping between the adjusted and unadjusted income figures. This is unsurprising as the magnitude of the lies of dictators would also have to vary over time in order to change the results. Finally, when we look at the effects of changes in economic freedom (rather than levels) on income growth, we find stronger effects of changes in economic freedom with the adjusted GDP numbers (but mostly after 2000). We argue that this can be explained by the fact that liberalizing regimes are also those whose lies about GDP get smaller. As such, unadjusted data will overstate pre-liberalization income relative to post-liberalization income. Adjusted data could thus show faster income growth for liberalizers.

We subject our results to a series of robustness checks. First, we replicate our results with different time windows. This is due to the issue that economic freedom values are available only every five years prior to 2000. This means that some interpolation between values of economic freedom must be used pre-2000. As this could drive our result, we replicate using only post-2000 data and find even stronger effects of economic freedom. Second, we used a different set of corrections proposed by Martinez (2022) that account for any potential biases that could be due to the level of oil production in a given country.[6] With these estimates instead, we also find stronger associations between economic freedom (levels and changes) and income (levels and changes). Finally, we also provide a series of additional robustness checks (i.e., including lags and using instrumental variables) that point in the same direction. We also replicate our results with the Heritage Foundation's Index of Economic Freedom. However, to keep the article brief, we mention these replications in the text and relegate the full results to a supplementary file.

The present article is solely concerned with how claims about associations between economic freedom and economic development. It does not attempt to deal with causal claims even though there is a rich literature on the topic of the causal relationship (Farr et al., 1998; Heckelman, 2000; Vega-Gordillo and Alvarez-Arce, 2003; Aixalá and Fabro, 2009; Lawson et al., 2019; Grier and Grier, 2021; Callais and Pavlik, 2022; Callais and Young, 2023). Our motivation for doing so is that we first seek to assess which relationships the lies of dictators can plausibly affect. This can then be used to re-investigate the existing work on the causal effects of economic freedom. As such, we see our contribution as a roadmap for future research assessing the strength of causal claims.

Our paper is divided as follows. Section 2 reviews the lies regarding GDP and the relationship between development and economic freedom. Section 3 lays out the data and our strategy. Section 4 presents the results. Section 5 concludes.

2. Dictators, Lies, and economic freedom

Dictatorships have a long record of producing questionable statistics. There are three sets of (non mutually exclusive) reasons for this.[7] First, dictatorships can lie for propaganda purposes and create outright fabrications. The lack of a free press or

[4] See also Justesen (2008), Xu and Li (2008), Peev and Mueller (2012) and Bergh and Bjørnskov (2021) for examples of the linkages between growth and levels of economic freedom.

[5] Notice the use of the plural here as this is meant to denote that we will focus on both income levels and income changes with and without the bias.

[6] We thank a helpful referee for pointing out the possible issue.

[7] There is a fourth set. Those who tend to be philosophically closer to anarchist thought point out that it has to do with the nature of the state itself. The state collects only information that it deems relevant for its activity (Rothbard, 1960; Scott, 1999). This divorces what could be called "true" well-being from "reported" well-being (i.e., national accounts producing GDP numbers). That criticism is of a different nature than the lies and distortions we deal with here and so we will not deal with it in the present paper.

competitive election means they can get away with lies more easily. In contrast, democracies feel compelled to produce data more transparently (Adiguzel et al., 2020). The presence of a free press allows scrutiny to be conducted while free elections allow for punishment of bad political behavior inclusive of lying about statistics.

Second, the production of questionable statistics is not *only* the result of a deliberate process organized by an autocrat at the top such as with the example of the Soviet Census of 1939.[8] While dictators lie about statistics to promote their regimes (e.g., Stalin's lies about population growth in the USSR), there can be lies that are accidental if they result from distorted incentives within the autocrat's bureaucracy (Magee and Doces, 2015; Gonzalez and Gilleskie, 2017; Berdine et al., 2018; Chan et al., 2019; Chen et al., 2019, 2020, 2021). For example, agents of the autocrats who fear punishment for not meeting certain targets might produce erroneous data that tilts one way. Individuals or organizations may manipulate or game the system to meet the target, rather than genuinely improving the underlying factors that the measure was intended to represent. This is not a planned outcome of the regime – i.e., there is no deliberate central plan to lie about the statistics.

Third, dictatorial regimes tend to invest relatively more in goods (notably military) whose economic value is difficult to assess and they rely on coercion to acquire resources which foil prices used to compute the value added (Higgs, 1992; Field, 2022; Geloso and Pender, 2023; Geloso, 2023). This further decreases the informational quality of data produced by autocrats. Moreover, dictatorial regimes are generally tied with smaller investments in public goods (Moselle and Polak, 2001; Deacon, 2009). If one considers high-quality public access statistics to fit that definition, then it is likely that the quality will be inferior in dictatorial regimes as well.

The production of national accounts (i.e., GDP statistics) falls prey to all three problems (deliberate lies, distorted incentives, low quality) under autocracy.[9] The best examples of this are Cuba and China – two autocratic regimes. Cuba's national accounts are constructed using dubious methodological choices are made in ways that consistently overestimate living standards (Devereux, 2021). This is an instance of deliberate lies. In China, it appears to be the result of bad incentives for the autocrats' agents. Local Chinese bureaucrats are assigned growth targets that they must meet in order to earn promotions. As a result, they falsify GDP numbers or act in ways that artificially boost numbers (Lai and Zhu, 2022). Moreover, in China, many individuals are forced into labor camps where they are producing outputs (which are measured) under threat of physical harm (whose cost to well-being is ill-measured) (Peng and Callais, 2022; Geloso, 2023). The first two problems are significantly smaller in liberal democracies and the third one is virtually non-existent.[10] As such, regardless of the reasons, we expect a greater divorce between "true" and "reported" well-being in dictatorships and autocracies than in democracies. Essentially, this is a one-sided measurement error (Millimet and Parmeter, 2022).

These lies matter to the question of assessing the true relationship between economic freedom and economic development (measured either as income levels or income growth). As we indicated in the introduction, the literature finds a strong positive association between the two (Hall and Lawson, 2014; Lawson, 2022). However, as the different panels of Fig. 1 show, economic freedom generally tends to be related to political freedom. Higher values on the economic freedom index mean higher levels of freedom. For all the measures of civil and political liberties, with the exception of the Freedom in the World report, higher values mean more freedom. The Freedom in the World report uses an inverse measure where lower values are better (Repucci and Slipowitz, 2022). The first panel shows the correlation with the Polity V index in its aggregate (Marshall and Gurr, 2020). The second uses the V-Dem component for electoral democracy (Coppedge et al., 2021).[11] The third panel shows the relation with the Cato Institute's Human Freedom Index (Vasquez et al., 2022). The fourth panel shows the relation with Freedom House's Freedom in the World Index. Regardless of the measure selected, higher levels of economic freedom is associated with higher levels of civil and political liberties. As such, through the correlation between economic freedom and political freedom, the autocrats' lies regarding GDP numbers seep in to bias our estimations of the effects of economic freedom.

In terms of the literature tying levels of economic freedom (Easton and Walker, 1997) to income levels, the issue is relatively straightforward. As unfree regimes are expected to report higher levels of GDP while governments of economically freer nations accurately report their income levels, the relationship between levels of economic freedom and income levels should be artificially muted. There is also a possible bias with respect to growth. Normally, if a dictator's lies about GDP are level-errors, estimates of growth should not be affected. Indeed, proportional changes remain the same at different levels. As such, the literature that argues that there are no relationships between the level of economic freedom and income growth (De Haan and Sturm, 2000; Sturm and De Haan, 2001; Heckelman and Stroup, 2000) should not see its conclusion materially altered by shifting to a better measurement.[12] However, this might not be true for countries that move in the direction of greater economic freedom. If an unfree nation enjoys an episode of political and economic liberalization, we should expect the lies to become smaller or vanish.[13] This means that the pre-liberalization GDP estimates may contain lies that are absent in the post-liberalization GDP estimates. If this is the case, the association between *changes* in economic freedom and income growth will also be artificially muted by the lies of dictators.

[8] The Soviet census of 1937 showed lower-than-expected population figures. After executing the civil servants in charge, Stalin ordered a new census for 1939 which was manipulated (Davies et al., 2018).

[9] It is worth pointing out that Pritchett (2000) suggested that repressive regimes could be overstating capital accumulation in national accounts while they are in fact padding the pockets of dictators. Unfortunately, with the exception of Murphy and O'Reilly (2018), we were unable to find papers that tied this type of falsified data to the issue of economic freedom as we do here. However, it does fit the first type of data falsification – deliberate lies.

[10] Although one could argue reasonably that prison labor in use in the United States has some effect (Poyker, 2019).

[11] We opted for this component rather than the more encompassing liberal democracy index. This is because multiple other components of the broader index also speak to economic freedom such that we would observe correlation by design.

[12] At the very least, any perceived stalemate in the literature should not be broken by our change in measurement. Debates should thus move to other issues (e.g., specification, construction of the index, weight to the different components) (Heckelman and Stroup, 2005).

[13] Using 48 political transitions from authoritarianism to democracy since the mid-1970s, Rode and Gwartney (2012) found that political liberalizations are often accompanied by economic liberalizations.

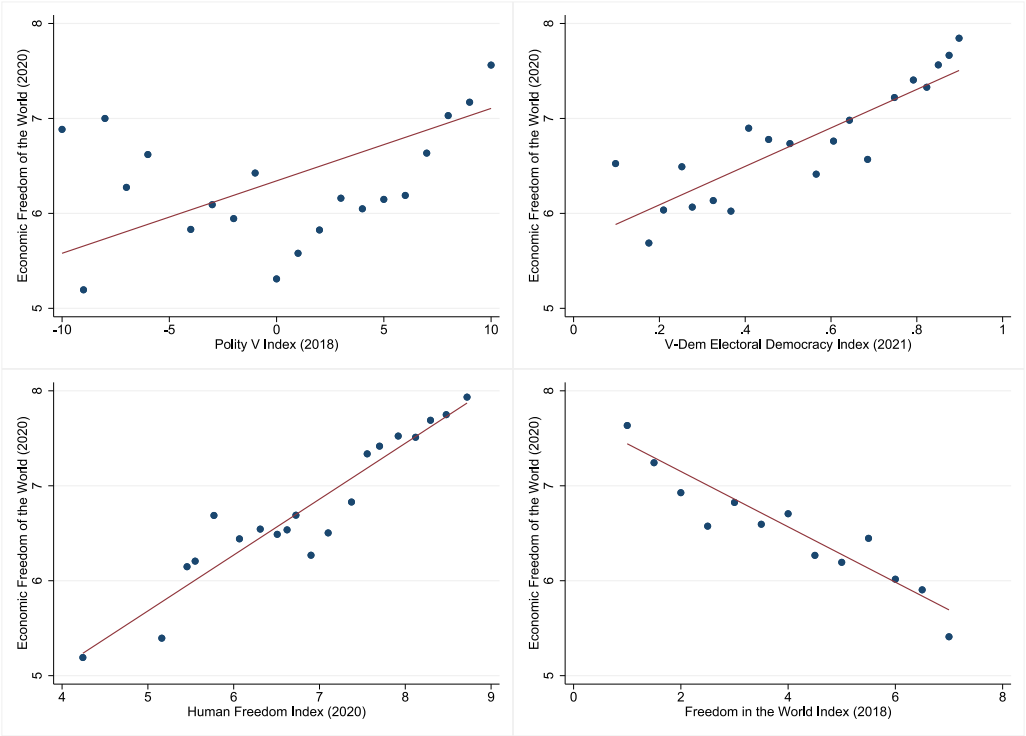

Fig. 1. Binscatter of the relation between economic freedom and different variables of political and civil liberties.

How can we repair the potential damage made by dictators' lies on our understanding of the relationship between economic freedom and development? With the notable exceptions of China (Lai and Zhu, 2022) and Cuba (Devereux, 2021), corrected GDP numbers for autocratic regimes are sparse. However, in recently published work, Martinez (2022) corrected GDP numbers between 1992 and 2013 by relying on artificial nighttime light intensity (henceforth NTL).[14] Simply put, NTL is taken to be a true "measure" of well-being because it is independently created via satellite imagery. NTL is thus immune to data manipulation. Using a log-linear regression, Martinez (2022) creates a specification where the log of GDP is explained by NTL and an interaction term between NTL and regime type. The coefficient associated with the interaction term is the autocracy gradient of the bias in the data.[15] He found that this coefficient was positive – suggesting that autocracies were over-reporting their growth rates relative to the true growth (proxied by NTL). These gradients are then extracted to adjust the figures in unfree regimes to make them comparable with the "non-massaged" figures in free regimes. In multiple appendices, Martinez (2022) conducted robustness checks – one of which was for heterogeneous elasticities where interaction terms with some key variables such as oil production were used. He found highly similar results suggesting that his results held up to specification changes. In fact, his baseline set of correction factors were closer to the lower end of the range of potential correction factors he estimates. Using his baseline corrections thus provides a conservative

[14] See also (Nye and Moul, 2007) and Michalski and Stoltz (2013). Both works relied on the use of Benford's Law regarding the distribution of the first digits of a set of numbers in order to detect lies about national accounts. The former uses GDP and the latter uses balance of payments data. However, we are reluctant of relying on these works. First, Benford's law is scale-invariant. Multiplying a set of numbers by a scalar does not affect the result of the law's application. Relying on Benford's law thus hinges on the assumption that lies about output are *not* scalar lies. However, examples provided by Devereux (2021) (for Cuba), Nakamura et al. (2016) (for China), and Davies et al. (2018) (for Soviet Russia) suggest that the lies amount to what is essentially a scalar multiple. This means that Benford's law is not a useful tool to detect these lies. Second, members of government statistical agencies know Benford's law. They thus understand – if pressed by the ruling class – how to lie in a less detectable manner. Using Benford's law entails that clever liars can more easily get away with it. Third, the latter of the two first two articles cited above uses balance of payment data rather than GDP *per se*. Its focus is narrower. Fourth, case studies of GDP manipulation exist in large numbers in a way that echoes what the articles that use night lights find (see notably the recent example of Phan (2023) as it includes a review of the existing literature). As such, we tend to assign a higher weight to papers that rely directly on light-intensity data. Fifth, the (Michalski and Stoltz, 2013) paper actually does not reach a strong conclusion. As its authors state: "Our results lead us to believe that countries with poor institutions produce BoP (Balance of Payments) data with irregular first digit behavior. However, we do not have a story why such countries would manipulate the data. The answer may be that it is not due to bad data collection procedures or methodologies". Essentially, the authors cannot make sense of why poor institutions leads to misleading data reporting. They instead assume that the "traits" of these poor institutions are what is driving the results. This is hardly a strong result on which to doubt the large body of articles that show that nighttime light data is superior at detecting lies.

[15] Martinez (2022) has a strategy that allows democracies to deviate from their true growth rate because of mundane factors such as the well-known GDP statistical discrepancy which emerges when one differences the expenditure and factor-cost estimations of GDP. Indeed, the gradient essentially creates a comparison of the bias that is *specific* to dictatorships.

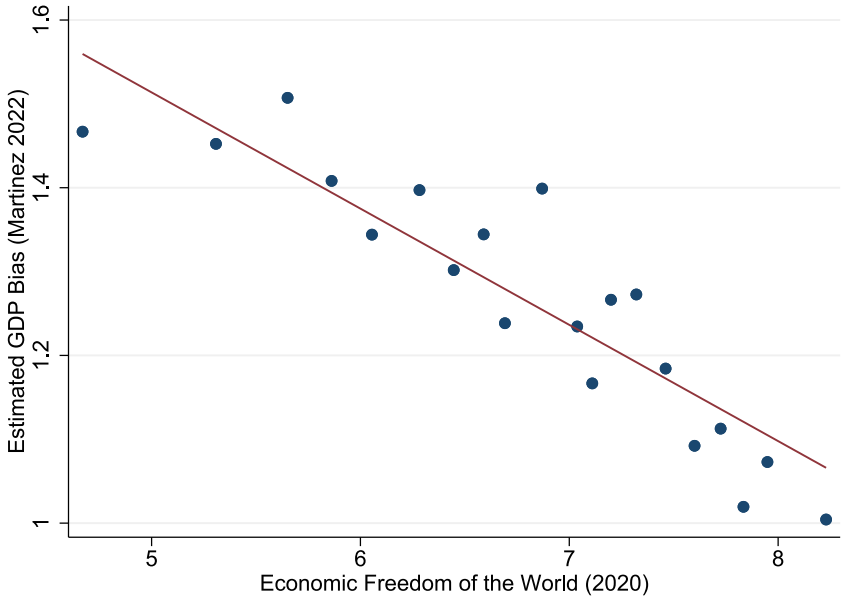

Fig. 2. Binscatter of the bias in GDP estimation found by Martinez (2022) with the level of economic freedom.

assessment of the effects of the lies of dictators regarding GDP. Moreover, recent work by Phan (2023) used a different strategy than (Martinez, 2022) and arrived at the same conclusion that lower institutional quality is tied to inflated GDP numbers.[16]

To provide an idea of the amplitude of the manipulation, Martinez broke down the data into three categories of regimes: free, partially free, and unfree. With unadjusted (total) GDP numbers, economic growth between 1992 and 2013 for unfree was close to 1.4 times that of free countries. For partially free countries, that proportion was more than 1.2 times greater. However, with the adjusted numbers, economic growth was roughly equal across all three regime categories. Given that population growth was faster in the unfree and partially free countries, this means that per capita incomes increased fastest in free countries during the period.

For the sake of illustration, in Fig. 2 we depict the errors estimated by Martinez (2022) with economic freedom on the x-axis. As can be seen, the errors are also larger for economically unfree regimes: countries that are economically unfree overestimate their GDP numbers by a factor greater than 1.4 compared to nearly no-overestimation for countries that have higher scores of economic freedom. Given Figs. 1 and 2, this means that any bias in the relationship between democracy and economic development induced by the low quality of GDP statistics in unfree regimes must be – to some degree – mirrored in the relationship between economic freedom and economic development. That bias is one that has likely led the literature to underestimate the strength of the association between economic freedom and development.

3. Data and methodology

How can we assess the importance of the underestimation of the strength of the relationship between economic freedom and economic development? Using the data produced by Martinez (2022) allows us to possess adjusted estimates of GDP to compare with unadjusted ones. However, this is only for the period from 1992 to 2013. Many studies that test the relationship between economic freedom and economic development take far different windows and they generally try to begin with the first year (i.e. 1970) of available economic freedom estimates. However, we can circumvent this issue relatively easily. Our first specifications have the following forms when using the unadjusted GDP numbers (which we divide by population to get per capita income):

$$ln(GDPPCReported)_{it} = EFW'_{it}\delta_1 + x'_{it}\beta + \alpha_1 V_i + \alpha_2 V_t + \epsilon_{it} \tag{1}$$

$$\Delta GDPPCReported_i = EFW'_i\delta_2 + \Delta x'_i\gamma + \epsilon_i \tag{2}$$

$$\Delta GDPPCReported_i = \Delta EFW'_i\delta_3 + \Delta x'_i\eta + \epsilon_i \tag{3}$$

[16] Moreover, the procedure used by Martinez (2022) echoes the results of case studies by country. For example, Devereux (2021) recreated the national accounts of Cuba from 1959 to today. He found that the gap between the official and corrected numbers was quite close to each other (less than 10% difference) with the Martinez estimates being smaller. This suggests external validity is a minimal issue and that using Martinez's corrections may be somewhat conservative. Devereux is not the first to note that the GDP numbers and other economic data of the regime are upwardly biased (Pérez-López, 1998, 2020; Hernández-Catá, 2019).

In specification (1), because we are using a panel setup, i and t represent countries and years. The vector X refers to our control variables (which we describe further below). The term V_i controls for time-invariant unobserved heterogeneity at the country level that may have affected GDP per capita. The term V_t controls for internationally shared economic events. *EFW* refers to the level of economic freedom of the world (EFW) index produced by the Fraser Institute (Gwartney et al., 2022) in country i at year t.[17] We expect the coefficient δ_1 to be biased because of the lies of dictators which are inversely correlated with *EFW*. In specifications (2) and (3), the terms V_t and V_i disappear because we use an ordinary least square (OLS) specification to estimate the growth rate between 1992 and 2013. In (2), the economic freedom variable is the average level for that period. In (3), we use the change in economic freedom over the period between 1990 and 2013 (see more below regarding time window for economic freedom). This difference between (2) and (3) is meant to reflect the point in the debates over whether it is the level of economic freedom or the changes in economic freedom that affect growth rates. For both specifications, we include the change in the control variables as our object of interest. We also include the initial income per capita (unadjusted) as a control to capture convergence dynamics.[18] Then, we run the following general specifications:

$$ln(GDPPCAdjusted)_{it} = EFW'_{it}\delta_4 + x'_{it}\lambda + \alpha_3 V_i + \alpha_4 V_t + \epsilon_{it} \tag{4}$$

$$\Delta GDPPCAdjusted_i = EFW'_i\delta_5 + \Delta x'_{it}\theta + \epsilon_i \tag{5}$$

$$\Delta GDPPCAdjusted_i = \Delta EFW'_i\delta_6 + \Delta x'_i\zeta + \epsilon_i \tag{6}$$

which are respectively identical to specifications (1), (2), and (3) with the exception that we employ the adjusted GDP numbers from (Martinez, 2022) as the dependent variable instead. In these specifications, the biases from dictatorships are absent. Using the ratios of δ_4 over δ_1 (for income levels), δ_5 over δ_2 (for income growth with levels of economic freedom), and δ_6 over δ_3 (for income growth with changes in economic freedom) allows us to estimate the bias in the relationship created by the lies of autocrats and dictators. Given the discussion in Section 2, we expect $\frac{\delta_4}{\delta_1}$, $\frac{\delta_5}{\delta_2}$ and $\frac{\delta_6}{\delta_3} > 1$. Our objects of interest are these ratios.

We run a series of variants of our specifications where we include different types of controls to generate the different δ_i. For example, we would generate a first set of estimates based only on bivariate regressions which would provide a first set of ratios. Then we proceed by creating a second set of estimates with a small number of control variables. This generates a second set of ratios. And we continue this process by gradually adding controls (or fixed effects in the case of specifications (1) and (4)). This generates a range of results of the bias present in the literature. We repeat this exercise for the aggregate economic freedom index and its five main components/areas (size of government, legal system, and property rights, sound money, freedom to trade, regulation) as the literature on economic freedom often emphasizes the importance of the different components taken separately (see Bolen and Sobel (2020) for a relevant discussion).

We use two sets of covariates which we label as "Limited Controls" and "Full Controls". The latter set includes education, urbanization, exports, and gross capital formation whereas the former includes only education and urbanization. We use both sets because moving from "limited" to "full" cuts the sample size (from more than 140 countries to less than 100). Most of these covariates are from the World Bank's WDI and were already included in the (Martinez, 2022) dataset. We added a control for the percentage of the population that resides in an urban area for every country in our sample from the year 1992 to 2013. The data comes courtesy of the United Nations' Population Division's *World Urbanization Prospects: The 2014 Revision*, which measures the percentage of a country's population which resides in urban areas at the middle point of a given year.[19] The report has data starting in 1950 and has projected urban growth figures for the years 2015–2050. For the purposes of our study, we truncated data to the years 1992 to 2013. Finally, for specifications (1) and (4), we also attempt an additional variation where the lagged values of GDP per capita are included.

Before we proceed, we should also highlight four steps that we take to assess robustness. First, we replicate our results with different time windows. This is due to the fact that the economic freedom estimates from the Fraser Institute are available every five years pre-2000 (the estimates are annual thereafter). As such, in the specifications used above we had to interpolate between 1990 and 1995 and 1995 to 2000. This could affect the results. As such, we try with two additional time windows (1995 to 2013, 2000 to 2013) as opposed to our original window (1992 to 2013).

Second, some might be concerned that luminosity data may have some noteworthy limitations. For example, dictatorial regimes are often found in countries characterized by a large density of natural resources such as oil, natural gas, lithium, phosphate, nitrate, and other precious metals and raw materials. These regimes also offer large subsidies (both to consumers and producers) that may disrupt the quality of the relationship between luminosity and regime type as estimated by Martinez (2022). Fortunately, there is a solution provided by Martinez. His strategy consisted in exploiting the interaction term between nightlight intensity and oil revenue in explaining GDP (alongside the interaction term between nighttime light intensity and regime type). If the above-mentioned concerns regarding the role of natural resources are correct, that interaction term should not be the same for all countries

[17] In the appendix, we replicate our results with the Heritage Foundation index. We will also use the different components separately of the economic freedom index. This is standard in the literature but it also allows us to implicitly circumvent the concerns of scholars such as (Ott, 2018) who argue that economic freedom is a better measure once the size of government is excluded (however, that opinion has been questioned empirically by Murphy (2022).

[18] For specifications (5) and (6), we use the adjusted GDP income figures. We also repeated the exercise in specifications (1) and (4) with the lagged values of income as a control.

[19] We also used the WDI measure for urbanization from the (Martinez, 2022) dataset and the results were nearly identical.

(i.e., there are potentially heterogeneous elasticities). To deal with this issue, Martinez produced a detailed appendix where he added an interaction term between oil and nightlight intensity. With this effect accounted for, he found even larger biases than with his baseline results. We will use that second set of corrections to replicate our own results from specifications (4), (5), and (6).[20]

Third, there might be the usual concerns for endogeneity. For our purposes, this is not important as we are only trying to state that the relationships are downward biased. If the results from our simple strategies that correct for the flaws in GDP show stronger effects of economic freedom, it stands to reason that stronger strategies that attempt to deal with causality will also be improved (see more below). However, to be on the safe side, we attempt a replication of the approach used by Faria and Montesinos (2009) by incorporating an instrument for legal origins and ethnic fractionalization. We also tried the measure of state capacity produced by Chong et al. (2014). That measure is based on the length of time needed to return letters sent to non-existent business addresses in 159 countries. This, they argued, formed a reliable indicator of government efficiency across countries. Under the logic advanced by some in the economic freedom literature, state capacity (which we can take as synonymous with quality of government) is necessary to have economic freedom (Murphy, 2019a,b, 2023). Ergo, it too can act as an instrument. Because these three instruments are time-invariant, we can unfortunately only use them for specifications (2), (3), (5), and (6) or an OLS formulation of (1) and (4) (i.e., using a single year). We will also replicate our results with the Heritage Foundation Index of Economic Freedom as well.

The first two robustness checks will be included in the paper. The latter two robustness checks will be mentioned but the full results are relegated to the appendix to keep the article brief as they do not alter the pattern of results.[21]

Two final points of clarification are necessary. First, we prefer to leave questions of causality to future research. This is why our language emphasizes "associations" rather than causation. However, if our hypothesis is correct, all causal inference results will be updated in the same direction as our results.[22] Take for example the causal inference design of Grier and Grier (2021). They used a one-point jump in economic freedom as a treatment for "big liberalization" in any five-year window from 1970 to 2015. Then, they matched (using propensity scores and Mahalanobis distances) those big liberalizers against non-liberalizers and assessed outcomes on economic growth in the following five years. They found that there was a 16% increase in income – a causal claim. However, if our hypothesis is correct, then the 16% they find understates improvement since the start-point overstates income relative to the end point (i.e., there are larger lies in the pre-liberalization period and smaller ones in the post-liberalization period). Other causal inference works such as Pavlik et al. (2022), Adisa et al. (2023), Feldmann (2017), and Callais and Young (2021) (to name only a few) will be automatically affected as well.[23]

Second, we want to test whether Martinez's estimates have conceptual validity. In the logic he lays out, democracies do not lie in meaningful manners or degrees about their national accounts. As societies become less democratic, so the argument goes, the lies grow larger. This is how Martinez explains his finding that the degree of overestimation of income levels and economic growth increased with the degree authoritarianism. In other words, the function of the degree of lies is increasing the degree of authoritarianism. Can this mechanism be observed by other means than using NTL? A positive answer to this question authorizes the use of Martinez's estimates as a substitute to the reported GDP estimates.

To answer that question, we took specification (1) as our starting point but we excluded the variable for economic freedom. We also restricted the sample to only free countries. Because no governmental deceit of importance is expected within this group, the specified model can be assumed to yield unbiased estimates. That model is then used to predict the GDP per capita of unfree countries. The predicted values are those we should expect – holding all else equal – absent the lies of dictators. From there, we created two ratios. The first is the ratio for any country i at time t of Martinez's adjusted GDP per capita estimates to the estimates of GDP obtained from the fitted values ($\frac{GDPPCAdjusted_{i,t}}{GDPPCFitted_{i,t}}$). The second is the ratio for any country i at time t of the reported GDP estimates to the estimates of GDP obtained from the fitted values ($\frac{GDPPCReported_{i,t}}{GDPPCFitted_{i,t}}$). The level of these ratios is not of interest. What matters – to test if we can see Martinez's proposed mechanism by other means – is whether they remain stable or change as we move along the interval of index values for any measure of democratic institutions. The first ratio, ($\frac{GDPPCAdjusted_{i,t}}{GDPPCFitted_{i,t}}$), should be stable at all levels of democratic life because we are stripping out the lies of dictators. The second ratio, $\frac{GDPPCReported_{i,t}}{GDPPCFitted_{i,t}}$), should

[20] Martinez (2022) made a series of attempts to assess the robustness of his baseline finding. This included adding multiple variations of the above-mentioned strategy of interacting oil revenue and nighttime light intensity. This meant swapping oil revenue for variables such as private consumption, investment, government spending, exports, imports (all those separately or combined), agriculture as a share of area, agriculture as a share of output, natural resources extraction as a share of output, industry as a share of output, manufacturing as a share of output, services as a share of output, area, top-coding of light cells, and inequality in nighttime light intensity. The resulting corrections were generally above the baseline correction factor that we used. The correction based on oil revenue yielded the largest correction factor. As such, our use of it amounts to presenting the upper end of potential biases induced by the lies of dictators. Our baseline specification is closer to the lower end (i.e., our main results produce a conservative assessment). Most importantly, his entire appendix C concerns modifications made to the baseline specifications to see if the choice of specification altered the extent of GDP exaggerations. He notably shifted to a specification that allowed to test whether GDP was overstated in both a proportional and additive manner. He found no differences in his results. Essentially, all the alternatives specifications he made yield the same pattern of results.

[21] We also attempted sample restrictions such as excluding China and the top 5 oil-producing countries. Our pattern of results is similar.

[22] Incidentally, it would also affect symmetrically other concerns raised towards the economic freedom index. For example, Balliew et al. (2020) argue that shifting for geometric mean (rather than arithmetic mean) of the components of the index changes the results slightly. While we do not wish to take a stand on this, our results suggest that whatever they find is going to be downward biased because of the lies of dictators). As such, our results speak to other peripheral questions in the economic freedom literature (another example is the relationship between size of government and economic freedom – see Ott (2018) and Murphy (2022).

[23] It is worth pointing out that Martinez (2022) (in his appendix, p. A18) finds that the elasticity of nighttime light intensity to GDP changes when countries liberalize politically and when they become dictatorships. The difference-in-difference approach that he uses essentially confirms that lies collapse with liberalization. This will affect any causal inference that rely on GDP.

Table 1
Descriptive statistics.

Name	Mean	S.D.	Min	Max	Observations
Education (Years)	7.56	3.20	0.40	14.20	4746
Urbanization (Proportion)	56.00	24.22	6.29	100.00	5945
Export to GDP ratio	38.27	22.19	0.18	181.78	3739
Gross capital formation (Invest in GDP)	0.23	0.08	−0.02	0.79	3582
Unadjusted GDP per capita (Constant 2005 USD) (non-logged)	10 439.92	17 384.68	50.04	158 802.50	4017
Adjusted GDP per capita (Constant 2005 USD) (non-logged)	9119.13	16 273.09	32.55	151 408.20	3888
Adjusted GDP per capita (Constant 2005 USD, oil revenues adjusted) (non-logged)	9016.29	16 210.72	30.21	149 855.9	3888
Growth in GDP/Capita in USD	0.6065947	0.6631623	−0.4536273	5.424674	4756
Adjusted growth in GDP/capita in USD	0.6133372	0.6627561	−0.430271	5.290526	3498
Adjusted growth (with oil revenues adjustment)	0.6309145	0.6896484	−0.4480905	5.56546	3498
Economic freedom of the world	6.553639	1.132938	2.721802	8.849427	3909
Area 1 (Size of government)	6.606921	1.198673	1.573963	9.577378	3901
Area 2 (Legal system and property rights)	5.213474	1.666195	1.804654	8.997781	3909
Area 3 (Sound money)	7.617283	1.955586	.0099824	9.923805	3909
Area 4 (Freedom to trade internationally)	6.790084	1.59871	.2255733	10	3808
Area 5 (Regulation)	6.616368	1.231589	1.169706	9.277325	3909
Δ Economic freedom of the world	1.200838	1.101532	−2.453129	4.18438	3277
Δ Area 1 (Size of government)	0.7103232	1.479845	−2.623227	4.640112	3248
Δ Area 2 (Legal system and property rights)	0.4400869	0.7449644	−2.452459	2.552577	3277
Δ Area 3 (Sound money)	1.995532	2.41297	−1.926487	8.61133	3277
Δ Area 4 (Freedom to trade internationally)	1.53161	2.027962	−3.897091	5.804699	2900
Δ Area 5 (Regulation)	1.222774	1.233435	−1.789991	5.54926	3277

change as the numerator contains the lies of dictators. A regression using the first ratio as the dependent variable should show that variables for democracy have no significant association. The second ratio should show a significant association. At the very least, the coefficient on the second ratio should be larger than the first. If this is the case, there is validity to the underlying mechanism that Martinez believed existed to generate his adjustments.

We conducted this exercise using the two most time-continuous variables of democratic institutions: the Freedom in the World (FIW) index and the Polity V score for democracy. The FIW score operates inversely, where higher values signify less democratic societies, whereas the Polity V score operates directly, with higher values indicating more democratic societies.[24] The regression is conducted *only* on unfree countries. The data used are the same as the one described in Table 1.[25] The results are depicted in Table 2. The left column illustrates the association between either FIW or Polity V and the ratio of Martinez's GDP estimates to the fitted GDP estimates. There are no significant associations. A significant relationship emerges when examining the right-side column. Here, the reported GDP estimates progressively increase relative to the fitted values as the level of democracy decreases. Given that FIW operates inversely to democracy, it follows logically that the ratio should ascend as democracy declines. Conversely, for Polity V, which operates directly in measuring democracy, the anticipated effect is the opposite; an increase in Polity V indicates a move towards greater democracy. Hence, we anticipate a negative coefficient with that indicator in order to be consistent with Martinez's logic. Both coefficients exhibit the expected signs and are statistically significant, offering a degree of conceptual validation for Martinez's estimates.

While this gives us a sound basis for justifying the use of the Martinez estimates, it does not mean that they are the perfect adjustments. Better adjustments could be created in the future, thus improving the quality of all our findings. However, we are comfortable with saying that our statements will be directionally accurate.

4. Results

4.1. Levels of economic freedom and income levels

The specifications used above for income levels imply that we ran 10 regressions for the EFW index as a whole and an extra 10 for each component of the index (for a total of 60 regressions). Across all specifications, the effects of economic freedom and its components taken separately were positive and significant (always above the 5% level). This is consistent with the empirical literature on economic freedom and income levels (Hall and Lawson, 2014; Lawson, 2022). However, the coefficients when using the adjusted GDP numbers were always larger and more frequently exceeded the 1% significance threshold. In Section 4.4, we provide a concise discussion of these changes in significance.

[24] In the FIW regression analysis, we developed a model for all countries with a score of 2 or below. This is the group that Martinez (2022) defined as politically free. For the Polity V regression, we estimated the model for all countries with a score greater than or equal to 8. We should note that we use both FIW and Polity V for greater robustness (rather than just using FIW to benchmark ourselves fully on Martinez's work).

[25] However, as we add controls provided in the original dataset of Martinez (2022) such as agricultural land and oil revenues as share of GDP the results we describe below get stronger. This makes us highly confident in the results obtained in Table 2. We should also note that adding or excluding year-fixed effects has no effect on the results we obtained.

Table 2
Regression of different ratios of GDP estimates to democratic indicators.

	Levels of income (if FIW > 2)	
	Martinez estimates/Fitted values	Reported estimates/Fitted values
Freedom in the World Index	0.003 (0.005)	0.020** (0.009)
	Polity V democracy score (if Score < 8)	
	Martinez estimates/Fitted values	Reported estimates/Fitted values
Polity democracy score	−0.011** (0.005)	−0.017** (0.007)

Notes: *** p < 0.01, ** p < 0.05, * p < 0.1.

In Table 3, we present the ratios of the coefficients of economic freedom that resulted from specifications (1) and (4). In an online appendix, we make available the full table of results rather than this summarized set. The coefficients estimated using adjusted GDP per capita figures are the numerator whereas the coefficients obtained using unadjusted GDP per capita are in the denominator. Table 3 has three panels. The top panel shows the effects of EFW between 1992 and 2013 (i.e., with interpolation for EFW from 1990 to 1995 and 1995 to 2000). In the absence of controls and fixed effects, the true effect of economic freedom appears to be stronger by at least 10%. However, once fixed effects are included, that proportion grows. However, once fixed effects are included, the proportion remains stable even if we gradually increase the number of controls included.[26] Overall, the use of adjusted GDP figures suggests that the effect of economic freedom (i.e., the aggregate index) is 27% larger than estimated with unadjusted GDP figures. Adding on the lagged values of the GDP per capita figures confirms (and strengthens) this pattern of results (see the last line of the top panel where the difference increases to 51%). For the different components of the index, the use of adjusted GDP figures has an uneven effect. For example, regulation and freedom to trade suggest that the true effects are roughly 20% larger than when using the unadjusted GDP figures. In contrast, the true effects for the component that speaks to the protection of property rights are more than 33% to 39% stronger. These are economically significant results that speak to a large bias against finding a pro-development effect of economic freedom.

The second panel of Table 3 shows the effects of cutting the window of observations to years where there is no need to interpolate values of EFW. The results are similar to those in the top panel although the range seems to have shifted more noticeably for some components (size of government and open trade). When the lagged values are used with 2000 to 2013 values only, the results are somewhat similar but there are some jumps that need to be mentioned. For area 1 (size of government in the last row), the ratio is −0.29. This is because the coefficient changes sign from negative to positive. However, in both cases, the effect is not statistically significant. For area 3 (sound money), the jump appears gigantic (3.63) but this is because both are very small numbers near zero. Neither of these are significant. In the bottom panel of Table 3, we show what happens if we use a different set of correction factors – which account for the differential effect of oil production – from (Martinez, 2022) in order to create adjusted GDP per capita. When we use this set instead, the results are noticeably stronger across the board. The adjusted GDP figures suggest that the effect of economic freedom (as a whole) is 28% to 29% greater (instead of between 23% and 25% in the top panel). These stronger effects are also noticeable in some of the components such as property rights (area 2) and sound money (area 3). When lags are added, the effects also get larger.

In the supplementary materials to this article, we replicated our results with the Heritage Foundation's index and found that the coefficients for economic freedom are 9% to 10% larger with the adjusted GDP figures. We also attempted an IV specification using the average time to return letters mailed to false addresses, legal origins, and ethnic fractionalization as instruments. Obviously, this means abandoning the panel approach in favor of an OLS strategy. Setting 2013 as our year of choice, we found that the coefficients for economic freedom were 6% to 29% larger with the adjusted GDP estimates after instrumenting.

4.2. Levels of economic freedom and income growth

We now turn our attention to specifications (2) and (5) where the relationship between the change (i.e., economic growth) in income and the level of economic freedom is our object of interest. The results are depicted in Table 4 below. Just as in the case of Table 3, we present three different variations on the specifications used. The only difference is that the lagged values of income are excluded as specifications (2) and (5) are OLS regressions. Instead, income per capita at the beginning of the period is used as a control.

In all three panels of Table 4, the ratio of coefficients for economic freedom are smaller and closer to unity.[27] For some areas, such as the size of government (area 1), the use of the adjusted data actually reduces the coefficient but the effect is not significant.

[26] The coefficients get smaller with controls but the gap between adjusted and unadjusted remains proportionally stable.

[27] Nearly all of the ratios that are below a value of 1 are in instances when the economic freedom is negative with both the adjusted and unadjusted data. However, the coefficient with the adjusted data is closer to zero than with the unadjusted data. In those cases, being below 1 is actually stating that there was an improvement in the effects of economic freedom. However, these coefficients were also generally insignificant which is why we report this here in footnote.

Table 3
Ratio of the effects of economic freedom on adjusted and unadjusted log of income per capita across multiple variants of specifications (1) and (4).

	Panel (A) With EFW interpolations					
	EFW	Area 1	Area 2	Area 3	Area 4	Area 5
No controls	1.13	1.13	1.15	1.13	1.13	1.1
Country-fixed effects	1.12	1.13	1.16	1.13	1.13	1.11
All fixed effects	1.25	1.23	1.33	1.29	1.21	1.19
Limited controls + All fixed effects	1.24	1.24	1.36	1.28	1.2	1.19
Full controls + All fixed effects	1.23	1.23	1.33	1.28	1.17	1.19
Full controls + All fixed effects + Lags	1.51	1.09	1.39	1.90	1.34	1.25
	Panel (B) Using 2000 to 2013 only					
No controls	1.10	1.07	1.11	1.10	1.15	1.07
Country-fixed effects	1.10	1.07	1.11	1.10	1.16	1.07
All fixed effects	1.21	0.99	1.29	1.25	1.24	1.15
Limited controls + All fixed effects	1.23	1.01	1.31	1.27	1.28	1.16
All controls + All fixed effects	1.27	1.36	1.29	1.32	1.30	1.16
All controls + All Fixed Effects + Lags	1.24	−0.29	1.46	3.63	1.34	1.02
	Panel (C) Using GDP corrections with effects of oil revenues					
No controls	1.15	1.15	1.18	1.16	1.15	1.12
Country-fixed effects	1.15	1.15	1.18	1.16	1.15	1.13
All fixed effects	1.29	1.27	1.38	1.34	1.25	1.22
Limited controls + All fixed effects	1.28	1.29	1.42	1.33	1.23	1.23
Full controls + All fixed effects	1.28	1.27	1.39	1.33	1.20	1.22
All controls + All fixed effects + Lags	1.62	1.14	1.50	2.09	1.41	1.32

Notes: Area 1 is size of government; Area 2 is Legal System and Property Rights; Area 3 is Sound Money; Area 4 is Freedom to Trade Internationally; Area 5 is Regulation. Panel (C) uses all years from 1992 to 2013. We replicated our results from that panel from 2000 to 2013 only. The results were highly similar. These are available on demand.

However, the overall pattern suggests that the use of adjusted GDP data has a more modest effect than in Table 3.[28] Even though there are some differences, this militates in favor of stating that poor data quality is a contributor to the debate over whether higher levels of economic freedom are associated with faster growth regimes. However, because the coefficients were generally not significant and also because the ratios are relatively small, we believe that it is a minor contributor.

This result is unsurprising. The lies of dictators bias the estimated effect of the level of economic freedom on growth if and only the magnitude of the lie changes over time. If a dictator consistently overstates income by 20%, the growth rate between two false estimates of GDP is going to be the same as the growth rate between the two more accurate estimates of GDP. As such, the finding of smaller biases with respect to economic growth appears reasonable.

We also replicated our results with instrumental variables for legal origins, the average time to return letters mailed to false addresses, and ethnic fractionalization. The pattern is the same as in the three panels of Table 4. However, for brevity, we placed those results in our supplementary materials.[29]

4.3. Changes in economic freedom and income growth

We now turn to the question of whether shifting from adjusted to unadjusted GDP affects conclusions regarding how changes in economic freedom affect economic growth. As explained above, this not only replicates the strategy of De Haan and Sturm (2000) but also allows us to assess whether regimes that liberalize economically and politically stop lying about GDP in ways that mute the benefits of liberalization. If pre-liberalization estimates of GDP are upwardly biased whereas the post-liberalization of GDP are closer to the truth, growth rates will appear slower than they truly are. As such, the estimated benefits of liberalization (i.e., large changes in economic freedom) could be biased when unadjusted GDP figures are used. The results are presented in Table 5 in the same format as for Tables 3 and 4.

The top panel of Table 5 suggests a very similar pattern as in the top panel of Table 4 – that the flawed GDP numbers do not materially affect results. However, unlike in Table 4, restricting the period to 2000 to 2013 (when the economic freedom data are continuous) causes a large jump in the coefficients using the adjusted GDP figures. The use of adjusted GDP figures suggests that the

[28] We also replicated this regression but we swapped the average level of economic freedom (and components) for the initial level. The story is similar – shifting to adjusted estimates does not unveil any consistent pattern in how the relationship between economic freedom levels and growth is affected by the lies of dictators. Results available on demand.

[29] Also, it is worth pointing out that the instrumental variable approach here is not particularly informative. While it makes sense to believe that there is endogeneity between the level of income and the level of economic freedom (Acemoglu et al., 2001; Albouy, 2012), it is harder to see why there would be endogeneity between economic growth from 1992 to 2013 and the level of economic freedom. This issue carries over in the next subsection where it is even harder to see why there would be endogeneity between the change in income and the change in economic freedom. We nevertheless implemented these additional tests to create a validity check on our main specifications.

Table 4
Ratio of the effects of levels of economic freedom on growth of adjusted and unadjusted income per capita across multiple variants of specifications (2) and (5).

	Panel (A) With EFW interpolations					
	EFW	Area 1	Area 2	Area 3	Area 4	Area 5
No controls	1.08	1.11	1.06	0.99	2.35	1.01
Limited controls	1.09	1.07	1.13	1.02	1.12	1.07
Full controls	1.16	1.49	1.19	1.05	1.19	1.13
	Panel (B) Using 2000 to 2013 only					
	EFW	Area 1	Area 2	Area 3	Area 4	Area 5
No controls	0.89	0.98	0.87	0.94	0.89	0.89
Limited controls	1.16	0.68	1.16	1.10	1.14	1.10
Full controls	1.21	0.84	1.16	1.25	1.15	1.11
	Panel (C) Using GDP corrections with natural resources effects					
	EFW	Area 1	Area 2	Area 3	Area 4	Area 5
No controls	1.00	1.13	1.03	0.90	1.27	0.79
Limited controls	1.13	1.02	1.19	1.06	1.17	1.08
Full controls	1.20	1.23	1.25	1.09	1.24	1.13

Notes: Area 1 is size of government; Area 2 is Legal System and Property Rights; Area 3 is Sound Money; Area 4 is Freedom to Trade Internationally; Area 5 is Regulation. Panel (C) uses all years from 1992 to 2013. We replicated our results from that panel from 2000 to 2013 only. The results were highly similar. These results are available on demand.

Table 5
Ratio of the effects of changes in economic freedom on growth of adjusted and unadjusted income per capita across multiple variants of specifications (3) and (6).

	Panel (A) With EFW interpolations					
	EFW	Area 1	Area 2	Area 3	Area 4	Area 5
No controls	1.03	1.03	1.13	1.08	0.99	1.03
Limited controls	1.01	0.98	0.52	1.07	0.94	1.02
Full controls	0.98	0.94	−0.85	1.01	0.93	0.98
	Panel (B) Using 2000 to 2013 only					
	EFW	Area 1	Area 2	Area 3	Area 4	Area 5
No controls	1.19	1.00	1.23	1.23	1.21	1.18
Limited controls	1.33	1.12	1.35	1.32	1.68	1.22
Full controls	1.31	1.29	1.30	1.31	1.67	1.17
	Panel (C) Using GDP corrections with natural resources effects					
	EFW	Area 1	Area 2	Area 3	Area 4	Area 5
No controls	1.07	1.07	1.20	1.11	1.03	1.08
Limited controls	1.03	0.99	0.58	1.08	0.96	1.07
Full controls	1.00	0.93	−0.65	1.01	0.95	1.03

Notes: Area 1 is size of government; Area 2 is Legal System and Property Rights; Area 3 is Sound Money; Area 4 is Freedom to Trade Internationally; Area 5 is Regulation. Panel (C) uses all years from 1992 to 2013. We replicated our results from that panel from 2000 to 2013 only. The results were highly similar to those in Panel B. These results are available on demand. For Panel A, one could ask why there are interpolations since we are using changes. This is because the GDP data is for 1992 whereas as the nearest estimates of economic freedom are for 1990.

effect of economic freedom (i.e., the aggregate index) is 19% to 44% greater with the adjusted GDP figures. This is also true for the different components/areas of the index. In one case (the freedom to trade internationally in Area 4), the coefficient changes sign from negative to positive and it is substantially larger. The bottom panel of Table 5 – which uses the entire period from 1992 to 2013 – shows that there are few differences in changing to adjustments that incorporate the effect of oil production. Interestingly, the coefficient for area 2 (property rights) is negative in the unadjusted series and positive in the latter (hence the negative sign).[30]

Overall, these results point in the direction that there is some underestimation of the effects of changes in economic freedom on economic growth.

4.4. Are the differences significant?

The above results suggest that there are some aspects of the relationship between economic freedom and economic development that are being missed as a result of the one-sided measurement errors created by the lies of dictators regarding GDP. The next, and

[30] Using the same instruments as in the previous subsection produces the same pattern as described above.

Table 6
Effect of economic freedom and components on the difference between unadjusted and adjusted GDP estimates in specifications with full controls.

	EFW	Area 1	Area 2	Area 3	Area 4	Area 5
Panel (A) Table 3	−0.0185***	−0.00528	−0.0249***	−0.00590***	−0.00554**	−0.0106**
	(0.00495)	(0.00397)	(0.00740)	(0.00181)	(0.00269)	(0.00465)
Panel (B) Table 3	−0.0224***	−0.00349	−0.0225***	−0.00511***	−0.00788***	−0.00989***
	(0.00489)	(0.00370)	(0.00590)	(0.00184)	(0.00248)	(0.00356)
Panel (C) Table 3	−0.0217***	−0.00632	−0.0292***	−0.00695***	−0.00642**	−0.0126**
	(0.00580)	(0.00465)	(0.00869)	(0.00211)	(0.00316)	(0.00546)
Panel (A) Table 4	−0.0118*	−0.00260	−0.0129**	−0.000654	−0.0146**	−0.00277
	(0.00698)	(0.00590)	(0.00521)	(0.00401)	(0.00719)	(0.00552)
Panel (B) Table 4	0.00000683	−0.00164	0.00195	0.000483	−0.000854	−0.000125
	(0.00298)	(0.00250)	(0.00173)	(0.00158)	(0.00197)	(0.00254)
Panel (C) Table 4	−0.0161*	0.000538	−0.0202***	−0.00274	−0.0194**	−0.00225
	(0.00852)	(0.00669)	(0.00635)	(0.00467)	(0.00876)	(0.00619)
Panel (A) Table 5	−0.00183	0.00120	−0.0162**	−0.00118	0.00282	−0.000194
	(0.00536)	(0.00401)	(0.00646)	(0.00265)	(0.00384)	(0.00541)
Panel (B) Table 5	−0.0366**	−0.0082	−0.0316**	−0.0108**	−0.0196**	−0.0170*
	(0.0133)	(0.0117)	(0.0146)	(0.00534)	(0.00767)	(0.00909)
Panel (C) Table 5	−0.00488	0.000985	−0.0148**	−0.00143	−0.000438	−0.00636
	(0.00670)	(0.00488)	(0.00736)	(0.00329)	(0.00461)	(0.00639)

Notes: *** p < 0.01, ** p < 0.05, * p < 0.1.

final step, is to determine whether the differences we obtained in Tables 3 through 5 are significant. To answer this question, we computed the difference between the unadjusted GDP figures (growth and levels) and the adjusted ones. We then regressed that difference on our independent variables to see if there is a significant effect of economic freedom and its components. We should observe that the difference between the GDP figures should be inversely associated with economic freedom (and its components). In Table 6 below, we produce the results. Each row of that table refers to a different panel with "full controls" of Tables 3, 4, and 5.

As can be seen, with the notable exception of Area 1 (size of government), an extra point on the economic freedom index has a negative and statistically significant effect on the difference between estimates of GDP level. For the difference in growth rates and the level of economic freedom, the absence of notable differences in estimates showcased in Table 4 appears to be confirmed in Table 6. The index as a whole and all the components – except maybe Area 2 (property rights) – appear to have no effect on the difference in growth rates. Finally, there are some signs that the relationship between the difference in growth rates is significantly affected by the change in economic freedom when we restrain the time window to the period between 2000 and 2013. This suggests that it is correct to point out that the benefits of liberalization are probably being understated by the GDP lies of dictators. However, we should also point that *regardless* of the specification used, property rights (i.e., area 2) is nearly always significant suggesting that the lies of dictators do affect our understanding of the importance of economic freedom in that dimension (both to growth and to levels of income).

5. Discussion and conclusion

In this short article, we argued that the well-documented proclivity of dictators to fudge GDP numbers biases our estimations of the effects of economic freedom on economic development. Since dictatorships are generally also countries with low economic freedom, overstated GDP numbers can fool us into finding weaker associations between economic development and economic freedom.

To test our argument, we employed newly generated adjustments to GDP numbers based on artificial nighttime light intensity that corrected for the overstatements that dictators made (Martinez, 2022). Swapping unadjusted and adjusted GDP numbers as dependent variables in similar econometric setups allowed us to estimate just how large the bias is. For income levels between 1992 and 2013, we find that the true effect of economic freedom is between 1.1 and 1.62 times larger than estimations based on manipulated GDP numbers. For some components of the index, the effects are larger still. We find little to no sign that the lies of dictators affect the association between levels of economic freedom and income growth. However, we do find signs that the association between income growth and changes in economic freedom is being modestly understated.

Our results are consistent with findings that dictatorships are generally unable to sustain high levels of economic development and that they are not noticeably better at securing faster economic growth (Gandhi, 2008; Klomp and De Haan, 2009; Carden and James, 2013; Papaioannou and Van Zanden, 2015; Knutsen, 2015; Alexandre et al., 2022). We believe that these results should invite economists to revisit some of their findings tying economic development to economic freedom (especially if the variable of interest is the level of income rather than growth rates). This applies as much to papers that attempt to establish propositions for the mechanisms tying economic freedom to development[31] as to papers that try to more directly find causal effects. For example, Grier

[31] See also examples from Bjørnskov and Foss (2008) and Sobel et al. (2007) for a mechanism tying economic freedom to development through entrepreneurship. See Gwartney et al. (2006), Hall et al. (2010) and Bengoa and Sanchez-Robles (2003) who draw a connection from economic freedom to development through capital accumulation and the productivity of investments. Also worth revisiting would be the relationship between economic freedom, culture and growth as studied by Williamson and Mathers (2011).

and Grier (2021) used propensity score matching and two-way fixed effects to analyze whether countries that experienced a 1-point in economic freedom enjoyed faster economic growth than countries that did not liberalize. They found these "liberalizers" were 16% richer than they would have been 10 years after the onset of reform. However, as the results from Section 4.3 suggest, countries that liberalize also tend to have underestimated economic growth. As such, the proportion of 16% found by Grier and Grier (2021) could be a conservative estimate. Other such causal papers could be revisited to see whether our findings of the underappreciated importance of economic freedom have broader ramifications. Overall, we see our results as an invitation to revisit certain findings and assess their robustness by swapping between unadjusted and adjusted GDP numbers to make the results robust to the lies that dictators spew to shore up their legitimacy.

CRediT authorship contribution statement

Sean P. Alvarez: Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Vincent Geloso:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Macy Scheck:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

None of the authors have a competing interest to declare. We did not use any AI generating tool to produce our article.

Data availability

Replication codes are available as supplementary material.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.ejpoleco.2024.102577.

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