Technical Note on the Second Empirical Anti-Money Laundering Conference

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The aim of this technical note is to summarise the main findings and highlights of research presented at the Second International Conference on Empirical Approaches to Anti-Money Laundering (AML) hosted online by the Central Bank of the Bahamas 27-29 January 2021. More broadly, it also seeks to put these findings in the context of some general debates about money laundering and anti-money laundering among researchers and practitioners.

The global AML regime is currently beset by a paradox. At one level, AML standards set and propagated by the Financial Action Task Force (FATF) have been hugely successful in the breadth and increasing depth of their coverage. Almost every country in the world has committed to implementing the FATF 40 Recommendations. Along with a flag and a national anthem, having a national AML apparatus has become a necessary concomitant of membership in the international community. Over the last 30 years, AML standards have expanded to encompass a wider and wider range of economic activity and political aims (e.g. countering the finance of terrorism and proliferation finance) since the first relatively limited goal of intercepting the proceeds of illegal drugs sales. A huge and still growing compliance industry has sprung up in the private sector to implement AML standards.

Yet for all this progress, there is a lack of evidence concerning the effectiveness of the global AML system in reducing money laundering, or the predicate crimes that generate dirty money in the first place. A lack of evidence for effectiveness is not the same as evidence for ineffectiveness, especially given the obvious difficulties of studying illegal activities that take place in secret. Nevertheless, the relatively small group of researchers studying money laundering and AML have evinced some skepticism that the system generates the results it is intended to.

Given this paradox of the successful global diffusion of AML rules and policies, combined with increasing doubts about the results, the conference comes at a particular timely moment for reflection. It is hoped that the papers, and this summary, will aid both researchers and policy-makers in this process of reflection.

Existing treatments of money laundering and AML have disproportionately been a combination of journalistic exposés, and how-to guides for those implementing AML measures. The FATF and its regional satellites have produced dozens of evaluation reports assessing the degree to which countries adhere to the 40 Recommendations. The papers delivered at the 2021 Bahamas conference are different in being empirical studies that try to get to grips with the areas where our knowledge is thinnest, for example measuring the effectiveness of AML policy.

This note is divided into two unequal parts. The first briefly draws out some common themes across the papers. These include data problems relating to how to measure money laundering, the dangers of extrapolations, and the difficulty of accurately identifying risk, but also the benefits of a greater engagement between researchers and practitioners. The second, longer section focuses more on summarising the individual contributions. These are reviewed in four rough groupings: those primarily focused on the data problem (Dawe, Ferwerda and Reuter,

Keatinge and Moisienko, Littrell, Barone et al.); those looking to better identify risks (Jofre et al., Pauselli, Riccardi, Mold, Findley et al.); those studying key actors (Harvey, Bartolizzi et al., Tsingou, Berry and Gundur); and lastly papers concerned with the intended and unintended effects of AML regulations on other economic outcomes (Case-Ruchala and Nance, Collin et al., Nershi, Gara et al., Agca et al.). In practice many papers relate to two or more themes, and so this grouping is somewhat arbitrary but serves to structure the subsequent discussion.

General Themes

The first theme of the conference was a general dissatisfaction with the availability of data on almost every aspect of the topic of money laundering and AML. In large part this knowledge gap is the main reason why the conference was held in the first place. As one conference attendee has put it, AML policy is almost an 'evidence-free zone' (Halliday, Reuter and Levi 2020). While no doubt inconvenient and frustrating for researchers, the scarcity of data, and our corresponding ignorance about money laundering and the policies designed to stop it, have severe practical consequences. If we don't know how, or even whether, AML policy is affecting money laundering, it is difficult or impossible to reform the system to make it work better.

Evidence and data on illicit, secret activities like money laundering will always be hard to come by for obvious reasons. As Reuter pointed out, even estimates of the size of the best studied criminal market, that for illegal drugs in the United States, produce figures from $50-150 billion per annum. Such variation in the baseline figure means that it is in practice impossible to measure the effect of any policy change or law enforcement intervention on the size of this market. *A fortiori*, if we cannot get a usefully bounded estimate of the US illegal drug trade, there is little chance of getting a useful estimate of the value of money laundered globally. None of the conference papers aimed at contributing to what is now something of a cottage industry of estimating 'Illicit Financial Flows' (for critiques, see Reuter 2012, 2017; Forstater 2018).

The relative indifference of the AML policy establishment, especially the FATF, to collecting data and conducting cost-benefit analyses has been striking, even in countries where such analyses are mandated. There are figures which should be practically possible to collate that are nevertheless not available, such as the total convictions for money laundering or value of criminal assets confiscated. Even at the national level, such figures are usually difficult to determine, and there is nothing close to a credible global measure. The conference featured a presentation from FATF Vice President (and senior Mexican official) Elisa de Anda, on the FATF's emerging strategic review. There are some indications that the FATF will devote more effort to building and encouraging useful international datasets, but the shape and balance of this work has yet to be determined. Researchers on the related topic of international tax evasion and tax avoidance, which are surprisingly little referenced in the conference papers, seem to have gone further in addressing data problems than those studying money laundering (for example, Alstadsaeter et al. 2017, 2019; Johannesen and Zucman 2014; Zucman 2013).

The most acute consequence of the lack of data for researchers and policy-makers alike is the lack of a valid and reliable dependent variable against which to assess the effect of AML interventions. As noted, the most obvious missing information is any idea of the sums of money being laundered globally, or the scope of underlying predicate crime. The FATF wisely gave

up on attempting to come up with such a figure, in some ways showing more intellectual honesty and rigour than some academic researchers.

To the extent that money laundering convictions or sanctions are used as a substitute dependent variable, such totals are a tiny and almost certainly unrepresentative sample of the universe of cases; it is not possible to extrapolate from the small number of money launderers that get caught to the presumably much larger number that do not. In addition, it is not clear if large or small absolute numbers of convictions are good news or bad news. Do low conviction rates indicate a low level of money laundering, or just that a large majority of money launderers are never detected? As a result of these problems, often the default has been to measure outputs (e.g. Suspicious Transaction Reports), rather than outcomes. Yet these outputs are an even more invalid measure of the real problem, and tend to create a bureaucratic process by which means to an end become ends in themselves.

A further related theme is the problem of accurately identifying money laundering risks. There is a strong sense that despite the risk-based approach having become the touch-stone of the regulatory regime, in fact both national authorities and those in the private sector are flying blind here. This uncertainty applies to results of national-level assessments, but also in the process used to generate these assessments. Internationally the problem seems to be that the risks are in the opposite places than official blacklists would suggest, i.e., in FATF member states, and not the unlikely clutch of small developing jurisdictions that conventionally end up on such lists.

Historically and to the present, there has been very little contact or dialogue between AML practitioners and researchers. A common related critique at the conference was the mismatch between the huge amount of time, money and effort that now goes into the global AML system versus the essentially trivial resources devoted to finding out what difference this system makes, if any.

The main points above are almost unrelentingly negative: things that are not working or that we don't know. In part, this explains the disconnect between AML researchers and practitioners, including the FATF. The FATF is already facing growing public doubts about its efficacy; research may provide fuel to further those doubts, while at the same time offering little in terms of solutions to the problems identified. There are two responses to this charge.

First, knowing that something is being done wrongly or that a policy is not working is a valuable contribution in itself; ignorance is expensive. In some cases AML policy-makers seem to be locked into public positions that they themselves do not believe, and owe much more to politics than evidence. Prominent examples are that money laundering harms banks, or that it endangers the stability of the financial system in general. Given the 'emperor's new clothes' quality of such claims, these falsities need to be challenged from the outside. Relatedly, the persistence of 'folk theories' about how money laundering works (e.g., the prevalence and price sensitivity of professional third-party launderers) and anti-money laundering systems function (e.g. AML-based de-risking, or the risk-based approach more generally) needs to be carefully scrutinised (Halliday 2018). The 'myth-busting' function of AML research may be the most valuable one, though this may be frustrating in not being able to specify what does or would work better than the status quo in fighting dirty money.

Second, although there are more negative findings than positive from the conference papers, there are also many positive and practical suggestions for policy-makers. These range from how to better collect and analyse data on money laundering (Celik; Dawe; Littrell; Ferwerda and Reuter), to the radical and innovative policy solutions for drug-related money laundering (Berry and Gundur).

There are also lessons for researchers in the various contributions to the conference in terms of how to address some of the problems above, and advance research on money laundering and anti-money laundering. The first is an honest confrontation with our lack of knowledge on the subject of money laundering and AML. As noted, in some respects the FATF has been better at this than academics. It is difficult to see how making heroic assumptions that we have knowledge, where in fact we don't, advances either science or policy. We do not know how much money is laundered, and are unlikely to ever find out. We generally do not know where the money laundering risks are at the national level, and we have good reason to think they are misidentified at the international level. We know that it is dangerous to extrapolate from the small and unrepresentative samples of money laundering cases that are detected to money launderers in general. We know that outputs of the AML system like Suspicious Transaction Reports are not a useful proxy for either actual money laundering or money laundering risk. Given all this, researchers should start from the limits of our knowledge and build out, not try to assume that we have knowledge that we lack. Studies that ignore these limits are built on very shaky foundations. It is perhaps in this sense that Riccardi makes his comment about criminologists taking back the study of money laundering from economists.

The more ethnographic, case-oriented approaches offer some advantages in getting away from the often heroic data assumptions that define large quantitative studies (for examples of qualitative studies in cognate subjects, see Venkatesh and Levitt 2000; Venkatesh 2006, 2008; Harrington 2016, 2018). Because of the data problems discussed, and derivative problems of causal inference from quantitative studies, there is a strong rationale for taking an in-depth, local approach to researching this issue. In particular, in seeking an answer to the basic nagging doubt (why doesn't AML policy seem to work?), qualitative studies may provide particular insight. For example, if FATF blacklists don't work as we think they do, how do they work? One approach might be to ask those on the receiving end.

Drawing on examples from the tax literature as well as that on AML, some of the strongest quantitative contributions are those that work from new empirical data. Thus Alstadsaetler, Johannesen and Zucman have made creative and effective (if still contestable) contributions to the study of tax evasion through leveraging various leaked material, while Collin has used similar sources to generate new insights on AML (Collin 2019). In various ways several of the conference papers seek to make original use of new data, from bunching in trading patterns, to field experiments, to close analysis of inter-bank transfers.

Turning from general themes and patterns, the summaries below highlight the contributions of individual papers loosely divided into the four groups specified above.

Addressing the Data Problem

Given the lack of good data, what is the official FATF position on data collection and analysis? **Dawe's** presentation looks at Recommendation 33, which requires countries to collect

information on their AML systems. Dawe takes a systematic view of the data gaps many others mention, and squarely addresses the elephant in the room: no one has any idea how much money is laundered globally. As such, it is almost impossible to estimate policy effectiveness, either in an absolute sense, or relative to the direct and (much larger) indirect costs of AML. Akin to Ferwerda and Reuter, Dawe identifies common flaws in the way data is collected and used in international AML reviews and assessments, and suggests a range of practical and helpful reforms for improving and standardising data collection in these exercises.

Ferwerda and Reuter take a searching and skeptical view of the FATF-mandated National Risk Assessment (NRA) process. They focus on assessments from eight rich FATF member countries that might be expected to produce the highest quality analyses (Canada, UK, US, Italy, Singapore, Switzerland, Netherlands and Japan). Given such NRAs are now a universally-mandated FATF requirement, it is sobering that the paper finds they are almost entirely wasted effort. The authors find weaknesses from vague and inconsistent definitions of key terms (notably 'risk'), mismatched and incommensurable evidence, a basic lack of familiarity with professional risk assessment methodology, and an almost complete lack of explanation as to how and why particular information was included or excluded. As a result, the assessments provide little benefit to policy-makers, regulated entities in the private sector or researchers, except perhaps to reinforce fundamental doubts about the AML regime in general. This verdict tends to confirm the findings of other papers (e.g. Findley et al., Riccardi) that despite the risk-based approach supposedly being the foundational principle of the AML system, it is not applied in practice. The authors conclude with a helpful list of suggestions on how to improve the existing unsatisfactory state of affairs, the most important being greater transparency and willingness to learn from previous mistakes in the way assessments are conducted.

Beginning where Dawe and Ferwerda and Reuter's papers conclude, **Littrell's** contribution examines the potential for improving National Risk Assessments via use of 'opportunistic data', using the Bahamas as an example. The paper once again notes the problem of a lack of standardised method for conducting NRAs and the paucity of consistent data. Taking advantage of readily available sources, Littrell's approach helps in narrowing down where the risks are, and perhaps even more importantly, given limited regulatory and investigative resources, where they are not. Real estate is identified as the biggest domestic threat, but for the Bahamas the overwhelming money laundering risk is in the international flows that dwarf the local economy. Refreshingly, the paper is highly attuned to the need for a cost-effective approach to AML compliance in a small jurisdiction with many other demands on the public purse.

Keatinge and Moiseienko's paper covers the much-discussed area of beneficial ownership. They present a state of play with regards to existing policy, and in particular the rise of public central registries of beneficial ownership (Knobel 2020), championed with evangelical zeal by the British government and many NGOs. Informed by the views of their interviewees, the authors suggest a number of policy priorities. These include the necessity of verifying information, however it is obtained; the need to apply standards consistently to avoid loopholes that create new secrecy products (e.g., Scottish Limited Partnerships); ease of access to beneficial ownership information for both domestic and foreign parties; and the use of this information in building new money laundering typologies. These priorities seek to re-orient the debate away from a contest between open registries vs. regulated intermediaries, and more pragmatically towards the quality and use of beneficial ownership information.

Barone, Masciandaro and Schneider aim to quantify the macro relationship between corruption, money laundering and anti-money laundering. They posit the existence of trigger, multiplier and accelerator effects. The first refers to corruption generating a need to launder corruptly-obtained funds, the second is the way laundering cleans funds for subsequent re-investment, and the third covers how corruption in the AML system can facilitate laundering. Using a set of rather demanding assumptions (e.g. that FATF blacklists are an accurate indicator of money laundering risk, and that we have accurate estimates of criminal wealth), the authors calculate and project the figures for the carrying capacity of different economics for illicit wealth far into the future.

Where are the Money Laundering Risks?

An innovative approach to finding money laundering risks is that employed in **Jofre, Bosisio, Riccardi and Guastamacchia's** paper, based on a machine-learning study of corporate structure and beneficial ownership. This takes the records of over 3 million companies from nine European countries included in the Orbis dataset (for related work on corporate structures working from the same dataset, see Phillips et al. 2021). The paper concentrates less on the 'who' of the individual beneficial owner and more on the 'how' of the corporate structure. The study then looks at a sample of companies and beneficial owners subject to official sanctions or enforcement actions, and finds that this sample is more likely to exhibit the risky corporate structures, i.e. those that are overly complex, missing information, or connected with risky jurisdictions.

Pauselli's paper is once again devoted to sharpening our ability to locate money laundering risk. It concentrates on 192 Italian non-banking intermediaries (Italy is by far the best-studied country of the 2021 conference). Data is drawn from reports of all transactions over €15,000. Various risk categories are used, but information explaining and justifying these risk factors themselves is quite sparse.

As with the Jofre et al. paper, there is something of a problem of regress, in that judgements of where the risks are assume that FATF judgments of risky jurisdictions are accurate. This is a highly debatable point, with some studies indicating that AML country risk indicators are completely at odds with each other (Littrell and O'Brien 2020), or that they are actually negatively related to where the real money laundering risks are (Riccardi; Collin 2019; Findley et al. 2014). Some FATF listings are obviously nonsensical, as even a Wikipedia search would suggest, e.g. the uninhabited Bouvet Island. The EU AML and tax lists are even less credible, based as they are on an explicit double standard of excluding EU member-states. Finally, returning to a general theme raised earlier, almost all sources agree that the number of financial criminals subject to punishment is a very small and unrepresentative sample of the whole, and hence there are serious dangers in seeking to extrapolate from the former to the latter.

A common-sense yet little discussed draw-back of various global blacklists and country-by-country risk rankings is that a particular jurisdiction may pose a major risk to one country (say a close neighbour with strong economic ties and close cultural affinities), and yet pose almost no risk to another (a much more distant country). Although based on sophisticated data and analysis, **Riccardi's** paper is firmly grounded on this intuitive observation. The idea that different countries face different risks in this way, that risk is inherently relational, might be

thought to be obvious. But as Riccardi's paper points out, it is very little appreciated beyond the earlier gravity models (e.g.Walker and Unger 2009). For example, the EU decided that countries like Mongolia and Namibia pose serious tax avoidance threats to its member states, but not the Netherlands, Ireland or Luxembourg. Riccardi rightly dismisses such blacklists as invalid; he finds they actually have a negative correlation with his bespoke measure of money laundering risk, confirming an earlier paper by Collin on 'the money laundering paradox': countries with high FATF ratings tend to hold more criminal wealth than those with low ratings (Collin 2020).

In arguing persuasively for a more bespoke approach, he looks at the example of Italy, assessing the risk posed by other jurisdictions based on their proximity, opacity, and security (e.g. property rights). These are measured against a database of 2818 individuals in Italy arrested, prosecuted or convicted of money laundering 2014-20, and the countries mentioned in connection with these individuals' criminal activities.

Littrell rightly observes that Riccardi's paper was one of the strongest contributions to the conference in the way it combines sophisticated use of data with a plausible measure of money laundering outcomes. This is undoubtedly true, but it is also symptomatic of the limits of our knowledge that even given these strengths, there are potential challenges to the inferences made. Perhaps Italian investigators find it easier to detect, investigate and prosecute foreign money laundering cases that are close by than those in far-distant countries? One doesn't have to think too hard as to why the Italian AML system might work better in San Marino than Dubai or Hong Kong. And although the measure of money laundering risk is perhaps the best around, we still face the problem that the sample of those found to be engaged in cross-border money laundering are a small and unrepresentative sample of the total population of such criminals. What if those launderers who keep it local are more likely to be caught than those that launder far from home?

If banks signal risk to the authorities in their reporting patterns, is it possible that criminals send signals to each other in the numerical value of their transactions? Taking a practitioner's perspective, and in line with the familiar 'arms race' metaphor, **Mold** states that criminals are highly adaptive in the ways that they launder money, usually outpacing banks' efforts to detect their laundering. In some ways this framing of the situation is actually something of a back-handed compliment with regards to the effectiveness of the AML system. If true, money laundering techniques are constantly being defeated, and so launderers are under relentless pressure to innovate. Thanks to our general ignorance, it's hard to adjudicate between this scenario and another that says the effectiveness of the AML system is so low that criminals can keep on using the same techniques, like cash and shell companies, decade in and decade out. Mold believes that one of the latest illicit financial innovations sees criminals communicate through the penny values of large inter-bank wire transfers. She argues that the recurrence of certain penny values cannot be explained by random variation or exchange rate conversions.

Studying Key Actors

The papers grouped together here look at key actors in money laundering and AML, beginning with the state AML apparatus and Financial Intelligence Units, then the private sector compliance industry, and last but certainly not least the money launderers themselves.

Harvey's presentation (part of a bigger project) is focused on the AML system in Nigeria, but is also relevant for how corruption funds flow from Nigeria to be laundered in the UK. This is a focused national look at the basic recurring question of AML research: why don't the rules on the book seem to work in practice? Despite information sharing being a watch-word of combatting financial crime, Harvey finds that even among different public sector agencies in the same country, information is a jealously guarded commodity. More broadly, the implication of the study supports the common conclusion that in the developing world, AML policy is not done to counter money laundering, but as a Potemkin village arrangement to placate the FATF and other powerful outsiders.

Bartolizzi, Gara, Marchetti and Masciandaro's paper further investigates the nature of the regulators, in this case Financial Intelligence Units (FIUs). The paper first sets out the various types of FIUs, generally dichotomised between administrative FIUs, which collect and process financial intelligence, and law enforcement bodies which also investigate on the basis of this intelligence. This classification is based on a dataset of 71 FIUs, coded on the basis of their financial power, law enforcement prerogatives, independence and accountability. FIUs with a wider range of powers are associated with wealthier economies, higher-quality governance, greater financial transparency and membership of more international organisations. The paper does not seek to assess whether the nature of the FIUs has implications for their control of money laundering.

Tsingou's careful and detailed ethnographic paper (again part of a bigger project) takes an insider look at those providing the 'last mile' of AML policy: the growing cadre of compliance workers in private financial institutions. For all the expansion of the AML apparatus in the public sector, this growth has been dwarfed by that in banking and other financial services. According to Tsingou, what has arisen is not just a new compliance industry, but also a new profession, organised around a standardised core body of knowledge and practice, and complete with its own accreditation, association and conference circuits. While the underlying driver has been public policy, the proximate mechanism has been the spectacular bootstrapping success of the Association of Certified Anti-Money Laundering Specialists (ACAMS), which created something out of nothing by self-certifying its own qualification *de novo*.

The importance of these new 'masters of compliance' (to use Tsingou's phrase) is that, along with the few companies that provide the standard software packages, they increasingly define what AML practice is. Attempts to finds out how, and indeed whether, policy in this area works practically, and how much it costs, might now perhaps be better directed at ACAMS and its members than the FATF and government officials.

The parties most often missing from studies of money laundering are the money launderers themselves (Levi and Reuter 2006). **Berry and Gundur's** paper is a fascinating and valuable exception in being based on interviews with small- and medium-sized drug-dealers (see also Levitt and Venkatesh 2000; Venkatesh 2008). The authors demonstrate how small-scale qualitative studies can produce big conclusions: most criminals do not need to launder their illicit income in any meaningful sense, because it can simply be spent as cash in the retail sector (Riccardi's findings about how the overwhelmingly local nature of most laundering in countries like Italy, Spain the Netherlands and Finland support this point, see also Reuter 1983).

If this is true for rich and increasingly cashless economies like the US and Britain, it is almost

certainly true of much of the rest of the world. To this extent much of the AML policy edifice and the basic notion of 'taking the profit out of crime' is built on false premises, or at least targets only a small minority of instances of large-scale money laundering. The authors' claim that seizing criminals' assets may lead to more crime rather than less, as criminals need to raise money to pay back new loans from back-streets lenders, is intriguing and counter-intuitive.

Even the mid-level criminal entrepreneurs engage in relatively simple, opportunistic and yet effective laundering strategies that seem quite alien from those AML policies are designed to counter, for example voluntarily declaring criminal income to tax authorities. In this sense the disconnect between what bodies like the FATF and ACAMS think money launderers are doing, versus what they actually seem to be doing, is stark. This might suggest why the vast majority of drug-dealers have been able to continue to ply their trade despite 30 years of AML, which had the overriding original purpose of shutting them down. Finally, this paper also suggests iconoclastic policy responses, like amnestying criminals so they can exit from illicit to licit businesses, in effect de-criminalised laundering.

Effects of AML Regulation

One instrument used to foster the international spread of and adherence to the FATF's standards has been the prospect of publicly listing jurisdictions whose performance the FATF sees as lacking. There have been a variety of such schemes, from 2000 the Non-Co-operative Countries and Territories initiative, and then after a break, a system of grey/blacklisting by the FATF's International Co-operation Review Group. In each case listing has been used as a sanction, or prospectively as a threat. *Prima facie*, listing seems to be highly effective, in that most countries have worked hard to avoid being placed on such lists, and those that have been included have usually worked hard to win their removal. However, papers by Case-Ruchala and Nance and Collin, Cook, Soramaki and Wadwa take issue with earlier work asserting that such lists achieved their effect through damaging the targeted jurisdiction's international economic linkages.

One example of how this 'market mechanism' by which listing translated into concrete financial loss might work was a paper presented at the first conference by Julia Morse (2020). This argued that trade finance was the Achilles heel vulnerable to listing, and hence the point of leverage through which listing achieved its coercive effect. Taking an expanded sample of countries, however, **Case-Ruchala and Nance** found that listing had no significant effect on targeted countries' access to trade financing. In critiquing the more general thesis that listing works through economic damage, Case-Ruchala and Nance also tested for effects on long- and short-term debt issuance and portfolio investment. On each measure the paper failed to find that listing had a significant effect.

A joint paper by **Collin, Cook, Saromaki and Wadwa** again looks at the effects of FATF listing, this time via a difference-in-difference analysis of SWIFT cross-border banking data. Once again, the story is largely one of dogs that don't bark (contradicting earlier research by the same authors from 2016), with no discernible de-risking effect whereby listed jurisdictions suffer reduced transactions. The only partial exception to this negative finding is that listed jurisdictions are somewhat more likely to experience an out-flow of funds, which may represent a negative reputational effect

These two papers thus add complexity to an on-going debate about whether blacklists impose concrete economic costs on jurisdictions targeted in this manner. Jointly they challenge not only the previous year's paper by Morse, but also earlier work by Donato Masciandaro (2013). In some sense, then, the jury is still out on blacklists, especially as policy-makers on the receiving end are nearly unanimous that such lists do indeed constitute a threat.

As work at the 2020 conference showed, banks themselves have often tended to publicly blame the cost of AML and/or Countering the Finance of Terrorism regulations in explaining their selective withdrawal from developing countries. On the evidence above, however, AML policy seems not to be responsible for most de-risking. Yet in some ways the original puzzle remains unanswered: most governments seem to be worried about the prospect of ending up on one of the FATF's negative lists, and those that are listed strenuously reform in order to be rehabilitated. The two papers critique the mechanism said to make lists effective, but they largely do not dispute that the lists are consequential. How else might these lists create compliance pressure?

If the business of FIUs is to process reports from the private sector, what affects the frequency of such reports? Continuing the Italian theme, **Gara, Manaresi, Marchetti and Marinucci** examine the impact of on-site visits from regulators on the propensity of Italian banks to lodge Suspicious Transaction Reports 2012-13. Here the policy challenge is that both under- and over-reporting are potential problems, the former because banks may be missing or turning a blind eye to dirty money, the latter because they may be 'crying wolf', making many low-quality false-positive reports. There is a concern that on-site inspections and enforcement actions by regulators may fix the first problem while creating or worsening the second.

Using a difference-in-difference approach, the authors show not only that regulatory attention (predictably) increases the number of reports subsequently submitted by banks, but also that it does not diminish their quality, measured in terms of the average risk rating given to each report by FIU (banks made both more low-risk and high-risk reports after inspection and enforcement action). While good news in terms of the proximate problem of 'defensive reporting' and 'junk reports', whether this is good news in terms of accurately identifying money laundering risk is less clear.

Findley, Nielson and Sharman presented an updated version of their global field experiment testing whether Corporate Service Providers and banks do in fact identify beneficial owners in setting up shell companies and associated corporate bank accounts, as international standards say they should. The study is massive and long-running, based on around 40,000 email solicitations over a decade in this Banking Bad exercise, as well as the previous related Global Shell Games study. In essence, the study is a test not only of the overall level of compliance, but in particular of the risk-based approach that is supposed to be a fundamental principle of international AML policy. Are risky customers in fact subject to extra scrutiny? Do they have a harder time setting up shell companies and corporate bank accounts compared with low-risk customers?

The experiment suggests not, with results demonstrating an extraordinary insensitivity whereby customers with wildly varying risk-profiles receive basically the same treatment from CSPs and especially banks. The conclusions for overall compliance are less clear-cut, with almost no banks and few CSPs willing to provide their services without any proof of identity

at all, but they often mistakenly focused their Know Your Customer efforts on the company representative, not the beneficial owner. Offshore providers were more likely to handle risk by following the rules in vetting customers, while onshore providers were more likely not to respond or to refuse business.

Nershi brings welcome rigour to the study of cryptocurrencies, a subject on which hype routinely out-runs evidence. Using an instrumental variable design, the paper focuses on the effects of reporting thresholds on trading from crypto to regular currencies taking place in exchanges. Nershi shows that on some exchanges, trades tend to bunch just below the Customer Due Diligence reporting requirements. This finding suggests that AML regulations are consequential, but not in the way authorities would hope, in that they prompt some individuals to structure their trades so as to evade reporting requirements. It also shows cross-national variation, once more in a way that confounds the self-perceptions of OECD countries as paragons of virtue in this domain. The paper is especially valuable in reorienting attention from national laws to the actions and inactions of those private entities (in this case cryptocurrency exchanges) charged with actually applying standards.

Agca, Slutzky and Zeume aim to test the effect of AML enforcement on concentration on the banking industry, and thence to the real US economy. They do so by zeroing in on so-called High Intensity Drug Trafficking Areas in the United States (e.g. counties along the border with Mexico). They look for change from 2012, said to be the beginning of a new era of very large fines against banks for AML non-compliance. The empirical puzzle to be explained is that, first, big banks expanded their share of the market in such trafficking-prone areas relative to the rest of the US, and, second, that these areas saw better economic outcomes, such as real estate price growth.

The authors explain these outcomes first by the differential effects of AML compliance costs: big banks can better internalise and absorb these costs, and because they are ‘too big to jail’, they are less vulnerable to sanctions for compliance failures than smaller banks. In this sense there are some parallels with the work on ‘de-risking’, though here the story is domestic rather than international, and the effect of AML regulation is said to benefit rather than harm the wider economy.

Conclusion

The second Bahamas AML conference, like the first, demonstrated the benefits of creating better connections among the world’s practitioner and academic researchers. This is the case even though the papers commonly raised more questions than they answered, and despite the recurrent theme of knowledge gaps and insufficient data. The Central Bank of The Bahamas’ commitment to continue this conference series in 2022 and beyond should continue to strengthen the world’s empirical AML research community.

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[Note: conference papers in italics are from the 2021 conference and are available at: https://bahamasamlconference.com/event].

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