The Political Economy of Data Production

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Abstract: Macroeconomic statistics are nearly ubiquitous in comparative politics and international relations research. These data are inevitably measured with error, but our use of them implies a belief that those errors are random, or at least unrelated to the political phenomena we use them to understand. However, recent work indicates that data production is politically informed, suggesting the need to better understand the political economy of data production and its consequences. We contribute to that goal by considering the heretofore-neglected impact of international institutions. We theorize that IMF programs incentivize countries to produce more accurate GDP statistics, which we test using a dataset of *ex post* revisions to the World Development Indicators' GDP growth statistics. We show that the revisions data supports our main theoretical contention, *and* that the resulting non-randomness in measurement error affects the apparent empirical relationship between the IMF and economic growth.

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Empirical work in the study of comparative and international politics often employs cross-national datasets of hard-to-measure concepts, such as democracy, corruption, state violence, or the ease of doing business. Measuring the concepts is difficult, not least of which for the various ways that politics can color what should be (but not always is) an apolitical measurement exercise.[3] To political science's credit, debates about measurement and about the politics of measurement are commonplace and influential.[4] Those debates improve how we use data, as well as those data's accuracy, construct validity, and cross-national comparability.

Those debates usually focus on overtly "political" variables. We are in general less reflective about cross-national macroeconomic data, and less scrutinizing of politics' role in the process that generates them. We shouldn't be. These data are often just as central to our empirical work, and they too represent hard to measure concepts and are the product of imperfect measurement processes. Dramatic revisions to African GDP estimates—62% in 2010 in Ghana, 89% in 2014 in Nigeria—gave the imperfections of this process a recent star turn in the mass media,[5] but these numbers' frailty has long been known in academic and policy circles.[6] So much so that a senior official at the World Bank characterized African economic data as a "Statistical Tragedy" (Devarjan 2011).

Why do political scientists tolerate "tragic" imperfections in macroeconomic data that we do not tolerate in more overtly political data? While the proximate cause is likely that we rarely think about it, our inattention suggests an assumption that macroeconomic data's inaccuracies lack political correlates and are therefore random

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[3] In recent news see https://qz.com/1179239/world-bank-doing-business-ranking-chief-economist-paulå-romer-apologizes-for-unfair-results/

[4] See, for example, Goertz 2006, Munck 2009, Vreeland 2008, Cheibub, Gandhi, and Vreeland 2010; Pemstein et al 2010; Schedler 2012a, b.

[5] See The Economist, "Step Change" Apr 12th 2014 available at <<http://www.economist.com/news/finance-and-economics/21600734-revised-figures-show-nigeria-africas-largest-economy-step-change>> See Palmer 2018 for a recent discussion. <http://foreignpolicy.com/2018/03/21/nobody-knows-anything-about-china/>>

[6] See, for example, Yeats, 1990; Yeats and Rozanski 1994; Jerven 2013, 2015; Ravallion 2016; Keilman 1998; Morgenstern 1950; Herrera and Kapur 2010; Deaton 2010; Srinivasan 1994; Milanovic 1999.

with respect to the political concepts we are traditionally concerned with, and do not hinder the goal of understanding those concepts better. That assumption does not hold up to scrutiny. Politics affects how economies are measured as surely as it affects how they function. We know, for instance, that governments occasionally "juke the stats," lying about the economy in order to hit politically useful targets.[7] Recent work by Magee and Doces (2015), Martinez (2018) and Hollyer, Vreeland and Rosendorff (2011) suggest that politics (specifically the distinction between democracy and autocracy) shapes the production of economic data more broadly.[8] The problem for empirical political science is therefore not that data production *can* become politicized, but that it is intrinsically so. This is in some unsurprising—measuring the economy is largely a state function and inherently shaped by the state's political-economic milieu—but we know relatively little about that process. And what we do know largely relates to whether or not a country's domestic politics, which may not fully represent the politics behind the data production process. Better understanding the scope and consequences of the political economy of data production is a laudable scientific goal in its own right, and an important tool for improving political economy research more broadly.

This paper expands our knowledge of the political economy of data production, empirically and theoretically. Our contribution to theory is to extend the literature's focus on distinctions between democracies and autocracies by considering the role of international institutions. We argue that a country's relationship with the IMF shapes the political environment in which it collects and disseminates economic data. IMF lending programs incentivize governments to credibly demonstrate progress towards macroeconomic benchmarks (to the IMF, to financial markets, to its own population), which, in turn, incentivizes them to invest in the production of more accurate data.[9] That suggests that data from country-years with active IMF programs should be more accurate, and therefore more stable in the face of subsequent revisions. To the extent that investments in more accurate data reduce politically informed uses of discretion (see,

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[7] For example, see: Wallace 2014; Jerven 2013; Kerner, Jerven and Beatty 2017; Alt, Lassen and Wehner 2014. "Juking the stats" refers to Wallace 2014.

[8] See also Herrera and Kapur 2010

[9] We thank the IMF's statistical department for suggesting that a main driver of data quality is the political will to establish a government's developmental *bona fides*.

among others, Jerven 2013), it suggests also that estimates from countries *without* IMF programs should be systematically more optimistic. More broadly put, we suggest that the political economy of data production has a substantial international component.

Our empirical contribution is to examine these issues through *ex post* revisions to the World Development Indicators' (WDI) GDP growth data.[10] These revisions offer a unique window into the politics of data production, but to our knowledge have not previously been used for this purpose. Revisions occur when the official account of an economy changes as a consequence of new information, such as new data or new models used to process data into statistical aggregates like GDP. In the face of that new information the historical record is adjusted and old estimates are replaced with new and presumably more accurate ones. That process generates "vintages" of data with each vintage representing the official account of a single data point (say, US GDP in 2004) at various points in history.

The vintaging process in WDI data is routine and mostly inconsequential. For example, between 2007 and 2008 the WDI's estimate of Algeria's 2005 GDP per capita moved from $2,121.45 to $2,121.42. Most revisions are similarly small and practically meaningless from a political science perspective. Some are more notable. Bangladesh's 2005 GDP per capita estimate fell from $432.63 to $400.28 between 2007 and 2008. A 7% reduction in Bangladeshi income in one year would be tremendous political-economic news had it happened in real life. But it occurred "only" in the historical record and for that reason seems to have escaped notice. Noticed or not, changes to the historical record on that scale affect knowledge generation. Empirical tests of theories on the South Asian political economy might come to a different conclusion if conducted in 2007 than they would if conducted in 2008. And to this paper's point: if there is something about Bangladeshi politics that made it likely that their but not Algerian GDPs would be initially overestimated, it suggests that our beliefs about politics' effects on economics might be systematically warped by its independent effects on economic measurement.

[10] The raw data used in this paper are available at http://databank.worldbank.org/data/reports.aspx?source=WDI-Archives. To facilitate researcher use of this data, we make it available in an R package, *revisions*. It is available at <redacted> and contains long- and wide-format data sets.

We use revisions to assess our theory about the political economy of data production and its practical implications. We do so in a number of ways. First, we estimate the predictors of GDP growth revisions using a random forest model, which, as noted in more detail below, assess variables' contributions to a model's predictive power without pre-specifying those relationships' functional forms. The results suggest that a country's relationship with the IMF substantially shapes the revisions process. Closer examination of the data (as well as regression models) indicates that growth estimates from country-years with active IMF programs hold up better to the revision process, and that the more dramatic revisions tend to come from countries without an active IMF program. That is consistent with our theoretical premises, and all the more notable considering the countries with IMF programs are likely to be the hardest to measure properly in the first place. We also find that substantial negative revisions (indicating initially over-optimistic estimates) are more common among estimate made in non-IMF country-years, though these differences are substantially smaller and are limited to the most dramatic revisions.

Second, we ask whether IMF-driven non-randomness in growth revisions generates unstable estimates of IMF-growth relationship across vintages. If revisions were randomly distributed (or if any non-randomness was small enough to be practically inconsequential), estimates of the IMF-growth relationship should remain stable across vintages, and gain precision over time as revisions reduce random measurement. If revisions were substantially non-random and related to the IMF, estimates of the IMF-growth relationship should shift across vintages as the initially non-random measurement error is revised out. We explore the stability of those estimates by estimating and re-estimating the GDP growth-IMF relationship using multiple vintages of the same data. In effect we ask the same question—Did countries with IMF programs grow faster during the first half of the 2000s?—and provide answers as they would have appeared in successive years between 2006 and 2012. We find that the relationship between IMF programs and growth can appear substantially different depending on which vintages of the data is used, which further suggests a revisions process with consequential political roots (or, at least, political correlates).

We also use our data to test existing claims in the literature about democracy's influence on data production. Democracies are thought to invest more in accurate and transparent data, and should be therefore subject to fewer and smaller *ex post* revisions (see, Hollyer, Vreeland and Rosendorf 2011). Democracies are also thought to be less prone to embellishment (see Magee and Doces 2015 and Martinez 2018), and thus less likely to produce growth data that are subject to subsequent downward revisions. Our analysis suggests (varying degrees of) support for these contentions, and similarly suggests that the vintaging process affects estimates of democracy's empirical relationship to economic growth. Interestingly, and somewhat outside of this paper's theoretical purview, the largest effects that we observe in our analysis is that growth estimates from African countries are especially prone to negative revisions.

The particular affects of the IMF (and democracy, and location in Africa) are notable, but the larger and more striking implication of our analysis is that there exists a political economy of data production that is multifaceted and consequential. That should give us substantial pause. Growth data for country-years in and out of Africa, with and without an active IMF programs and in democracies and autocracies are not as obviously or easily comparable as we assume them to be. We should use them in the knowledge that many of the same political factors that shape economics might also shape economic measurement.

The remainder of the paper is organized as follows. Section II describes how GDP data are constructed and revised. Section III assesses the predictive importance of IMF participation, democracy and other variables on GDP growth revisions using a random forest model, and visualizes the consequences of IMF participation and Democracy on the revisions process using Kolmogorov-Smirnov tests. Section IV uses regression analyses to explore the consequences of those revisions. Section V concludes.

Section II: How are GDP data made?

Political scientists typically pay more attention to the construction of "political" data than economic data. That generally makes sense. Conceptualizing and generating political data is our job; conceptualizing and generating economic data is not. We largely trust others to do it, and trust the results of that process. That would be a fine division of labor so long as macroeconomic data are measured in ways that, even if not fully uniform across countries, are at least not systematically tied to the political variables whose relationships to economics we use those data to uncover. In other words, it is a fine division of labor as long as there is no "political economy of data production."

Unfortunately, there is. The tie between measurement and politics is especially well documented in cases where policymakers are given a predefined economic target.[11] Wallace (2014) shows that Chinese provincial GDP data deviates from electricity production data on a political cycle; Jerven (2014) finds politicization in agricultural output statistics in Malawi, Nigeria and India; Kerner, Jerven and Beatty (2017) find evidence that GNI data is subject to "aid seeking" among countries near World Bank aid eligibility thresholds; Alt, Lassen and Wehner (2014) show how European countries used "fiscal gimmickry" to manipulate national accounts for electoral purposes. While it is harder to produce "smoking gun" evidence of data politicization in the absence of a clear target, social scientists and journalists have long suspected its occurrence, and work by Magee and Doces (2015) and Martinez (2018) suggests the possibility of more routine manipulations by autocratic regimes.[12]

How does this happen? Overt misrepresentations of economic realities are attention-grabbing and important, but a subtler enabler of politicized data is that the process of producing economic statistics is difficult and imperfect and leaves ample room for politics, in the broadest sense of the word, to seep in. National statistical offices are central to the data production process. Especially in developing countries, those offices vary in how well national governments fund them, and in the extent to which international development and financial institutions provide consulting and supplementary financial support.[13] These cross-national differences in statistical capacities are not random. Governments decide to what degree they fund national statistical offices, and governments decide to what degree they seek the support from the

[11] See Herrera and Kapur 2010.
[12] See also Owyang and Shell 2017; Economist 2017; Samuel 2014
[13] Herrera and Kapur 2010: 377

IMF's statistical department or analogous organs within the major aid organizations. Those decisions create politically informed distinctions in the data production process, and politically informed distinctions in the insulation of that process from politics.

We focus on the production of Gross Domestic Product (GDP). GDP (and its derivatives, GDP per capita, and GDP growth) are the primary data points used by academics, policymakers, private capital markets and the public to distinguish rich and/or powerful countries from poor and/or weak countries, growing economics from stagnating economies, and successful governments from unsuccessful governments. GDP may not deserve the prominence it enjoys, but it has it.[14] The process of generating GDP statistics and its history is described in detail in Coyle (2015), Jerven (2013), Ward (2004) and elsewhere. Put here simply, GDP captures the value of final goods and services produced in a country over the course of a year.[15] GDP figures draw on data gathered through administrative sources (i.e. tax collection, or through the accounts of the government and/or nationally owned firms) and through surveys of household consumption, private sector production, and so forth. The quality and comprehensiveness of these data vary according to, among other things, the formality of the economy, whether economic transactions are routinely recorded, property registered,

[14] Coyle (2013), for example, argues in favor of *net* domestic product; even Kuznets, who pioneered national accounting in the United States, was skeptical of GDP as an indicator of national wellbeing.

[15] GDP can be calculated in either of three conceptually equivalent ways: the expenditure method, the income method, and the output method. The expenditure method refers to the familiar Keynesian formulation of consumption + investment + government spending + (exports - imports). The premise behind this approach is that all things must be paid for, and so summing across purchases of good, services and investment by the private and public sectors, less what is consumed domestically but produced elsewhere, summarizes the scale of domestic production. Alternatively, the income approach calculates what the seller earns, rather than what is spent by the consumer, summing across wages and capital income, plus taxes net of subsidies. The output method calculates the gross value of output, and subtracts that from the value of intermediate consumption. Adherence to the IMF's Special Data Dissemination Standard (SDDS) standards requires GDP calculated through either the output or expenditure methods (see http://dsbb.imf.org/pages/sdds/DataDimensions.aspx).

taxes paid, as well as the amount of resources allocated to surveying and measuring the economy [16]

The full set of data needed to characterize the economy is not gathered every year. Instead, GDP estimates are typically tied to benchmark estimates for a base year. In that year a comprehensive survey is used to characterize the economy, its sectoral composition, relative prices, etc. GDP estimates from subsequent years are extrapolated from modeling assumptions and from information gleaned from more limited surveys, which are married to the previously established framework. This process repeats itself until GDP estimates are "rebased"—i.e. recalculated according to the results a new benchmark survey that introduces a new characterization of the economy and allows the introduction of updated data and methods. The IMF suggests a rebasing every 5 years in order to ensure that the models producing GDP data never fall too far out of step with the economy they describe. These estimates are then disseminated to researchers by international financial institutions (IFIs) such as UNCTAD, the World Bank or the IMF. While the IFIs play a non-trivial independent role in data production,[17] the national statistical offices are central to the process.[18] As Jerven (2011: 380) notes, "[international] data sets necessarily inherit all data quality problems originating in the country from where they are collected."

Once data are released they can be and almost always are subsequently revised. Revisions occur for a variety of reasons, including the availability of new or better data, the replacement of judgments made by statistical techniques with data, and the corrections of inevitable computational errors.[19] The biggest revisions tend to occur when countries rebase their GDP estimates after previously failing to do so on timely schedules. The longer a country goes between rebasing, the more unmoored its economic estimates become from its economic realities. Nigeria's aforementioned GDP

[16] Herrera and Kapur 2010, Coyle 2015; Jerven 2015. More generically, surveys are expensive and difficult to administer, particularly when they refer to informal sector activities, and their quality varies with the resources put into them

[17] For example, IFIs occasionally adjust the data provided by the national statistical offices prior to release, and where reliable data are in short enough supply the IFIs report estimates based on economic trends elsewhere (e.g. Jerven 2013: 8-32; 2012:24)

[18] See also, for example, Deaton and Heston 2010:4

[19] See Carson Khwaja and Morrison, 2004

revisions were so large because GDP estimates through 2013 relied on a 1990 base year.[20] Nigeria's economy changed dramatically during that time. Oil and gas's share of GDP decreased from 32 to 14 percent, agriculture's share decreased from 35 to 22 percent, manufacturing increased from 0.09 to 9 percent, and the service sector increased from 26 to 51 percent.[21] The entire Nigerian film industry—which is the continent's largest and currently accounts for 1.42% of Nigerian GDP[22]—barely registered in the data until rebasing.[23] Bluntly put: the models generating Nigerian GDP estimates prior to rebasing described an economy that did not exist. We called that "economy" "Nigeria," but it was neither.

The question of whether to prioritize and fund regular rebasing is above all else a budgeting decision. When politicians decide it is worth allocating resources to, they do. When they do not, they don't. It is political in the clearest and most basic sense of the word. Not rebasing in a timely manner invites further politicization. Consider the predicament faced by Tanzania in the 1980s and early 1990s, as described by Jerven (2011). As the Tanzanian government liberalized the economy, activity shifted out of the formal and state sectors and into the more difficult to measure informal sector. That reallocation of economic activity appeared in the data as a reduction in economic output. It wasn't necessarily—activity was simply shifting to where it could not easily be measured—but an under-resourced statistical office could not measure that activity directly, and had to use their judgements, rather than data, to estimate how the reduction in formal sector activity was offset by a practically immeasurable increase in informal activity. As it happened, Tanzania allowed its estimates of informal sector activity to decline, and it was understood that those declines were at least partially the product of decisions made in the statistical office more than in the actual economy.[24] Those

[20] Estimates from 2014 on use 2010 as a base year.
[21] Sy 2015
[22] Omanufeme 2016
[23] http://www.imf.org/external/pubs/ft/fandd/2016/06/omanufeme.htm. Bizarrely, Nollywood celebrated the 20th anniversary of its rise in 2013—the month-long "Nollywood@20" event before that industry meaningfully appeared in the data. http://www.vanguardngr.com/2013/11/nollywood-20-way-forward/
[24] Maliyamkono and Bagachwa (1990: 61)

"declines" were undone in 1997, when a rebased set of national accounts provided a firmer basis to estimate informal sector activity.

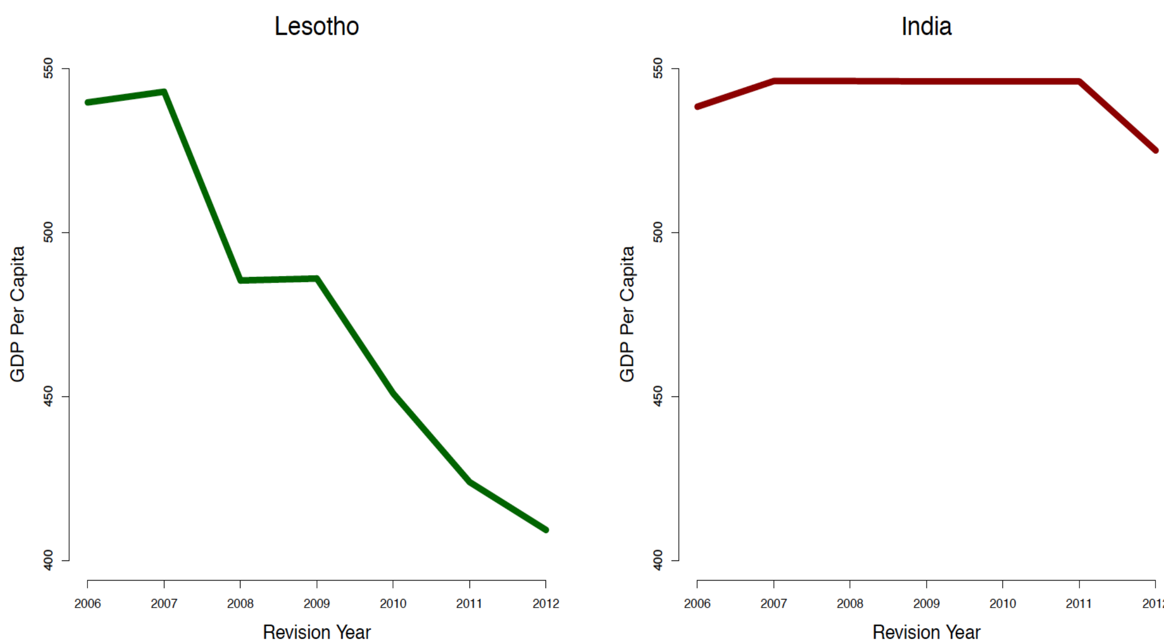
It is less important for current purposes how the Tanzanian authorities managed that situation, but simply that they had to manage it at all. These situations frequently arise, and when they do they are managed by a public sector whose actions occur against a political backdrop. Tanzania managed its national statistical office in the manner of its own choosing, considering the extent of available resources, competing funding priorities, relationships with IFIs, and the political and financial consequences of appearing to be poorer and growing less rapidly than it really was. They could have done this differently–their choices were not inevitable. The period of underreported Tanzanian growth, the timing of the "fix," and the availability of help from the IMF and the UK's Department for International Development (DFID) to guide the rebasing were all the products of choices and contexts specific to the time and place.

To take another, less documented example, consider Lesothan GDP revisions in the mid 2000s. Lesotho in the mid 2000s produced low quality statistics, which the IMF's Article IV country report from 2006 describes thusly:

"...the December 2003 mission noted that, except in the case of agriculture statistics, there was no regular annual survey program for economic statistics. ... The absence of easily tapped administrative data compounded the problem arising from the lack of survey data….A November 2004 mission reiterated the lack of timely and relevant source data to be the major obstacle in implementing the System of National Accounts 1993. A September 2005 mission reported continuing severe staffing problems in national accounts and concomitant problems with source data. The mission also noted the need to re-benchmark and rebase the GDP estimates, but emphasized that shortcomings in source data may preclude this from being done effectively." (IMF 2006: 36)

In the late 2000s Lesotho embarked on several projects to improve its data, with help from the IMF and DFID. That included rebasing its data from a base year of 1981 to 2007, which allowed the introduction of new data sources, including new surveys and administrative data made available through a VAT. As Lesothan data became more reliable, we learned that Lesotho was quite a bit poorer than we thought. In April 2006 the WDI reported Lesotho as having a 2004 GDP per capita of $539.62 (in constant 1995 dollars), essentially the same as India in that year. The next year (2007), 2004 Lesothan GDP per capita dropped to $542.88. In 2008 it dropped to $485.33 and in 2009 it dropped again to $485.92. By April 2012, constant-dollar WDI estimates of 2004 Lesothan GDP per capita had been revised down to $409.28, a reduction of almost 25%. These are big changes that alter our perceptions of Lesotho's place in the world. Figure 1 illustrates that graphically, comparing Lesothan income in 2004 to India's. Original reports were that Lesotho was comparably wealthy India, but by 2012 it was substantially poorer.[25]

Figure 1: Lesothan and Indian 2004 GDPs per capita across vintages



Note: Two Dimensional rendering of Lesothan and Indian 2004 GDPs Per Capita, across revisions years 2006-2012

Of course, nothing actually changed in Lesotho (or in Tanzania, Nigeria, Ghana, or other countries where revisions occur). But the historical record changed, and with it

[25] See Appendix A for a 3-dimensional rendering incorporating a larger swath of data.

the weight of evidence that the past brings to bear on theories relating politics to development. Are Lesothan or Indian political institutions better for development? Answers to that question are necessarily provisional. The drive for retrospective accuracy breeds instability in knowledge.

The situations described above are, at least in broad strokes, common. Many countries' GDP calculations currently use out of date base years, or lack sufficient survey data from which to make reliable GDP estimates. Roughly 50% of countries use benchmark years that are more than 11 years out of date.[26] Haiti, Yemen, El Salvador, Bolivia, Djibouti and the Central African Republic all currently use base years of 1990 or earlier. Many countries have conducted neither an agricultural nor industrial survey for at least 10 years.[27] All of those governments have to consider similar issues as the Tanzanians did, but in different contexts and facing different practical and political constraints. Not every set of authorities should be expected to make the same set of choices. This results in a *political economy of data production* that exists independently of any actors' intentions to "politicize" the data in an objectionable way. National economic statistics are intrinsically the product of a political process, and they need not be "juked" to make them so.

Section II: The Political Economy of Data Production

Why do some governments invest in high quality, data-driven macroeconomic statistics? A common theory is that investments in statistics follow from a government's need to credibly demonstrate its capacity to hit a macroeconomic target. For example, Carson et al. (2002) argue that governments pursuing an inflation-targeting monetary regime invest in statistical capacities because the credibility of that regime requires a reliable method of calculating price indexes. Likewise, demonstrating a capacity to hit developmental benchmarks such as the Millennium Development Goals requires the sort of credible economic data capable of making such a demonstration (Chen et al. 2013; Wold 2005; Sanga 2011).

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[26] Berry et al. 2018

[27] See <<http://databank.worldbank.org/data/reports.aspx?source=Statistical-capacity-indicators>>

We consider that IMF loan agreements act similarly. IMF loans typically demand economic conditions be met as a condition of subsequent financing, with potentially substantial implications for the flow a private sector capital via catalytic lending (Edwards 2005).[28] Those conditions often refer to macroeconomic data, such as IMF's use of debt/GDP ratios to inform a country's debt sustainability.[29] In the same way that an inflation targeting regime creates a need to calculate credible price data, the need to demonstrate that a government can or has hit those targets, to the IMF, to private capital markets and to domestic audiences, plausibly confers the same incentive to develop the capacity to produce accurate and widely accepted GDP data. Of course, active IMF programs do not dictate that governments will produce accurate GDP data, or commit resources towards doing so. To recall earlier examples, Nigeria and Lesotho actively engaged with the IMF in the later 1990s/early 2000s, at the same time that they produced GDP estimates that would later be revealed to be substantially inaccurate. But we suspect that active engagement with the IMF increases those incentives at the margins.

The IMF is not a passive actor in these interactions. Developing statistical capacity is often among the IMF's priorities in the countries it works in (Hollyer, Vreeland and Rosendorf 2011: 1199-1200). The IMF's statistical department can bring substantial human and financial resources to bear, and often coordinates efforts with and by the major national and private sector funders of statistical capacity development (IMF 2017). While the IMF statistical department routinely works with IMF member states even without an active loan program,[30] we suggest that the loan program generates a rationale for utilizing them. Lombardi and Woods' (2008) analysis of IMF surveillance suggests that adherence to statistical capacity frameworks such as the IMF's Special Data Dissemination Standard (SDDS) and (more relevantly for many developing

[28] Some of the IMF's efforts to promote statistical capacity are premised precisely on their ability to help member states access private capital markets (see Cady 2005).
[29] See, for example, the IMF's May 2018 debt sustainability for Mali for sense of the ubiquity of GDP data in the IMF's assessments.
<https://www.imf.org/external/pubs/ft/dsa/pdf/2018/dsacr18141.pdf>
[30] See also the IMF's work on SDSS-Plus standards.
<<https://dsbb.imf.org/#public/sdds-plus>> [30] See World Bank 2017 for a summary of recent efforts.

countries) General Data Dissemination System (GDDS) stems largely from governments' desire to stay in the IMF's good graces. By the IMF's own account, active engagement with the Fund substantially increases the seriousness of developing governments' efforts to increase statistical capacity.[31] In its 2015 review the IMF notes that "Success … has demonstrated the critical role the IMF can play in leveraging its instruments—data standards initiatives, capacity building activities, and surveillance—to promote statistical development. (IMF 2015: 10)."

For the above reasons we expect countries with active IMF programs to produce more accurate data that are subject to fewer and smaller subsequent revisions. To the extent that cruder systems of data production increase the potential for politicization, we would expect that countries without active IMF programs would be more likely to experience negative revisions, indicating systematically excessive optimism in the initial estimates.

Beyond being informative to the data production process, the IMF's potential role interestingly illustrates how a potentially problematic "political economy of data production" can emerge despite the absence of any intention to bring it about. The IMF's statistical department and the various national aid agencies it works with invest in statistical offices precisely to make these data *more* useful. Those investments aid the goal of knowledge accumulation. But because those efforts are distributed non-randomly, they potentially create non-obvious forms of non-comparability across data, even as they reduce some of the data's more obvious problems.

Extant theory also suggests that democracy affects the measurement process and, as such, the revisions process. Hollyer, Vreeland and Rosendorf (2011) argue that democratic governments' heightened sensitivity to popular welfare incentivises the

[31] Those efforts include the work of PARIS21 (Partnership in Statistics for Development in the 21st Century, founded in 1999), which partners the IMF, World Bank, UN, EU, and OECD to provide technical assistance, coordination, and advocacy around statistical capacity development, the World Bank's initiatives, including Statistical Capacity Building (STATCAP, approved in 2004), the Statistics for Results Facility Catalytic Fund (established in 2009), and the Trust Fund for Statistical Capacity Building. As an example, in Bolivia, $50 million of STATCAP funds were used to enhance national cartographic capacities and to help carry out population, agricultural and household economic surveys (World Bank 2017). See Morrison 2005 and IMF 2017 for a summary of recent IMF activities in this area

production of transparent and accurate statistics that allow individuals to engage in meaningful economic planning. They use this logic to explain why democracies release more data than autocracies, but it can just as easily (and arguably more straightforwardly) be used to explain why democratic data might be more accurate in the first instance, and therefore subject to less substantial *ex post* revisions. Martinez (2018) and Magee and Doces (2015) argue that autocrats may be more likely to exaggerate growth rates. Martinez suggests that these distinctions are driven less by motive but by institutional constraints. Political contestation under democratic rule creates checks on power such that politically expedient lies would be caught by opposition politicians and are thus made more difficult. If true, that dynamic would manifest in our data as autocracies having more negative growth revisions (i.e. initial estimates that were later shown to be overly optimistic) than democracies.

The next sections explore those questions empirically.

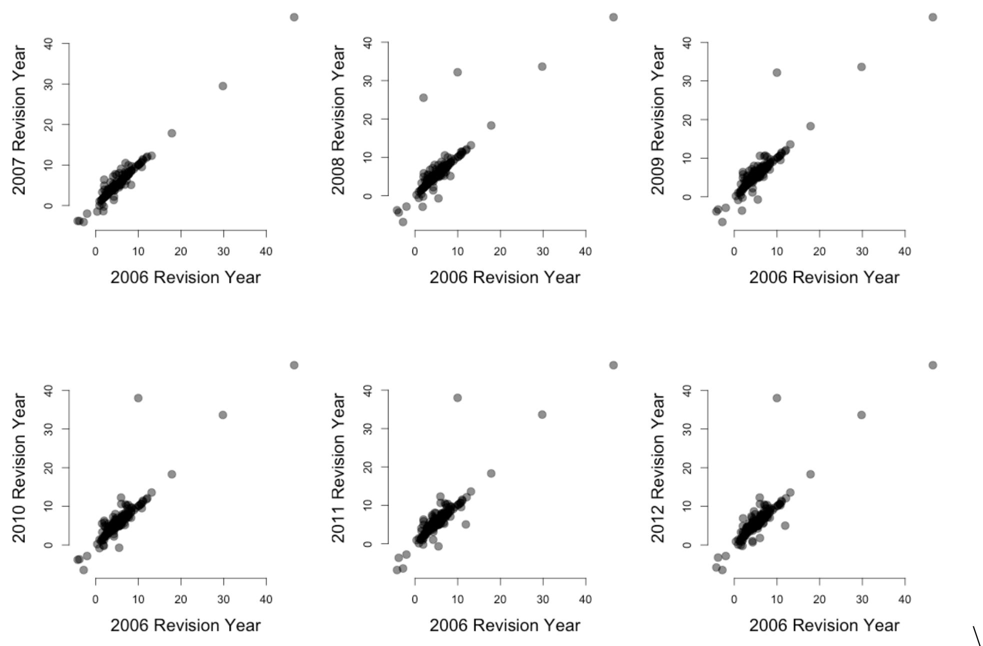
Section III. Empirical Analysis

We first illustrate the scale of GDP growth revisions by plotting the bivariate relationship between GDP growth in its first year of wide availability and as it appeared in subsequent revisions.[32] We treat the release two years removed from the observed year as the "initial" estimate (i.e. June of 2006 for GDP growth estimates of 2004).[33] Our reliance on constant-dollar data limits us to vintages using the same base year. The longest such stretch in the data available to us is between 2006 and 2012, all of which quote constant dollar GDP estimates in 1995 dollars. Beginning our sample in 2006 means that 2004 is the first year for which GDP data is widely reported. There is nothing special about 2004 beyond it's providing a long window of comparable data; we obtain similar results if we use different base years.

[32] We use June revisions as the revision of record for that year. In unreported analyses we replicate with GDP and GDP per capita what we show here with respect to GDP growth. All three data series show similar patterns.

[33] While some GDP per capita data are released in the June following the year of record (i.e. GDP per capita data from 2000 first appear in June 2001) the first instance in which a largely complete sample of data from developing countries is available for download is June of the following year.

Figure 2: GDP growth Across Multiple Vintages



Note: Bivariate relationship between the first year of GDP growth estimate and subsequent revisions. The y-axis is the revised data for one year. On the x-axis of each plot are the original estimates of 2004 growth estimates (i.e. 2006 Revisions).

The x-axes in Figure 2 show GDP growth estimates for the 2004 economy as they appeared in June 2006. The y-axis shows the same data point as it existed in subsequent years. The top left plot shows these data for June 2007, the top middle plot shows these data for June 2008, and so on. The absence of revisions would result in a straight line. The more substantially the revisions, the more the plotted points deviate from that line. In practice, and especially as these revisions compound over time, the relationship between the original GDP growth estimate and the revised estimates move from being nearly exact replicas of the original into something resembling a high correlation. These measures are clearly related, but different enough that old and new estimates are not obviously interchangeable.

Our initial exploration of the drivers of GDP growth revisions uses a random forest model, which, like regression models, simultaneously assesses the predictive power of multiple potential determinants of *GDP Growth Changes*. There are several reasons to prefer random forests to traditional statistical models for cases like this. The most important is that random forest models require no assumptions about functional form. Beyond the likely impact of democracy, the political economy literature does not provide a set of strong *a priori* theoretical expectations about the generative structure of this data. Unlike linear models, random forest models account for unspecified non-linearities, other functional form possibilities, and interactions.[34] For that reason random forest models often constitute "a much better option than an inflexible parametric model that is not fully implied by the theory" (Fariss and Jones 2017, 11). A second attractive feature of these models is that they are naturally robust to outliers, and more akin to median regression than to traditional ordinary least squares (OLS). As Figure 2 shows, several outliers are substantial and might otherwise drive the results.

Our dependent variable—*GDP Growth Changes*—is the difference between the 2012 vintage and the 2006 vintage of any particular country-year observation of *GDP Growth*. As in the data from Figure 2 we focus on revisions to growth estimates made about the world as it existed in 2004.[35] Higher values of *GDP Growth Changes* reflect countries making larger upwards revisions to their *GDP Growth* estimates; lower (more negative) values reflect countries making larger downwards revisions to their *GDP Growth* estimates; values of *GDP Growth Changes* at or near 0 indicate that *ex post* revisions to initial *GDP Growth* estimates are either small or non-existent. Our random forest model utilizes data from 164 non-OECD countries, with one observation of *GDP Growth Change* per country.[36]

We consider several candidate predictors of GDP growth revisions. In keeping with the above discussion, we include in our model a binary indicator of whether or not a country has an active IMF program (Noorudin and Simmons 2006) and the Polity2 measure from the Polity IV dataset. We also include variables meant to capture

[34] Breiman (2001). See Jones and Linder (2016) for a more complete introduction to random forest models.

[35] Other years (and year ranges) suggest similar results, though because older data tends not to be revised, the dynamics that interest us become less relevant for data that more substantially predates 2000.

[36] We focus on non-OECD countries in part because the presence or absence of IMF participation in them during this time period has a very different meaning than it does for OECD countries.

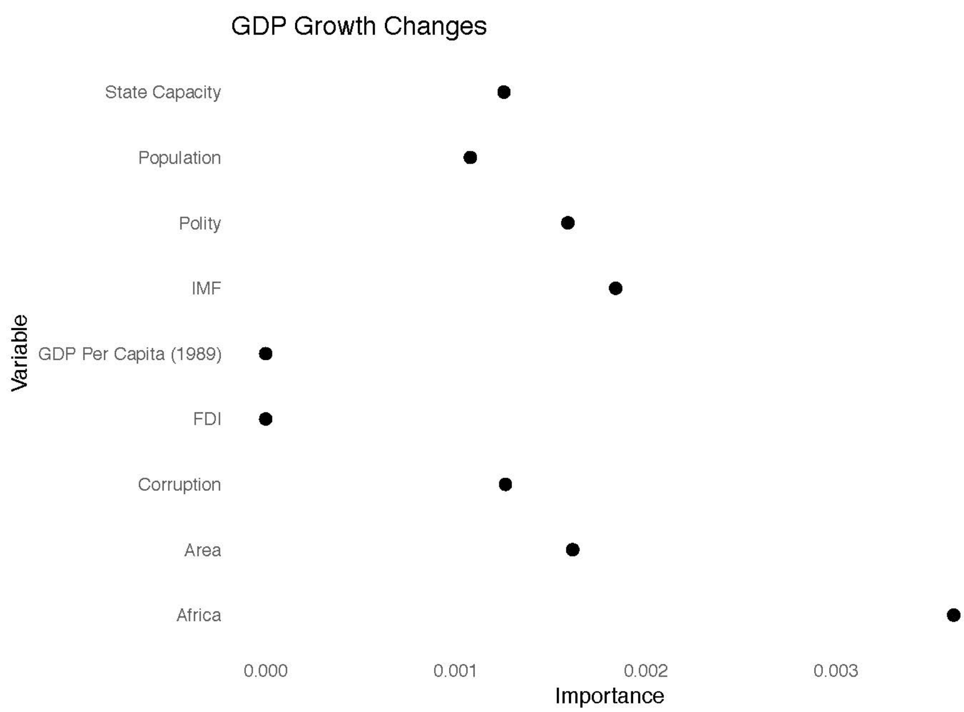
additional, plausible drivers of *GDP Growth Changes*. Levels of public sector corruption plausibly influence national statistical office's independence from political interference through causal mechanisms similar to those described in Martinez (2018). We measure public sector corruption using data from Coppedge et al. (2016). Because existing work suggests that statistics from Africa are notably crude (Jerven 2013; Jerven 2016; see also Samuel 2014), we include a dummy variable coded 1 if a country is in Africa and 0 otherwise. We also include measures of state area, population and state capacity to capture the possibility that the ease of collecting accurate national statistics might decrease with national size or the number of people in a nation, and increase the state's capacity more generally. Population and area data are taken from WDI; state capacity data are taken from Coppedge et al. (2016). We also include a measure of FDI as a percentage of GDP (taken from the World Development Indicators). MNCs can shift profits between affiliated firms in ways that might make GDP calculations systematically more difficult in economies where MNCs have a large presence.[37] Finally, we include a measure of GDP per capita to capture the possibility that the "politics" of data production are better explained by the difficulty of accurate measurement in poorer countries. To avoid endogeneity we use a measure of GDP per capita from 1989 (the exact lag length is practically inconsequential). All other independent variables are measured in 2003, the year prior to the initial GDP Growth estimate. They relate to the state of the world when the original data estimate was made, and not the world as it existed at the time of subsequent revisions.

Figure 3 displays the results of our random forest model. The y-axis sorts our variables from top to bottom in reverse alphabetical order. The x-axis displays estimates of permutation accuracy for each variable. Permutation accuracy is calculated as the difference in mean squared error between a model that is fitted using the observed values for a measure and a model that is fitted using randomly permuted (but realistic) values for the same measure. Positive values indicate that using the actual values of a variable increase the model's predictive performance, and, conversely, that using

[37] The case of Ireland's 2015 GDP estimate—corporate inversions during that year were such that Irish FDI inflows caused (on paper, at least) a nonsensically high 26.3% GDP growth rate --illustrates this dynamic well. (Regan and Brazys 2017)

randomly permuted values decreases its performance. If a variable matters, it should be the case that changing its values diminishes a model's predictive accuracy. A value of 0 indicates that using the actual value of a variable or a randomly permuted version of the value makes no difference to mean squared error. Higher positive values indicate more evidence that a variable matters (in some unspecified way) to the political economy of growth revisions.

Figure 3: Predictors of GDP Growth Revisions



Note: Figure 3 presents the results from a random forest model that examines the predictors of GDP growth revisions. The y-axis ranks variables according to their importance for predicting *GDP Growth Changes*. The x-axis displays estimates of permutation accuracy for each variable, calculated as the difference in mean squared error between a model that is fitted using the observed values for a measure and a model that is fitted using random (but realistic) values for the same measure.

As Figure 3 shows, several of the variables increase the predictive accuracy of the model. The presence of the IMF and the existence of a democratic regime both influence the revisions process. Their calculated permutation accuracies indicate that they are of similar importance as public sector corruption, national area, population size, and state capacity. The variable with the largest effect on the model's predictive power is whether or not a country is in Africa. FDI (measured as a percentage of GDP) and lagged GDP per capita appear to have minimal effects on revisions size. While these results are mute to the functional forms that link these variables to the production of growth data, they provide additional and meaningful support for the claim that the error in macroeconomic estimates—and the process by which initial errors are corrected in the historical record—contains a substantial political component.[38]

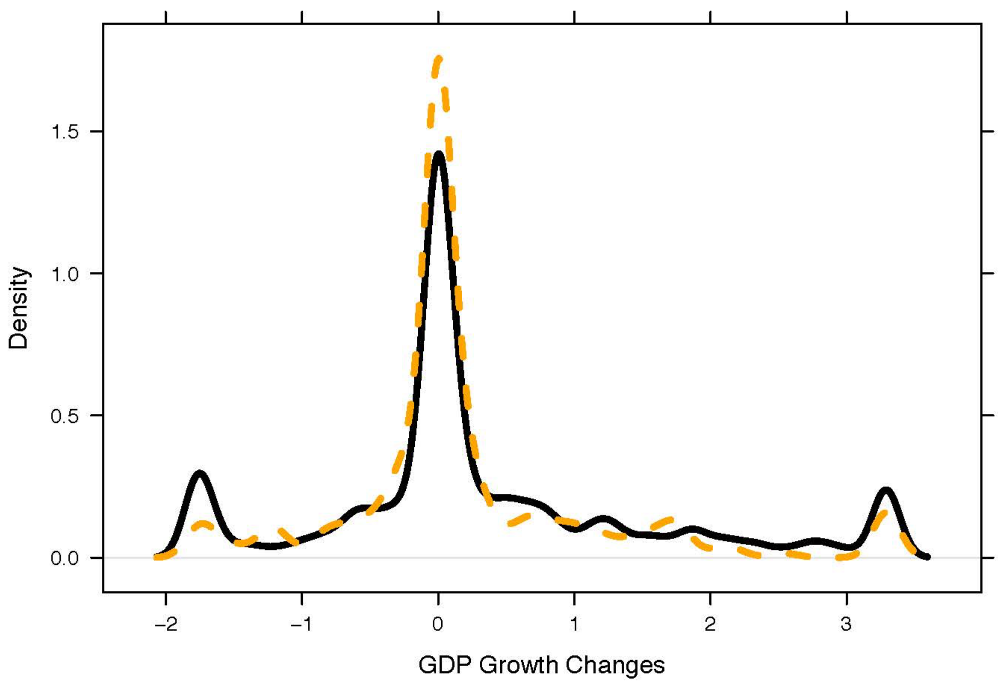
We get a more nuanced sense of how an active IMF program relates to GDP growth revisions by examining the distribution of those revisions directly using kernel density plots. Figure 4 compares the distributions of *GDP Growth Changes* for years with and without active IMF programs at the time the initial estimate was made.[39] The x-axis in Figure 4 indicates the magnitude of *GDP Growth Changes*; the y-axis indicates the height of the density function. The solid dashed orange line denotes country years with an IMF program, while the solid black line denotes countries years without a program.[40] To better focus attention on middle of the distribution we use a winsorzed version of the variable (which accounts for the uptick in observation density at both tails).[41]

The two distributions would be identical if the IMF were inconsequential to the production of *GDP Growth* data. Growth estimates for non-OECD country-years with and without active IMF programs would be "wrong" to the same degree and with the same frequency, and revised to the same degree and with the same frequency. We find instead that growth data from countries without an active IMF program are subject to larger revisions than are data from country-years with an active IMF program, as

[38] In unreported tests we re-ran our models using revisions to other economic indicators (GDP per capita, trade as a percentage of GDP, etc.). Whether a non-OECD country had an active IMF program was always an important predictor of revisions. We take this as evidence that the findings are generally indicative of a process by which IMF presence improves reported estimates, and not a fluke of GDP growth.
[39] We find similar results when lagging IMF program participation by a year.
[40] Both kernel densities are estimated using a bandwidth of 0.075 and an epanechnikov kernel. The use of the common bandwidth eases comparability, but is also quite conservatizing. Calculating optimal bandwidths for each distribution separately suggests a much more dramatic difference between the two distributions.
[41] We winsorize at the top and bottom 5% of the data.

indicated by the sharply higher density spike at 0 for IMF country-years.[42] While the median revision for both series is essentially 0—only a minority of data get revised to any substantial degree—the mean absolute value revision to a GDP Growth datum describing a country-year without an active IMF program is 0.57, vs. 0.25 for a country-year with an active IMF program. A Kolmogorov–Smirnov test (KS test) provides formal evidence that these two distributions of revisions data differ from each other ($p < 0.001$).[43] These differences are especially striking given that in a world without a "political economy of data production" we might reasonably expect that as a baseline condition countries with IMF programs should have *larger* revisions.

Figure 4: Distributions of *GDP Growth Changes* by IMF Presence



Note: Figure 4 presents compares the distributions of *GDP Growth Changes* for years with and without IMF programs. The y-axis indicates the height of the density function and the x-axis indicates the magnitude of *GDP Growth Changes*. The dashed orange line denotes country years with an IMF

[42] The size of the spike in the data is, because of the smoothing of the kernel density plot, misleadingly small. For non-IMF and especially IMF years, the overwhelming mass of data is at are extremely close to 0.

[43] We prefer the non-parametric K-S test to a two-sample T-test because it makes fewer assumptions about the data, such as about its distribution (Lilliefors 1967). We obtain similar results using a T-test.

program, while the solid black line denotes countries years without a program.

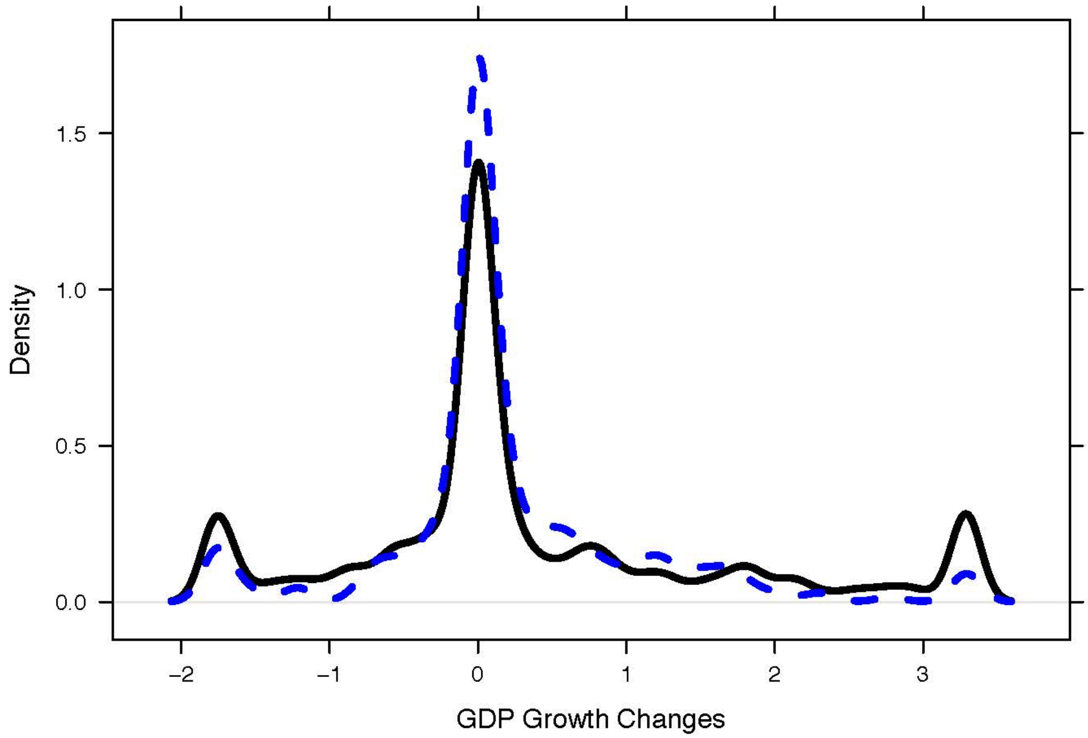
There is less indication in Figure 4 that the absence of the IMF leads to more initial optimism and, thus, more negative revisions. There is some evidence of such an effect in the left tail of the data, indicating, perhaps, that such a dynamic is operative with respect to the very largest revisions. While not dispositive by any stretch, this evidence is consistent with the possibility that under-resourced national statistical offices enable politically useful estimates.

These findings are provocative for what they say about the IMF's role in the political economy of data production, but also for what they say about our ability to know much about the political economy of the IMF. To the extent that GDP Growth data from IMF countries and non-IMF countries are generated through different processes, it complicates our ability to use these data to characterize the IMF's effects on growth. Regressions of IMF involvement on growth will necessarily also capture the effect of the IMF on growth *accounting*. It is not *a priori* clear how this would affect estimates in a real-life setting—that would depend in the specifics of any particular model—but those effects are plausibly substantial.

Figure 5 explores the bivariate relationship between democracy and GDP growth revisions using the same kernel density plots and Kolmogorov-Smirnov tests as above.[44] We separate the data using the same definition of democracy/autocracy used by Magee and Doces (polity scores greater than 5). We find some evidence to support Magee and Doces' and Martinez's expectations that autocracies are less likely to have a positive growth revision (suggesting initial growth estimates were too low) and more likely to have a negative growth revision (suggesting that initial growth estimates were too high), though the differences are subtle. The more visually obvious difference in these data is that democracy correlates with their *scale.* Autocracies are likelier to have large revisions, which comports with expectations implicit in Hollyer, Rosendorf and Vreeland (2010). A Kolmogorov-Smirnov test, though, indicates that the distributions of these revisions are statistically indistinguishable.

[44] The plots use an epanechnikov kernel and bandwidth of 0.075.

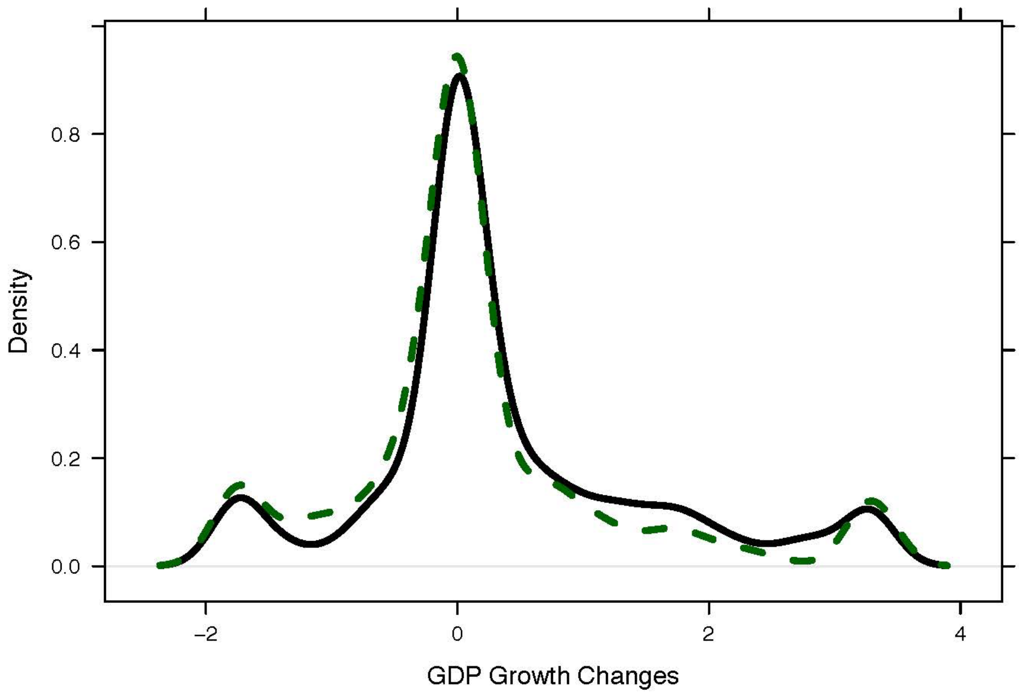
Figure 5: Distribution of Growth Revisions in Democracies and Autocracies



Note: Figure 5 presents compares the distributions of *GDP Growth Changes* for democracies and autocracies. The y-axis indicates the height of the density function and the x-axis indicates the magnitude of *GDP Growth Changes*. The solid black denotes autocratic country years, while the dashed blue line denotes democratic countries years.

To put the above in perspective, Figure 6 shows the same presentation of data as Figures 4 and 5, but separates the data according to whether the non-OECD country is in or outside of Africa. That distinction, to recall Figure 3, had the largest effect on the revisions process suggested by our random forests. The green dotted line in Figure 6 represents African data; the black solid line represents data from non-African, non-OECD countries. We do not observe in these data the same differences in the scale. African data are not revised to any greater degree than non-African data. What we do observe is clearer indication of a directional bias. African data tends to be revised downward, indicating the initial estimates are routinely overly optimistic. We do not have a compelling theoretical rational for African distinctiveness, but it is notable, and consistent with the literature's focus on that continent (e.g. Jerven 2013).

Figure 6: Distribution of Growth Revisions in non-OECD countries in and out of Africa



Note: Figure 6 presents compares the distributions of *GDP Growth Changes* for countries in and outside of Africa. The y-axis indicates the height of the density function and the x-axis indicates the magnitude of *GDP Growth Changes*. The solid black denotes non-African country-years, while the dashed green line denotes African country-years.

A complication to the above is that having an IMF program, being democratic and being located in Africa all correlate in various ways, and these plots do not allow us to examine their effects, conditional on the others. We take a rough cut at doing so by estimating OLS regressions using *GDP Growth Change* and its absolute value as dependent variables. These models include binary indicators for having an active IMF program, being a democracy, and being located in Africa as independent variables. As above, we use a winsorized version of *GDP Growth Change* to reduce the influence of outliers and include our independent variables at a one-year lag. We also include year fixed-effects in each model. Our results are shown in Table 1.

Models 1 and 2 show the results from the models with the absolute value of *GDP Growth Changes* as the outcome measure. The first model focuses on the most recent five years of data (2000-2004) while the second includes an additional ten years (1990-2004). Both models suggest that the fact of an active IMF program reduces the scale of revisions, even controlling for whether or not a country is democratic and whether it is located in Africa. Being democratic also reduces the scale of revisions, though to a lesser extent than the IMF. The results of these models suggest support for our theory as well as the theory implied by Hollyer, Vreeland and Rosendorf.

Table 1: Determinants of GDP Growth Change in non-OECD Countries

	Model 1	Model 2	Model 3	Model 4
DV	Absolute value of GDP growth Revisions		GDP growth Revisions	
IMF (lag)	-0.103*	-0.088*	0.029	0.009
	(0.044)	(0.021)	(0.058)	(0.025)
Democracy (lag)	-0.010*	-0.062*	-0.054	-0.020
	(0.045)	(0.023)	(0.061)	(0.028)
Africa	0.037	0.011	-0.160*	-0.059*
	(0.043)	(0.022)	(0.058)	(0.026)
N Years	711 2000-2004	2115 1990-2004	711 2000-2004	2115 1990-2004

Note: * indicates $p < 0.05$. Standard errors in parentheses. All Models estimated using OLS with year fixed effects.

Models 3 and 4 look at the direction of those changes. Again, the first model focuses on the most recent five years of data (2000-2004) while the second includes an additional ten years (1990-2004). Here we find less substantial results, which accords with the less visually striking results noted in the above figures. The coefficient on our IMF variable is positive in both models, which is consistent with our theoretical expectations, but small and not remotely statistically significant. The partial correlation between democracies and our outcome measure is counter-intuitively negative, but also statistically insignificant.

Across both these models, though, being in Africa correlates with more negative revisions, even controlling for whether or not a country is a democracy or has an active IMF program. *Ceterus Paribus,* African countries are substantially more likely to overstate growth in their initial estimates, which accords with the presentation in Figure 6. That downward bias is smaller in the larger sample (the coefficient drops by almost 2/3), which could indicate that whatever is motivating African exceptionality in the more recent data was not present during the 1990s. It could also be that those biases are just as present in the older data as in the newer data, but that revisions to those data either occurred prior to 2006 or not at all, and are thus not observable with this research design.

Section III. Effects on Empirical Political Economy Literature

The narrative and findings to this point suggest a political economy of data production that likely features domestic political institutions and the IMF. In this section we explore the consequences of that political economy. To what extent does the political economy of growth revisions affect our beliefs about other, already existing political economy relationships? This is a particularly important question given the prominence of the IMF and democracy in the broader political science and international relations literature.

To motivate this section, consider the problems posed by non-randomness in *ex post* revisions when viewed as classical measurement error. Throughout we refer to the concept of interest as "GDP," but we mean that to subsume GDP and GDP-derived measures, like GDP growth and GDP per capita. *GDP* cannot be observed directly. *GDP** can in practice only be observed as an approximation of the underlying latent concept (Rummel 1967). The classical measurement error model assumes that $GDP^* = GDP + U$, where U is normally distributed with mean 0 and σ^2_u (Freedman, Pisani, and Purves 2007), or in words, that measurement error in *GDP** is random (Trochim and

Donnelly 2008). Random measurement error influences statistical analyses in two ways. Consider the case of estimating GDP's effect on an arbitrary political outcome. Random measurement in GDP* would drive the estimated coefficient towards zero through attenuation bias, as the noise in the observed measure overwhelms the signal (Altonji et al. 1999). Researchers would in this case underestimate, and perhaps fail to observe, any possible effect of *GDP* on politics. Random measurement error in the dependent variable would, in contrast, increase estimates' uncertainty (Wonnacott and Wonnacott 1990), resulting in inflated standard errors for explanatory variables. Random measurement error in GDP would lead to unbiased, but less precise estimates of politics' effect on GDP.

In both cases, random measurement error increases the odds of Type II errors. Type II errors are not entirely innocuous, but we tend to worry less about them than Type I errors.[45] In this case, random measurement error should allow researchers to trust that statistically significant and substantively meaningful relationships between macroeconomic indicators and other factors reflect something true about the state of the world. As newer and more accurate data is collected, previously results obtained should only sharpen. Our interest in this section is less in the implications of random measurement error, than the potentially mistaken assumption that measurement error is random in the first place. Indeed, the analyses above strongly suggest that the errors revealed in the revisions process are non-random and related to, if not caused by, the very political processes whose relationship to growth we often seek to understand. If that is true, as it seems to be, the revisions process would not produce similar but more precisely estimated relationships over time. Instead would expect the coefficient estimates to move over time as the non-random errors in the macroeconomic data are corrected. The threat to knowledge posed by the data production is not (just) noisiness and Type II error, it is also, potentially, bias and Type I errors.

[45] e.g., Fox 2015, Wooldridge 2010

We illustrate those consequences by estimating a series of bivariate OLS regressions.[46] These are reduced versions of the canonical relationship between *GDP Growth* and *Democracy* (using the polity2 measure) and *GDP Growth* and the *IMF* (using the same Noorudin and Simmons-derived measure as above). Our focus on the bivariate relationship is purposeful. Any instability that we observe in these estimates is the result of changes in the data, and not magnified or otherwise distorted by conditioning on control variables. Problems identified here are likely to be larger in more complex or more fully specified models.[47]

We estimate these relationships using data covering the years 2000-2004 and do so separately using data vintages between 2006 and 2012.[48] In other words, we ask: how did the IMF/democracy correlate with growth in the first half of the 2000s, and report the answer as it would have appeared to researchers in 2006, 2007, 2008, 2009, 2010, 2011, and 2012.[49] This exercise produces a separate point estimate and confidence interval for each vintage. More formally, we estimate the models shown below in Equations 1 and 2, where *i* indicates the country, *t* indicates the year, and *v* indicates the vintage.

$$EQ1\text{: } GDP\ Growth_{itv} \sim Democracy_{it}$$
$$EQ2\text{: } GDP\ Growth_{itv} \sim IMF\ Program_{it}$$

Figure 7 displays the results our democracy regressions. Plotted points represent parameter estimates, thick bars represent 90 percent confidence intervals, and thin bars represent 95 percent confidence intervals. Each point is labeled with the data vintage used. If the year-to-year error in *GDP Growth* data is random measurement error, the parameter estimates for *Democracy* and *IMF* should remain stable, but the uncertainty around these estimates should fluctuate (and presumably reduce) as revisions are made

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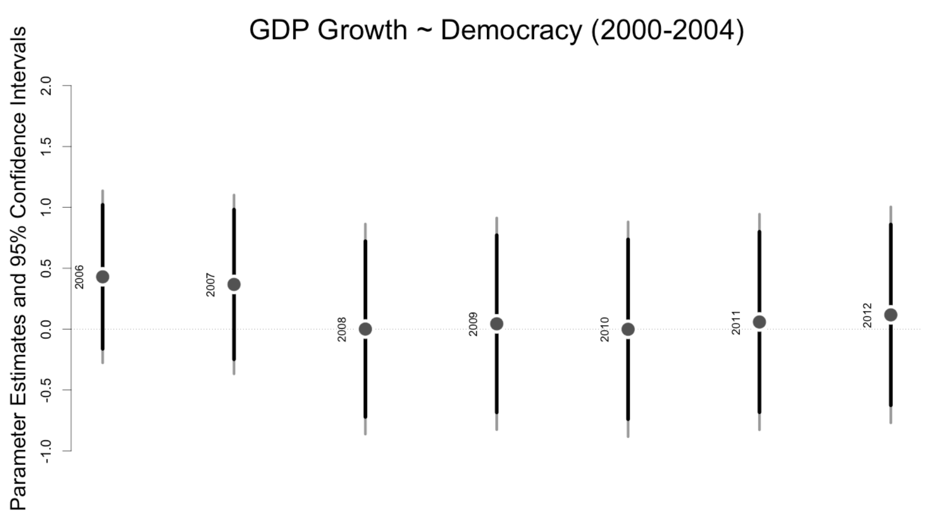
[46] In unreported tests we estimated these relationship using quantile regressions that are more robust to outliers and found essentially the same results. None of the reported results appear to be affected by outliers.
[47] Wooldridge 2010
[48] As before, that interval maximizes the amount of data available using a common base year.
[49] See Johnson et al. (2013) for a similar analysis of Penn World Table data.

over time (Angrist and Pischke 2008). Figure 7 shows no evidence of this. If anything the regression estimates are getting *less* precise over time. Rather, the estimated effects change, and by as much as 50%, reiterating in this context that the revisions to GDP growth are not randomly distributed with respect to democracy, and raising the specter of substantial bias. To the extent that the social sciences are concerned with identifying the substantive significance of social relationships, we should care about being "wrong" by this much. A 50% change in a bivariate relationship affected only by using different versions of nominally the same data is, in our opinion, a very big deal and all the more so considering the prominence of the democracy-growth relationship in the political economy literature. [50]

Figure 7: The Effect of *Democracy* on *GDP Growth*



Note: Figure 7 displays the relationship between *GDP Growth* and *Democracy* using the results from 8 bivariate regressions. Plotted points represent parameter estimates, thick bars represent 90 percent confidence intervals, and thin bars represent 95 percent confidence intervals. Each point is labeled with the vintage used.

Figure 8 replicates Figure 7 using the presence or absence of an IMF program as the independent variable. We arrive at very similar results, which may speak to the correlations between democracy and the existence of an IMF program during this time period. There is a large downward shift in the coefficient estimate moving from the 2007

[50] See, for example, Doucouliagos and Ulubaşoğlu (2008)

to the 2008 vintage of data, and no evidence of a diminution in standard errors over time as estimates nominally become less affected by measurement error. Unlike the prior exercise, however, these estimates move from showing a positive correlation that is statistically significant at the 0.05 level when using data from the 2006 and 2007 vintages to a positive relationship that is statistically insignificant at any conventional level. That too is a problem. As a field we rely on distinctions in statistical significance for guidance on which correlations are worthy of consideration (and publication) and which are not. As Figure 8 shows, simply changing the data vintage can alter those distinctions, even in as simplified an empirical environment as ours.

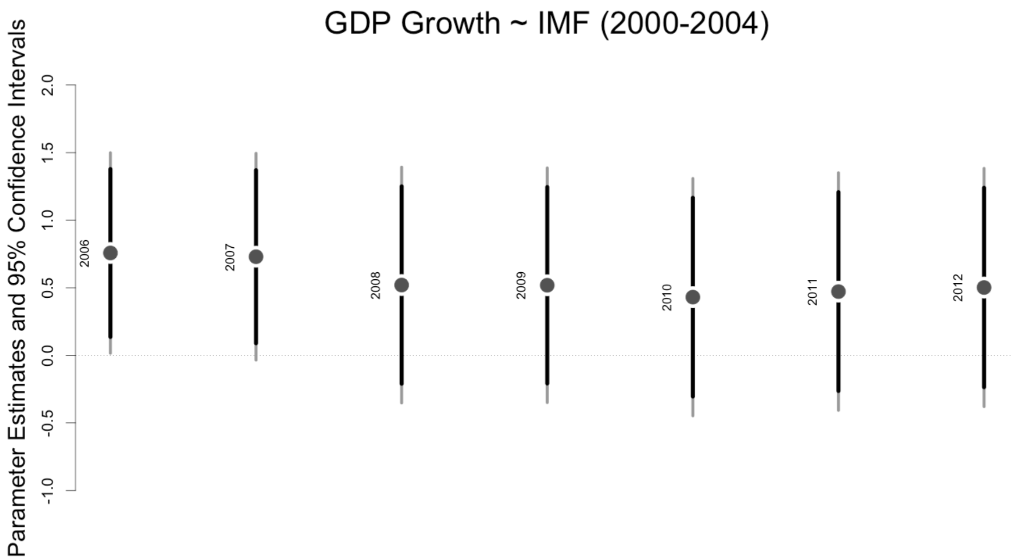
Of course, changing the data vintage is not uniquely capable of moving coefficient standard error estimates at this scale. Such differences are routinely the product of different modeling choices. But we would be rightly concerned about the validity of a finding if its coefficient could change by 50%, or if its statistical significance were undone, by an otherwise seemingly innocuous choice of estimator or the means of computing standard errors. As a field we have largely decided that such a degree of sensitivity matters. We should think of a politicized process of data production as threat of similar scale. Not one that will always be consequential, but one that analysts should consider to the extent that their work is plausibly affected by it.

We should stress that the above are not, strictly speaking, replications. The priority that we give to simplicity ignores confounding factors, endogeneity, selection bias and other barriers to causal identification.[51] These regressions are not designed to model plausible political economy relationships; they are intentionally simplified laboratories designed to show that estimates of those relationships can be sensitive to changing the data vintage. Models that are truer to the data generating process add complexity that might in some cases minimize those differences, but in other cases magnify them. In either case the political of economy of data production lurks in the background, and should be taken seriously as a threat to making causal inferences in

[51] However, in unreported tests we find that similar effects in a multivariate growth regressions including measures of *Primary School Enrollment*, *Income Inequality*, and *GDP*. Those models are meant to approximate a model introduced in Alesina and Rodrik (1994)

models that are otherwise designed to do so.

Figure 8: The Effect of *IMF Programs* on *GDP Growth*



Note: Figure 8 displays the relationship between *GDP Growth* and *IMF* using the results from 8 bivariate regressions. Plotted points represent parameter estimates, thick bars represent 90 percent confidence intervals, and thin bars represent 95 percent confidence intervals. Each point is labeled with the vintage used.

Section V: Conclusion

This paper argues that the IMF shapes the political environment in which GDP growth data are produced, leading to a systematically different data production process for countries with and without active IMF programs. We test and find support for that proposition using a dataset of revisions to the WDI's GDP growth data. We find further that these differences are consequential. Basic political economy relationships are sensitive to the vintage of data used to estimate them. The sensitivities that we observe in these data are large enough to warrant attention from researchers working in areas that are plausibly affected. It is difficult to know if politics affects the economy when politics also affects how the economy is measured. At a minimum it suggests the utility of replicating prior work in CPE and IPE. Even if there were nothing "wrong" with the original study, our beliefs about the accurate values for those data are likely now different, and there is little reason to assume away those differences as noise.

But the broader and hopefully more enduring contribution of this paper is to add to the rising chorus of work suggesting that there is a non-trivial and multifaceted "political economy of data production" and to consider that it is affected by a country's external relationships. But in adding nuance to our understanding of those relationship we also underscore how limited our understanding of it is. Measuring and reporting macroeconomics is an important state function. Governments don't just affect the economy; they also play a key role in describing it, and the process by which those descriptions are made is as rife with political implications and antecedents as any other. That process is dramatically understudied. Better use of economic data in political applications in CPE or IPE research requires better knowledge of how politics that underpin those data's creation. Revisions can be a useful tool towards those improvements, but revisions are just a small, observable slice of the data production process and important largely for what they imply about a broader data generating process that is less amenable to straightforward analysis. Advancing that research agenda means finding new ways to study it.

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Appendix A1:

Figure A1 illustrates the Indian-Lesothan wealth divergence with a pair of three-dimensional graphs. The left-side plot shows the evolution of Indian GDP per capita between 2000 and 2004, over vintages ranging from 2006 and 2012; the right side plot shows the same for Lesotho. If a country-year data point were to stay constant over vintages, the plots shown in Figure 1 would be flat across the Z-axis. That is largely the case for India. There is a slight uptick moving from the 2006 vintage to the 2007 vintage, and a slight downtick moving from the 2011 vintage to the 2012 vintage, but the data stay more or less constant and produce a fairly smooth plane. Lesothan GDP per capita data, on the other hand, drops precipitously and erratically across vintages.

Figure A1: Lesothan and Indian GDPs per capita across vintages

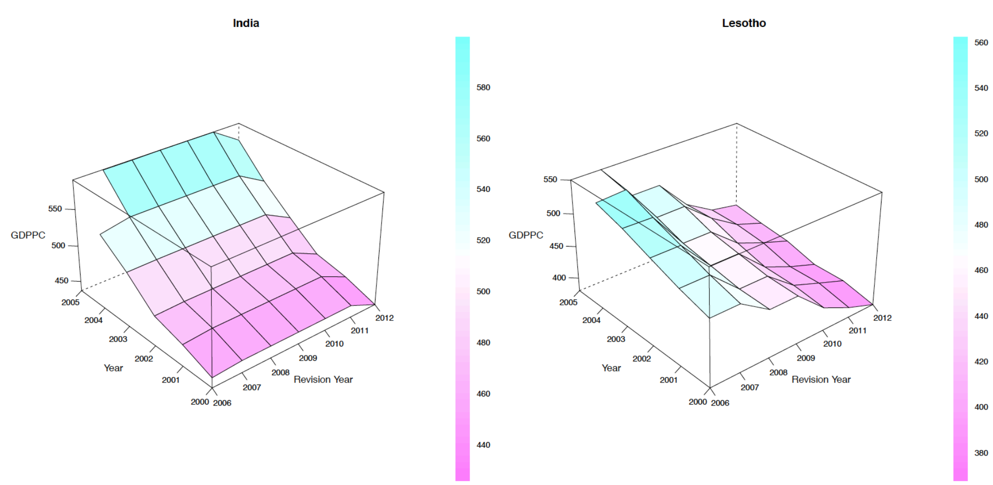

Figure 1: ***Note:*** Three Dimensional rendering of Lesothan and Indian 2000-2005 GDPs Per Capita, across revisions years 2006-2012