FATF blacklists don't work the way you think they do[*]

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Abstract: In this paper we reconsider the existing evidence regarding the impact of FATF's "blacklists," the lists that FATF maintains of jurisdictions that to varying degrees fall short of its anti-money laundering standards. Most observers argue that the lists are key to explaining FATF's impact, relying on what Morse (2019) calls the "market-enforcement mechanism" hypothesis: that listing would cause investors to flee, harming the listed jurisdiction's economy in the process. Evidence on this effect has been mixed. In this paper we replicate models that purport to confirm the market hypothesis by looking at the effect of listing on cross-border bank liabilities and find that the inclusion of more comprehensive data erases the effect. We then examine the impact of listing on a different asset class—cross-border portfolio asset investments—with the same cases and again find no effect of listing. These results confirm earlier findings that question the market mechanism interpretation of FATF's impact. We provide preliminary discussions of alternative explanations.

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1 Introduction

No aspect of the international anti-money laundering regime is as poorly understood, and yet so often referenced, as the Financial Action Task Force's "blacklists": the multiple lists that FATF maintains of jurisdictions that to varying degrees fall short of its AML standards.[1] Most observers–practitioners and scholars alike–argue that the lists are key to explaining FATF's impact. At the core of that argument is what Morse (2019) dubs the "market-enforcement mechanism" hypothesis: that listing would cause investors to flee, harming the listed jurisdiction's economy in the process. That logic is plausible, but evidence for it is mixed at best. Most research relies on anecdotal evidence or FATF's own assessment. The most systematic quantitative treatments–Kudrle (2009) and Morse (2019)–contradict one another. Kudrle concludes that investors do not depend on the lists to shape their investment decisions. Morse finds that listing has a statistically significant and negative impact on cross-border bank liabilities.

In this paper, we reconsider existing evidence on the impact of the FATF lists and contribute new evidence for consideration. In the following section, we provide a brief background on FATF's use of public lists, which highlights the uncertain trajectory of blacklisting as a strategy within FATF. In section IV we show that the significant and negative effect of listing in Morse (2019) is driven largely by imbalanced sample selection. While the original analysis included only 10 countries, adding the full sample of listed countries for the time period considered nullifies the significant effects. Descriptive plots further support this conclusion. Given those results, we then consider a second operationalization of investment flows–cross-border portfolio asset investments–and again find the effect of listing to be null. These findings strongly suggest that being listed by FATF does not lead to a significant change in investment patterns.

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[1]As we discuss below, there are multiple lists that signify different levels of AML system deficiency. Throughout the rest of the paper, we will refer to them collectively as the lists and use more specific terms to indicate more specific lists.

That said, it is clear that most FATF insiders find the lists to be necessary. In section V we provide a preliminary discussion of alternative explanations for the null results we observe and the common perceptions to the contrary. These include opportunity costs; the possibility of change over time and a failure to update mental models; and the problem of noisy signalling in the AML regime.

We conclude with a consideration of the implications, which are significant. If our findings hold, investors are relying on information beyond FATF to make investment decisions. That holds important implications for states and jurisdictions on the receiving end of investment decisions, like de-risking, that are often attributed to blacklists and the AML regime. It also calls into question a fundamental assumption about the drivers of FATF's impact. Given the much needed attention as of late on enhancing the effectiveness of the regime, the analysis presented here is an important contribution to shaping the future operation of FATF and the AML regime.

2 Blacklist background

The use of lists of any kind has been a controversial idea from the very beginning of FATF. FATF's first Annual Report (1991) notes that even the idea of "white lists"–naming those with strong AML systems– failed to generate consensus. Instead, the network's work centered around mutual evaluations and peer review (Gutterman and Roberge, 2019). The only note of enforcement was to be found in Recommendation 21 (now Rec. 19), which called for all members to practice extreme caution in dealing with financial bodies from states that did not meet FATF's standards, including obtaining written explanations justifying any financial transaction.

In response to egregious, on-going non-compliance by two members–Austria and Turkey– FATF developed a process of warning, naming, and potentially expelling non-compliant members (Nance, 2015). Those efforts were exceptions that proved the rule: FATF members

did not design FATF to maximize its enforcement capacity.

In 2000, FATF implemented a Non-Cooperative Countries and Territories (NCCT) process that would begin a new era more focused on enforcement, especially after the 9/11 attacks in the United States and the addition of counter-terrorism financing to FATF's remit. But prior to 9/11, in just four months, members reviewed "problematic" jurisdictions and listed 16. They added 8 more the following year. While this process earned FATF a more aggressive reputation and FATF members claimed it was successful, members stopped adding jurisdictions after 2002. All jurisdictions had been de-listed by 2006. Members then suspended the NCCT process, in large part because the World Bank and IMF were strongly critical of the process (Nance, 2018b). While FATF and some scholars (e.g., Drezner 2008) claimed it was effective, the NCCT process generated a mixed bag of compliance, partial compliance, and continued non-compliance among the targets.

In 2007, members replaced the NCCT process with the International Cooperation Review Group. From 2007-2009, the ICRG used mutual evaluation reports to create two lists. A grey list comprised states with "significant deficiencies" in the AML systems. Only two states made it onto the second list, the true blacklist: Iran and North Korea. Only those two states faced official FATF calls for countermeasures. That remains true to this day: FATF has never called for members to apply counter-measures against any jurisdiction other than Iran and North Korea.

In 2010, members again altered the listing process to its current arrangement. FATF members now issue two statements after Plenary meetings. The "Public Statement" establishes two categories of jurisdictions with serious deficiencies: those to which countermeasures apply and those against which there is no formal call for countermeasures. As before, only Iran and North Korea have ever been placed on the true blacklist of those countries facing calls for countermeasures. The 2nd category—deficient but not facing countermeasures—is a rotating cast of large and small developing countries and jurisdictions; Turkey, Vietnam, Nigeria, Sao Tome and Principe, Ethiopia, Yemen, and Ecuador, among others, have all

made appearances. The second statement FATF issues following plenaries is entitled "Improving Global AML/CFT Compliance: On-going Process." It names jurisdictions that have deficiencies but that also have made a "high-level political commitment" to address the deficiencies. These are subject to increased monitoring by FATF or its partners. Within the AML community, the two groups that make up the Public Statement are referred to as the blacklist, while the "On-going Process" list is commonly referred to as the grey list.

There are two key takeaways from this history. First, members did not design FATF to be a tool for forcing AML systems on states. While some states supported that idea, most members did not. Second, the consistent re-design of the blacklists suggests that they were not working as members would like. This most recent iteration is the longest standing version to-date, suggesting perhaps that members are now more comfortable with the process. This only underscores the need to better understand the mechanisms at work, and not at work, in the listing process. As the following section shows, research on this question is far from conclusive.

3 Debating the blacklists

Multiple studies cite the intended use of blacklisting as not just a "naming and shaming strategy", but also a cue for global investors about the potential investment risks associated with the listed countries (Sharman, 2009; Kudrle, 2009; Levi et al., 2018; Gutterman and Roberge, 2019; Morse, 2019). This "market-enforcement mechanism" hypothesis suggests that the material costs of divestment from blacklisted countries will coerce government leaders in listed countries to implement AML reforms and monitoring in accordance with the FATF guidelines. This mechanism has been suggested to occur through myriad channels:

- An actual direct withdrawal of funds (Kudrle, 2009; Morse, 2019), which may be more likely when the institutions in the target country are less concerned with reputational issues (Sharman, 2009);

- An anticipated direct withdrawal of funds (prior to listing) when the target country relies on high profile institutional business that is more sensitive to reputational concerns (Sharman, 2009);

- A loss of future deposit or investment opportunities, such as refusal to allow international expansion of depository institutions or loss of future loans (Morse, 2019);

- A reduction of funds due to the slowing down of financial transactions caused by increased scrutiny and oversight (Sharman, 2009);

- Refusal to work with local banks as correspondent banks or money service businesses (Levi et al., 2018); usually associated with "de-risking", which refers to the withdrawal of banking relationships wholesale from markets, including country or lines of business that might be subject to greater scrutiny by regulators (Nance and Tsingou, 2020; Gordon, 2019).

Most studies just assume the market mechanism works, but do not test the question. Drezner (2008) writes that the US and EU "were able to cajole, coerce, and enforce a global anti-money laundering standard into existence," but he provides no evidence of that enforcement. As already noted, the few studies that do aim to test the market mechanism hypothesis directly generate mixed results. Sharman (2009) tests the effect of being listed on compliance among a small sample of countries, but the market mechanism remains largely assumed. Kudrle (2009) evaluates the effect of being listed on the OECD tax haven list and the FATF blacklist on bank assets and liabilities, and non-banked assets and liabilities, among tax haven countries using data from the Bank of International Settlements (BIS). The results of this analysis are inconsistent, showing support for some instances of decreases in bank assets/liabilities as a result of being listed but also some instances of increases in bank assets/liabilities following listing. Kudrle suggests that investors likely base their behavior on factors beyond the lists. More recently, Morse (2019) also employed quarterly BIS data to examine the effect of blacklisting on bank liabilities for the most recent review period of

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2010-2015, and finds a significant and negative effect. Despite compiling blacklisting data for 47 countries, the final sample of countries used in the analysis includes only 10 listed countries, raising concerns about sample selection and generalizability. The aggregate effect of blacklisting on investments in targeted countries thus remains open to debate and demands further scrutiny.

Given those mixed results, it bears noting that scholars and practitioners alike emphasize the importance, or even primacy, of aspects of FATF other than the market-enforcement mechanism when explaining the network's influence. Simmons (2001) specifically points to the US' unwillingness to pay enforcement costs and says it relies on peer pressure to embarrass governments into compliance. Abbott and Snidal (2000) credit the accommodation of national diversity, the expectation of political costs for non-compliance, the legitimation of third-party influence, and the invocation of a legal discourse. Tsingou (2018) argues that members of the compliance industry as an epistemic community have become "the new governors on the block" and propel the regime forward. Jakobi (2013) argues via network analysis for a quasi-Gramscian understanding of US influence. Sharman (2009, 2011) argues for the constitutive effect of blacklists, such that those listed are constructed as bad actors, which turns key domestic constituencies into advocates for reform for fear of financial costs. Hülsse (2007) points to FATF's ability to persuade officials that money laundering was an important problem (via "ontological persuasion"), while Hülsse and Kerwer (2007) specify FATF's ability to market practical knowledge as the "correct" solution and thereby gain rule-making authority. Finally, Nance (2018a) emphasizes that FATF's impact has effectively been negatively correlated with the degree to which the network has emphasized enforcement over knowledge creation and diffusion. In interviews, practitioners also generally emphasize non-material factors as important drivers of the regime. That said, practitioners often emphasize the importance of the lists, too, just as many of the scholars just cited see social dynamics of authority and embarrassment existing alongside list-focused enforcement.

4 (Re)Considering the evidence

4.1 Empirical approach

We aim to move forward the debate about the impact of FATF gray and blacklisting by re-examining existing evidence for the "market-enforcement mechanism" hypothesis and process tracing the role of FATF lists in investor decision making through expert interviews. We begin by first replicating the findings in Morse (2019) and extending the analysis to another measure of bank investments: cross-border portfolio asset investments. First, the conflictual findings in previous studies, combined with the fact that Morse's empirical analysis reduces the full sample of 47 listed countries to only 10, raise concerns about the sample selection and generalizability of these results. Our hypothesis here is that including the full sample will nullify the negative significant effect found in the more limited sample.

To test the hypothesis that the effect of listing is in fact null, we first replicate the findings in Morse (2019) specifically included in Table 4, Model 4, which examines the effect of listing on bank liabilities using BIS data with a set of controls. In the original paper, the author finds that being listed has a negative and significant effect on bank liabilities, controlling for other variables that might affect variation in bank liabilities. This significant negative effect means that a country's appearance on the lists is associated with a decline in bank liabilities. However, any observations missing one value for one of the control variables are dropped from the analysis, resulting in a large drop in the number of observations. The overall sample size is reduced from 3,288 to 656 and the number of listed countries included in the sample from 47 to 10.

Control variables are important to include in regression models to separate their effects from the main explanatory variable, thereby reducing estimate bias in the main variables of interest (being listed). However, eliminating a large portion of the sample can itself generate bias especially if those missing values are not random, but driven by some systematic factor.

Thus to provide initial support for the problem of imbalanced sample selection in Morse's analysis, we examine the sample of listed countries and compare the effect of blacklisting across bivariate regressions, i.e. without the control variables, using first the original model sample and then the full sample. Removing the control variables in the bivariate regressions allows us to compare the baseline effect of listing between the model sample and the full sample of observations, again simply because observations were removed in the original model due to missing values of the control variables.

Further, we expect other operationalizations of cross-border investment may be relevant to consider. Morse's outcome variable—bank liabilities—is primarily an indicator of bank deposits and short and long-term debt securities and may or may not be an appropriate measure for the "market-enforcement mechanism" hypothesis. That is, a decrease in liabilities from one period to the next would require a direct withdrawal of funds by depositors or portfolio asset investors (of short or long-term securities). Yet there may be differences in responsiveness of these two asset classes to FATF lists. While depositors may be responsive to regulatory or reputation risk associated with FATF lists, this is arguably a relatively static asset class compared to portfolio asset investments. Short and long-term debt securities on the other hand may more responsively reflect global investor risk perceptions as these assets are a) more liquid and b) fluctuate with daily trading behavior. Prior research has indicated that investors use heuristics (information short-cuts) such as country or region-level indicators to drive investment decisions, particularly in sovereign bonds markets (Gray, 2013; Brooks et al., 2015; Bodea and Hicks, 2018). If blacklists are one such heuristic, as implied in the AML literature and "market mechanism" hypothesis, this would arguably be better captured in examining the effect of listing on portfolio asset investments versus bank deposits. Using aggregate bank liabilities however does not allow one to distinguish between these two asset classes.

We therefore extend Morse's analysis by examining the effect of blacklisting on cross-border portfolio asset investments using the IMF Coordinated Portfolio Investment Survey

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(CPIS) data in a similar series of regressions. Here our prior expectations are not strong; though we suspect that if an effect were to be found it would more like occur among this relatively more liquid and risk-responsive class of assets, we suspect that the effect will likely be null again given prior conflictual evidence and unclear specification of the "market-enforcement mechanism". The results of our replication and extension of Morse's analysis are presented in the next two sections, followed by a discussion of possible alternate explanations and concluding thoughts.

4.2   Replication results & discussion

Table 1 Model 1 presents the replicated results of Morse's Table 4, Model 4.[2] As in Morse's original model, country fixed effects are included to control for within-country correlation and results are presented with robust standard errors. As described in the previous section, the data used in this analysis include all the country-quarter observations for which there are no NA values for any of the control variables. This reduces the full original data set from 3,288 observations to 656 observations, and the sample of countries that are listed at all in the period of analysis (2010-2015) is reduced from 47 to 10 countries. Meanwhile, the sample of never-listed countries is reduced from 88 to 39. In this original model, the author finds a significant negative effect of Listing on bank liabilities, where Listing is a dummy variable that takes on the value of 1 for each quarter a country is listed and 0 otherwise. Going from 0 to 1 is associated with a decrease in bank liabilities, as the "market-enforcement mechanism" hypothesis would predict.

In order to test the effect of listing on the full sample of countries, we drop the control variables in Model 2, which shows the results of the bivariate regression of the effect of just Listing on bank liabilities. Again, dropping the control variables allows us to compare the baseline associate between Listing and bank liabilities in the model sample with the full

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[2]The time polynomial was coded incorrectly in the original model such that only "Time" and not the squared or cubed Time terms were included in the model. We include a set of regressions with the corrected time polynomial in Appendix Table 4, which yield similar results as those presented in Table 1.

sample given that missing observations in the original model were largely due to missing values in the control variables. Model 3 in Table 1 thus shows the results of running the same bivariate regression as Model 2 but using the full sample of countries. This fuller sample of 2,680 observations includes all 47 countries that are listed at all in the period of analysis.[3] Both bivariate regressions can be interpreted as the baseline correlation between listing and bank liabilities, controlling for country and time effects. That is, the effects of a bivariate regression should not be interpreted causally, rather they should be considered as a test for significant association (either positive or negative) between two variables—in this case, listing and bank liabilities.

In Model 2, the fact that the effect of Listing remains negative and significant and of a similar magnitude in the bivariate regression as in the full model (Model 1) suggests that the control variables are not controlling for any additional bias of Listed.[4] That is, the results suggest there is a negative and significant association between Listing and bank liabilities, and the addition of control variables does not do much to change this effect. However, once we add in the full sample to the bivariate regression in Model 3, the effect of Listing becomes insignificant. So the significant negative association between Listing and bank liabilities in the original sample (with 10 listed countries) seems to be driven by biased sample selection resulting from dropped observations due to missing values in the control variables, while the control variables are not controlling for any additional variation. After adding in the full sample (with 47 countries), the significant association between Listing and bank liabilities goes away, meaning the effect cannot be distinguished from zero and there is no particular relationship between Listing and bank liabilities. Collectively this supports the conclusion that the control variables are not controlling for a biased effect of blacklisting, but rather the sample itself used in the model is biased.

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[3]The reduction of observations from 3,288 to 2,680 is a result of missingness in the outcome variable (bank liabilities).

[4]Appendix Table 3 presents the same results but with additional models that iteratively drop control variables to show that the size, direction, and significance of Listing does not change across models as control variables are removed.

Table 1: The Effect of Listing on Bank-to-Bank Lending

	Dependent variable:		
	log.liab		
	(1)	(2)	(3)
Listing	−0.157***	−0.148***	−0.029
	(0.045)	(0.046)	(0.019)
Inflation	0.010***		
	(0.003)		
GDP (%Δ)	0.002		
	(0.005)		
Real Ex. Rate	−0.00001		
	(0.00002)		
Credit-to-GDP	−0.004***		
	(0.002)		
Debt-to-GDP	−0.008***		
	(0.002)		
Money Supply	0.001		
	(0.001)		
i-Rate Spread	−0.006		
	(0.006)		
Time	0.027***	−0.018***	−0.007**
	(0.008)	(0.007)	(0.003)
Observations	656	656	2,680
Note:			*p<0.1; **p<0.05; ***p<0.01

To examine and support the null effect found in the full sample (Model 3) compared to the significant negative effect results in the original model sample (Model 2), we use boxplots of the outcome variable to compare the distributions of bank liabilities of listed versus non-listed countries across both samples. Figures 1(a) and (b) present the box plots for the model sample (a) and full sample of countries (b). The boxplots indicate there is no significant difference in the distributions of the pooled sample of listed versus non-listed country-quarters; on average the level of log liabilities of listed and non-listed country-quarters does not differ. More importantly, it shows that the distribution of listed countries in the model sample (a) is left skewed, meaning there is an over representation of lower values of logged bank liabilities compared with the distribution of the full sample (b). This suggests that observations dropped due to missing control variable values were removed non-randomly;

more observations of listed countries with higher values of log liabilities were systematically dropped due to relatively more missingness in the control variables among these observations. This may be one factor contributing to the downward bias (the significant negative effect) in Models 1 and 2.

Figure 1: Log Liabilities Box Plots
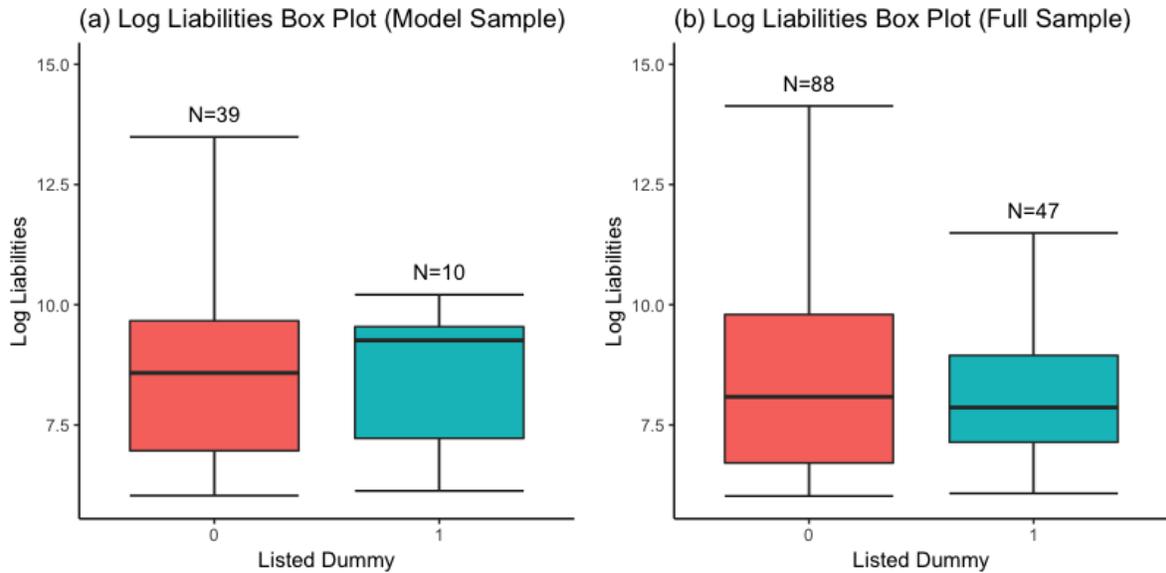
To diagnose the problem further, Figure 2 presents the time series data of quarterly bank liabilities over the period of analysis for listed countries, with vertical blue lines indicating the quarters in which these countries were listed. The plot includes only the country-quarter observations that were included in the original model (Table 1 Models 1 and 2). Figure 2 demonstrates that with the missing observations, there is little opportunity for within-country comparisons. That is, for some countries so many observations are missing that there are no years of a country *not* being listed such that the level of bank liabilities pre/during/post listing cannot be compared in the model. Figure 3 presents the times series of the full set of observations for the 10 listed countries included in the model, and Figure 4 presents the time series of the full set of observations for all 47 listed countries. These figures show that, with all the observations are included, overwhelmingly there does not appear to be an obvious discontinuity in the level of liabilities before, during, or after a country is

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blacklisted.

In the next section we examine the effects of listing on a different measure of investment as the outcome variable of interest. By re-running the baseline regression model and presenting similar time series plots, we show there exist similar null effects when considering portfolio asset investments as our outcome variable.

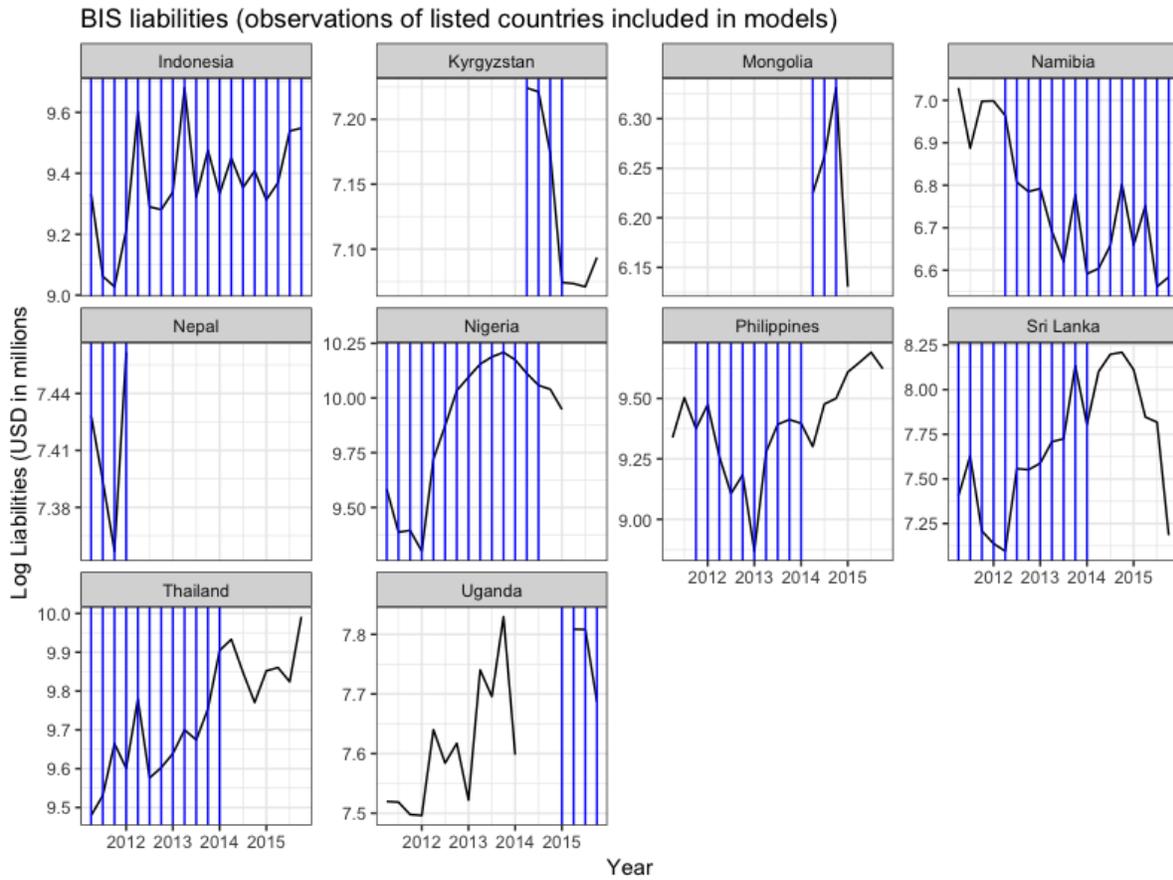Figure 2: Time Series for Model Listed Countries (Model Observations)

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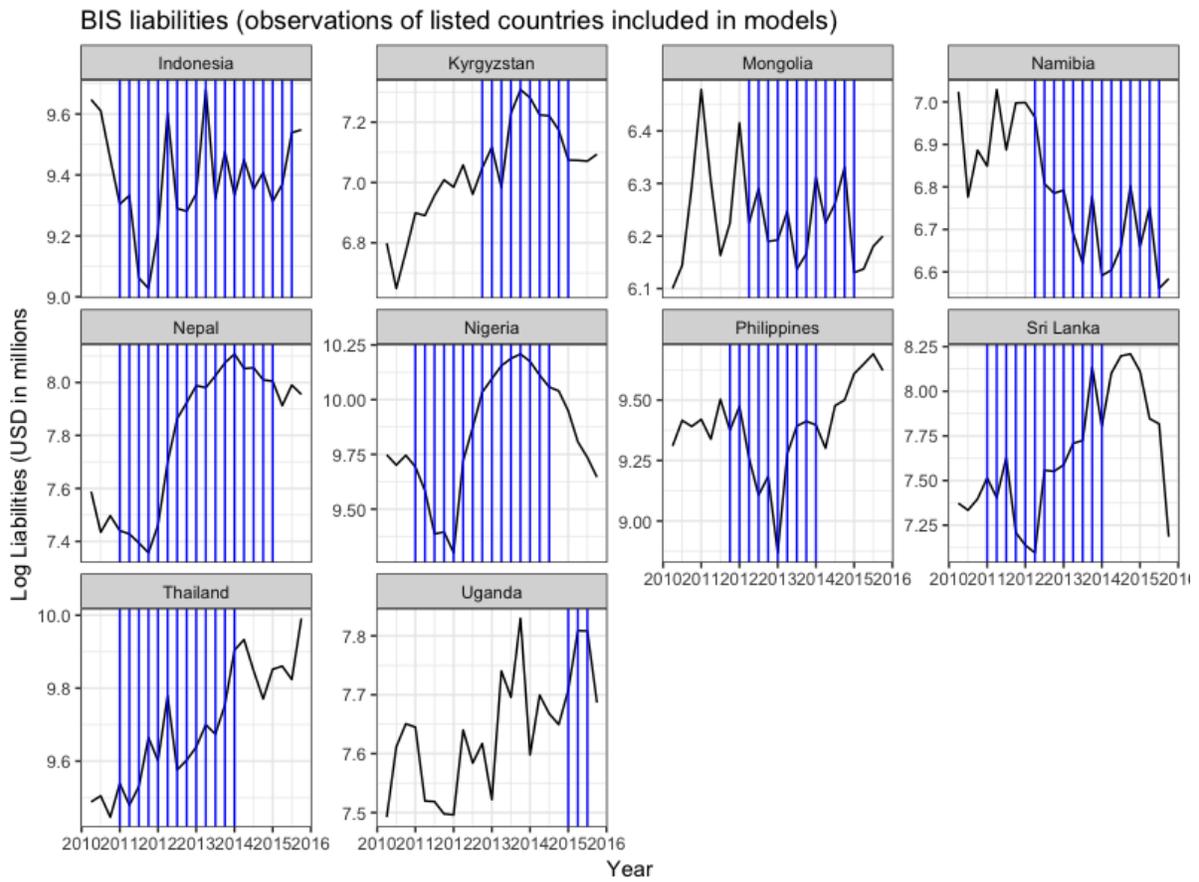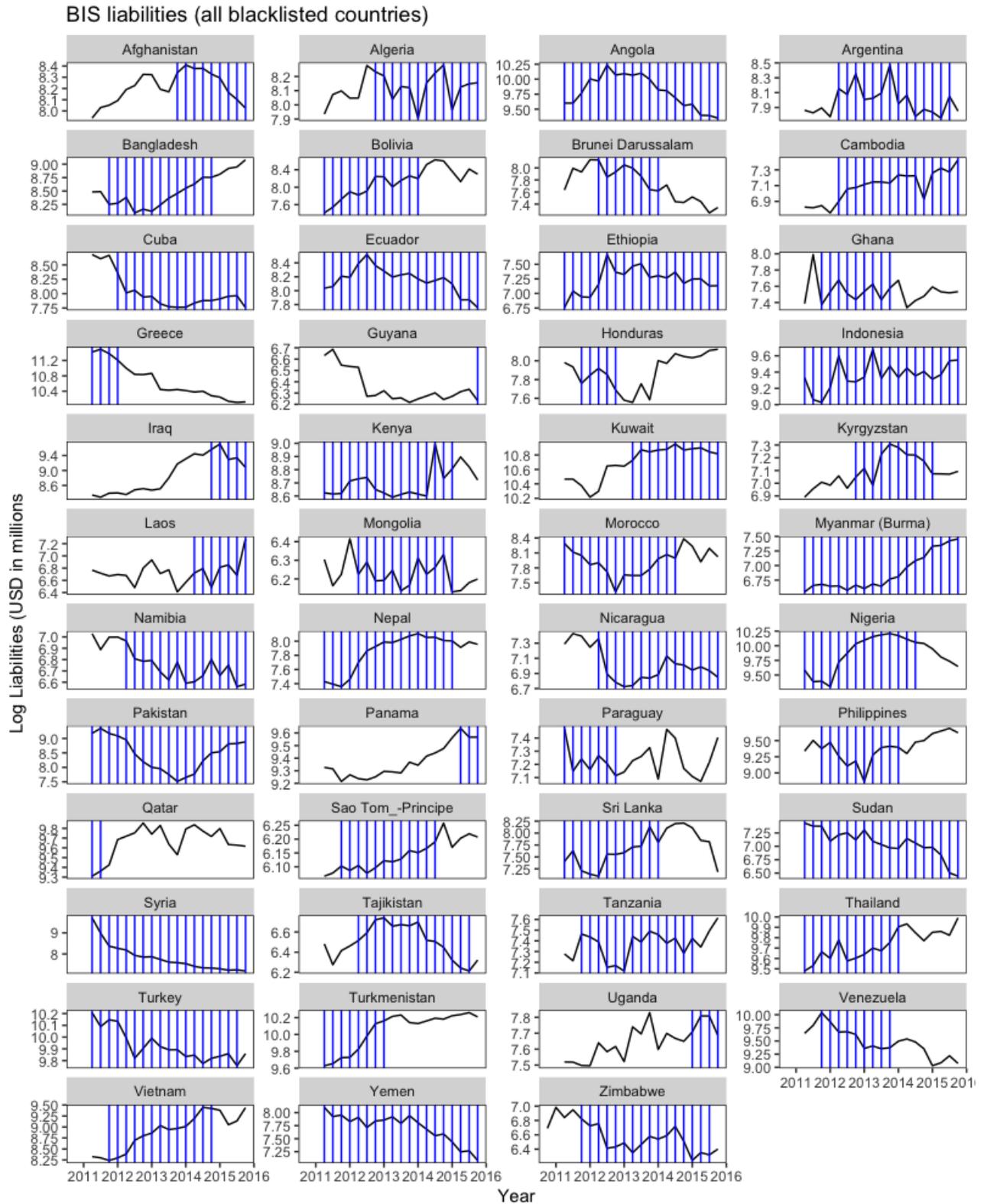Figure 3: Time Series for Model Listed Countries (Complete Observations)

BIS liabilities (observations of listed countries included in models)

Figure 4: Time Series for All Listed Countries

BIS liabilities (all blacklisted countries)

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4.3   Extension results & discussion

We extend our replication of Morse's analysis by considering the effect of blacklisting on another measure of cross-border investment—portfolio asset investments—using the IMF CPIS data. We use this data to construct an outcome measure of total investment inflows in a given country-year. This measure of investments differs from the BIS data used in Morse's analysis in two main respects. First, the IMF CPIS data captures all tradable assets, including equities and short and long-term debt securities. Neither deposits nor loans are reflected in this data. It therefore reflects capital that is highly mobile and is arguably a more responsive reflection of global investors' risk perceptions. Thus if investors are using FATF lists as a heuristic to drive investment decisions as suggested by the "market-enforcement mechanism" hypothesis, we would expect this to be reflected in this data. To check that this extension is not completely redundant and that there are in fact meaningful differences in the variation between BIS and the IMF CPIS data, Figure 6 in the Appendix plots the logged time series of the BIS and IMF CPIS data against one another for the sample of listed countries. The trends differ considerably in magnitude and timing for some countries, suggesting there may be some variation not captured in the BIS data that, if related to listing, may be masking a significant association between listing and investment in the full sample.

A second difference is that the data is collected on a bi-annual rather than quarterly basis, and we use year-end data in our analysis. On the one hand, this presents a barrier to examining the immediate effect of listing on investment flows. However, as shown in the time series plots of the BIS data in the previous section, most countries are listed for multiple sequential years. So it is reasonable to assume that if the "market-enforcement mechanism" is at play, this would get picked up in the level of investments at the year-end following listing, and in subsequent years in which a country remains listed.

We begin by re-running similar regressions as above, both the full model with control

variables and a bivariate regression using the full sample. Table 2 presents the results of the replicated model in Morse's Table 4 Model 4 (Model 1 in Table 1 above) with logged portfolio investment inflows as the outcome variable. Once again the model includes country fixed effects and a time polynomial (this time correctly coded). We include the full model, which like the original model has a limited sample of 488 observations, and the bivariate regression without controls has a more inclusive sample of 2,224 observations. In each case, a couple hundred observations are lost from the first model due to missingness in the new dependent variable and the fact that we are looking at annual instead of quarterly observations.

Table 2: The Effect of Listing on Portfolio Investment Inflows

	Dependent variable:	
	Log Portfolio Investment Inflows	
	(1)	(2)
Listing	−0.019	−0.064
	(0.045)	(0.114)
Inflation	−0.005*	
	(0.003)	
GDP Growth (Percent Change)	0.020***	
	(0.006)	
Real Exchange Rate	−0.0001***	
	(0.00002)	
Credit-to-GDP Ratio	0.005***	
	(0.002)	
Debt-to-GDP Ratio	0.007**	
	(0.003)	
Money Supply	0.001	
	(0.001)	
Interest Rate Spread	0.028***	
	(0.006)	
Time	0.449***	0.663*
	(0.150)	(0.368)
Time^2	−0.057	−0.148
	(0.055)	(0.137)
Time^3	0.0004	0.011
	(0.006)	(0.015)
Observations	488	2,224
Note:	*p<0.1; **p<0.05; ***p<0.01	

The results indicate that whereas in the full model using bank liabilities as the outcome, the effect of Listing was negative and significant, here the effect of Listing on portfolio asset

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investments in the full model (Model 1) is not significant. That is, in the limited sample and using the same controls as the original model, there does not appear to be a relationship between listing and portfolio asset investments. Unsurprisingly then we also find that in the bivariate regression (Model 2) with the expanded sample of observations, the effect of listing is again insignificant (not distinguishable from zero). Therefore a country going from being off the blacklist to on the blacklist does not have a particular relationship with portfolio asset investments, even in the bivariate regression that would establish a baseline correlation between Listing and investments. The upshot is that even among a more liquid and risk-responsive class of assets, wherein we might expect investor responsiveness to FATF lists to be more apparent than in bank liabilities, listing still does not have an effect.

Finally, Figure 5 shows the time series trends of the (unlogged) IMF CPIS portfolio investment inflows for the set of listed countries included in the full sample (of Model 2). Once again there do not appear to be clear, systematic discontinuous trends in bank liabilities where countries are put on/off the blacklist. Collectively these results suggest that in the aggregate, even among a more mobile and responsive class of assets, the effect of black-listing appears to be null. This does not conclusively mean that there is in fact no impact of blacklisting on investment; it could be the case that neither bank liabilities nor portfolio asset investments are the relevant measures to be considering. Yet these efforts to reconsider existing evidence cast doubt on prior empirical support for the "market-enforcement mechanism" hypothesis. Moreover, these null findings extend to another class of assets where we would expect the "market-enforcement mechanism" hypothesis to hold. In the next section, we discuss our efforts to process trace the role of blacklists in investor decision making to inform our consideration of possible alternate explanations.

Figure 5: Time Series for All Listed Countries

Investment Inflows (blacklisted countries; blue line at years on blacklist)

year
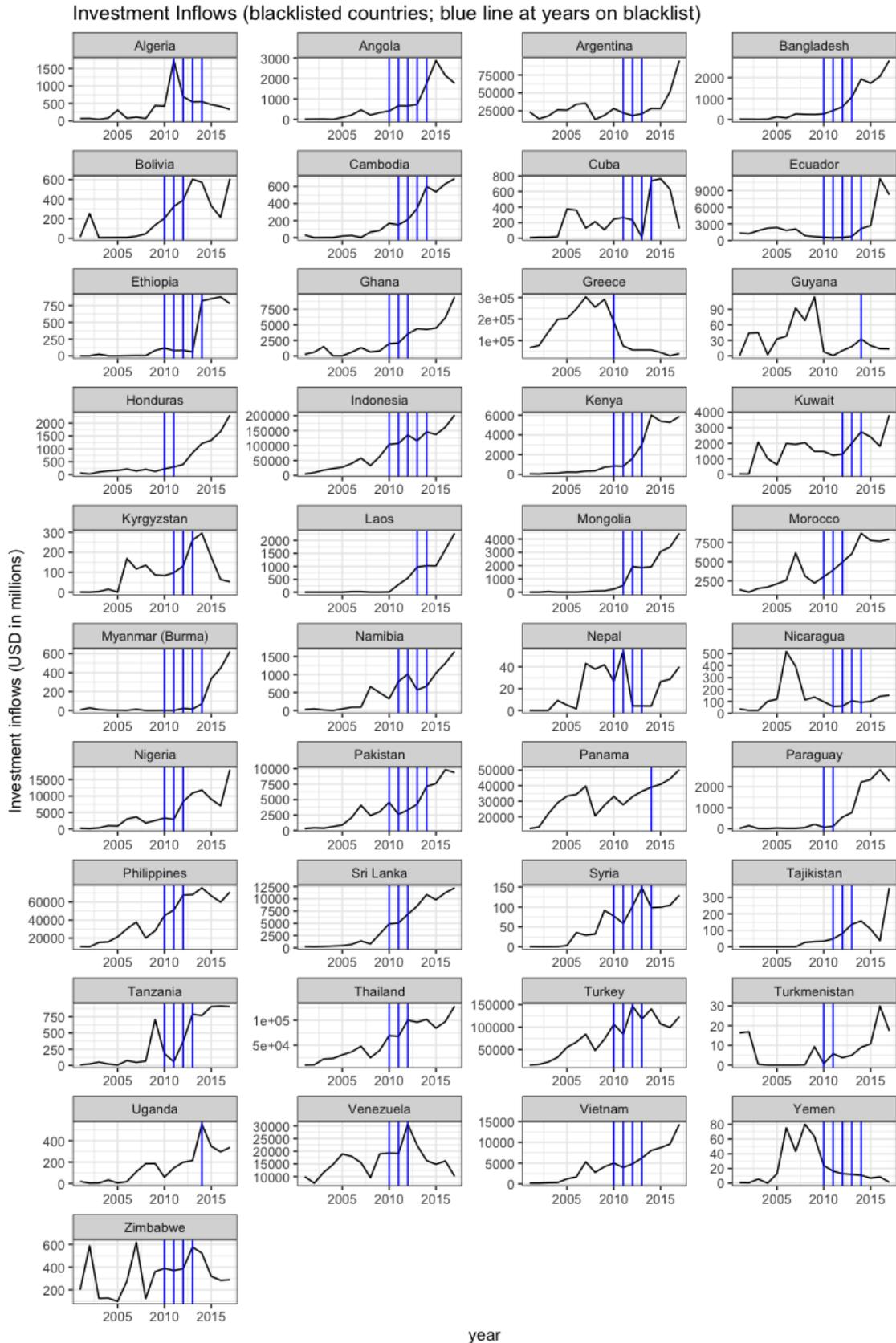
5 Alternative explanations - a preliminary discussion

The analysis above casts doubt on the market mechanism hypothesis in the scholarship on FATF. The amount of cross-border liabilities or incoming portfolio investments do not appear to vary systematically as a result of blacklisting on a within-country or cross-country basis. This is supported by the null results of a baseline regression model that includes the full sample of listed countries for both bank liabilities and portfolio asset investments as the outcome variable of interest. It is also supported by the additional descriptive plots comparing distributions across treatment and the over-time trends for listed countries.

Critically, this does not nullify the the other findings from Morse (2019), namely, that listed states comply with the recommendation on terrorism financing criminalization more quickly than states that are not listed, and that increased market integration, measured by cross-border liabilities, seem to intensify that disparity. Nor does it reject the general notion expressed by scholars and by multiple informants in interviews with the authors that the FATF lists are an important part of the AML regime. Rather, the paucity of evidence supporting the market mechanism and the insistence by practitioners that it matters present a compelling puzzle. If the market mechanism does not explain the role of the FATF lists, what does? Without claiming to present an exhaustive list, below we present four alternative explanations and very briefly consider the evidence for and against each.

The first alternative explanation—the opportunity costs explanation—sees the impact of the lists as forward looking. In other words, the real impact of listing is not capital flight, but capital avoidance. One informant, who otherwise was fairly skeptical of the importance of the lists, proposed precisely this explanation. Ultimately, however, an opportunity cost explanation rests on the credibility of the threat. Country Y fears that landing on the lists will lead to either a decrease or stagnation of investment. That data above suggest that is not the case. The descriptive plots in Figs. 4 and 5 are useful visualizations on this point.

A second explanation suggests that policymakers might fear the impact of listing, despite

the evidence, not because of it. That is, they believe that being listed will lead to investor flight or avoidance. This requires a more thorough treatment of the perceptions and beliefs of participants. Interviews with participants lend some credence to this explanation. One German Finance Ministry official made the point most clearly (Nance, 2018a). After talking about the importance of peer review and knowledge sharing within FATF, when asked about the lists, he was adamant that they, too, were critical. He emphasized that the lists generated fear of financial harm. But when asked to provide an example where that happened, he said directly: "It's simply believed."

A third explanation extends from that point of belief. If AML experts believe the lists have a financial impact despite evidence to the contrary, where do those beliefs come from? It could be misinformation and the power of economic models that see a positive, linear relationship between financial market share and political power. It is also possible that the lists used to matter, but no longer do: or perhaps matter less. The history of blacklisting recounted above includes several iterations. The work of Hülsse (2007), Hülsse and Kerwer (2007), and Nance (2015, 2018b,a) emphasizes the important role of legitimacy. As the FATF listing process became less diagnostic and more punitive, however—and especially with the securitization of FATF following the 9/11 attacks in the United States—targets of the lists proved themselves more willing to openly contest the legitimacy of the network. If increasing numbers of actors in the AML regime understand the FATF lists to be a process dominated by political preferences over technical expertise, then it stands to reason that they would also reject the validity, and utility, of the lists. In that case, those who see the lists as foreshadowing capital flight or capital avoidance may simply have failed to update their mental models: a failure to engage in Bayesian updating. This framework would still predict that states alter their behavior in light of the lists, regardless of the financial impact, at least for some time. The research on FATF listing as a whole lends some credence to this line of thought.

A fourth explanation revolves around the notion that the lists are a signal to investors

about country risk. We currently are investigating this alternative. While statistical analysis helps us see the existence or lack of correlations, we also need research that relies on careful process tracing to better understand how a listing filters out to an investor and to financial institutions (FIs). When we asked one head of compliance in an interview how this filtering happens, he emphasized the plethora of lists that now exist: "the OFAC list,... the EU list, the Bank of England's got a list, the UN's got a list, I mean, you could go on and on."[5] Banks decide for themselves which lists to include in their decision-making about the risk that a given transaction involves.

Given the complexity of the decision and the potential consequences of getting it wrong, there is now a highly specialized market for services that help FIs take in and filter that information more quickly. Many of the names in this space are well-known: McKinsey, EY, Dow Jones. Others are smaller and specialized in only "Know Your Customer" (KYC) processing. Some companies specialize in the platform or engine that sorts the data, other companies specialize in just the data, while still others do both. The newest versions, many of which seem to have come on-line after 2015, have substantial artificial intelligence or machine learning components to them. In Scandinavia, a different model is emerging as six of the biggest banks have combined to found a collaborative KYC utility, Invidem, that creates a regional standard for KYC and shares client information across members to make on-boarding customers more efficient. While our research into this area is still on-going, preliminary interviews with relevant actors, including compliance officers and owners of these newest companies, give reason to further doubt that the FATF lists would lead to the kind of divestment that the market mechanism hypothesis suggests. There are at least two key reasons for that. To begin with, as just noted, the FATF list is one of many lists. In other words, it is one signal among many signals. In our early research, nothing suggests that FATF's lists are given any additional weighting.

Perhaps more importantly, FATF's lists are rather blunt instruments. They provide

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[5]Informant 1. June 5th, 2020.

investors and FIs with information about country risk, but nothing else. In other words, as a signal the lists transmit a lot of noise. In our interviews to date, the informants spoke in large part about lists of names: about individual people, not even institutions, and much less entire countries. The founder of one company (via screen-sharing) showed how their platform could be used. He showed the results of searching a prominent oil and gas company, but then focused on the fact that the platform brought up information on the people associated with that company, and in some cases the associates of those people. These companies provide a dizzying array of micro-level information to FIs. This is further compounded by the host of companies that are engaged in transaction monitoring to identify suspicious activities in something closer to real time. In that world, a list that covers a whole country might well seem useless. The lists are faint, fuzzy signals is a world of multiple, increasingly laser-like signals.

The one key exception to this is the role of the regulator. Both informants talked of the need to show regulators that screening utilities included the FATF list in particular as part of the algorithm. But that likely is a box-ticking exercise. We are dubious that regulators dive into the details of utilities' algorithms to see how the lists are weighted versus others sources of information. A final alternative explanation is that we and others simply have not looked in the right corner of the financial world. There are many other places to look. But any model that finds an impact—for example on credit ratings—would also need to explain why those findings fail to generate side-effects visible in the data considered here and elsewhere.

6   Conclusion

The central contribution of this paper is to present a new analysis of existing data and a new operationalization of the key dependent variable of the market-mechanism hypothesis. At a minimum, these results confirm that we lack reliable evidence supporting the market

hypothesis. We are willing to go further and say that the preponderance of evidence supports the idea that FATF's lists have no widespread, identifiable impact on capital flows.

Establishing or nullifying empirical support for the market mechanism hypothesis and process-tracing how it actually functions (if at all) is important to the credibility and efficacy of the AML regime. As some have already noted, the composition of the FATF lists appears to be based less in empirical evidence and more in the political interests of the most powerful states composing the FATF (Gutterman and Roberge, 2019). Moreover, policy debates continue regarding banks' de-risking behavior and the degree to which the AML regime is to blame for banks' wholesale withdrawal of relationships with correspondent banks in countries targeted by the AML regime (Nance and Tsingou, 2020). Despite a lack of empirical support for the effect of the AML regime (including blacklisting) on divestment, the AML regime is the target of blame for driving de-risking behavior and a set of solutions to address the problem are already being proposed (Ibid.). A more precise understanding of the mechanisms of change at play within the AML regime will allow policymakers to create a more impactful AML regime.

Finally, the findings we report here are not necessarily bad news for FATF. The most effective AML system requires that actors remain within the formal financial system. FATF's stance on the debate over de-risking makes this clear. In FATF's view, the denial of services to customers based on AML risk is generally considered a bad outcome. Banks, they argue, should manage risk, not avoid it. In that sense, the fact that the lists don't lead to what in effect would be country-level de-risking is in line with that goal. The proliferation of utilities and screening technologies is also good news, as it means FIs have more information than ever about their customers. None of that is to say that the whole system is effective, as most every person writing about the AML regime has argued at some point. But without clear evidence to establish the efficacy and/or functioning of blacklisting as a primary feature of the AML regime, problems in existing policy cannot be diagnosed and more effective policy is difficult to develop.

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8 Appendix

Table 3: The Effect of Listing on Bank-to-Bank Lending (w/ Iteratively Dropped Controls)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Dependent variable:					
				Log Bank Liabilities					
Listing	−0.157***	−0.158***	−0.157***	−0.166***	−0.154***	−0.154***	−0.145***	−0.148***	−0.029
	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.046)	(0.019)
Inflation	0.010***	0.009***	0.010***	0.009***	0.008***	0.008***	0.008***		
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)		
GDP (%Δ)	0.002	0.002	0.003	0.001	0.007	0.007			
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)			
Real Ex. Rate	−0.00001	−0.00002	−0.00002	−0.00002	−0.00000				
	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00001)				
Credit-to-GDP	−0.004***	−0.004***	−0.004***	−0.005***					
	(0.002)	(0.002)	(0.002)	(0.002)					
Debt-to-GDP	−0.008***	−0.008***	−0.008***						
	(0.002)	(0.002)	(0.002)						
Money Supply	0.001	0.001							
	(0.001)	(0.001)							
i-Rate Spread	−0.006								
	(0.006)								
Time	0.027***	0.029***	0.030***	0.021***	0.023***	0.023***	0.021***	0.018***	−0.007**
	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.003)
Observations	656	656	656	656	656	656	656	656	2,680

Note: *p<0.1; **p<0.05; ***p<0.01

Table 4: The Effect of Listing on Bank-to-Bank Lending (w/ Corrected Time Polynomial)

	Dependent variable:								
	Log Bank Liabilities								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Listing	−0.159***	−0.160***	−0.158***	−0.165***	−0.151***	−0.152***	−0.143***	−0.152***	−0.023
	(0.045)	(0.045)	(0.045)	(0.046)	(0.046)	(0.046)	(0.045)	(0.046)	(0.019)
Inflation	0.010***	0.009***	0.010***	0.009***	0.009***	0.008***	0.008***		
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)		
GDP (%Δ)	0.001	0.001	0.002	−0.0001	0.006	0.007			
	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)			
Real Ex. Rate	−0.00001	−0.00002	−0.00002	−0.00002	−0.00000				
	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00001)				
Credit-to-GDP	−0.005***	−0.005***	−0.005***	−0.005***					
	(0.002)	(0.002)	(0.002)	(0.002)					
Debt-to-GDP	−0.007***	−0.007***	−0.007***						
	(0.002)	(0.002)	(0.002)						
Money Supply	0.001	0.001							
	(0.001)	(0.001)							
i-Rate Spread	−0.007								
	(0.006)								
Time	−0.249**	−0.250**	−0.252**	−0.303**	−0.286**	−0.284**	−0.303**	−0.214*	−0.280***
	(0.125)	(0.125)	(0.125)	(0.125)	(0.126)	(0.125)	(0.125)	(0.124)	(0.061)
Time^2	0.114**	0.115**	0.115**	0.131***	0.124***	0.123***	0.129***	0.101**	0.105***
	(0.046)	(0.046)	(0.046)	(0.046)	(0.046)	(0.046)	(0.046)	(0.046)	(0.023)
Time^3	−0.013***	−0.014***	−0.014***	−0.015***	−0.014***	−0.014***	−0.015***	−0.012**	−0.012***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.003)
Observations	656	656	656	656	656	656	656	656	2,680

Note: *p<0.1; **p<0.05; ***p<0.01

Figure 6: Time Series for All Listed Countries

Log BIS liabilities vs. Log IMF cross-border investments

Log BIS (green) vs. Log IMF (blue)

Year