Information Transmission in Finance:
Essays on Commodity Markets, Sustainable Investing, and Social Networks

Information Transmission in Finance:
Essays on Commodity Markets, Sustainable Investing, and Social Networks

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In memory of my aunt and uncle

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François Rabelais, 1532, *The Horrible and Terrifying Deeds and Words of the Very Renowned Pantagruel King of the Dipsodes, Son of the Great Giant Gargantua*

“The bird a nest, the spider a web, man friendship.”

William Blake, 1794, *The Marriage of Heaven and Hell*

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Chapter 1

Introduction

The nature of information transmission within economic systems and financial markets is an important determinant of human decision-making and economic outcomes (e.g., Hayek (1945), Stiglitz (2000), Hirshleifer (2020)). Information is often dispersed in society and aggregated into various forms such as prices, ratings, and socially transmitted ideas. All these forms of information are then used as decision-making inputs by economic agents.

For example, an increase in the price of a commodity might tell thousands of producers that it is time to use a cheaper alternative as a production input. Alternatively, the same price increase might be due to the strengthening of the global economy and, therefore, a signal that production should increase to meet rising global demand. Similarly, an idea that travels through social networks might lead investors to buy a certain stock or influence the decision of managers to change corporate policies. Studying how information is transmitted and the content of that information in different settings is therefore often an important step to understand economic phenomena.

This dissertation consists of three empirical essays that study information transmission in financial markets and the corporate world. In Chapter 2 I study the information content of commodity futures returns with respect to stock markets around the world. Chapter 3 provides a systematic analysis of whether or not ESG ratings have information about future stock returns. In Chapter 4 I examine how information about corporate social responsibility practices spreads across the business world through the social networks of corporate executives and directors. Chapter 5 consists of concluding remarks.

The information content of commodity futures markets

Commodity markets are the backbone of the global economy. Many countries are highly exposed to commodity trade. This is especially so for the least developed countries, 85% of which have over 60% of their revenues tied to commodity trade (e.g, van der Ploeg and Venables (2012)). According to our calculations, the level of dependence on commodity trade is also striking in some developed countries. For example, primary commodity trade accounts for 30% of the GDP of countries such as Australia, Canada, and Norway.

Price volatility in commodity markets can thus have devastating consequences for the economies and financial markets of many countries around the world, often the ones that are least able to cope with economic distress (e.g., Morduch (1995)). The recent 2007-2008 world food crisis illustrates. World rice prices tripled in six months, with prices of other commodities such as wheat, corn, and soybeans also surging to uncomfortable heights (e.g., Dawe (2012)). Political instability and violent riots fueled by hunger and fear spread through dozens of countries in the developing world. At least hundreds of injured and dozens of deaths were reported.

Despite the economic importance of commodity markets for the world economy, we know little about the relation between commodity and stock markets around the world. This is an important gap, especially in light of the concern that speculation in commodity futures markets might feed into the real economy by affecting commodity spot prices and firms' production decisions. The case in point is the recent model of Sockin and Xiong (2015) which provides a framework under which the surge in commodity prices in 2007-2008 may have been partially driven by distorted price signals originating from commodity futures markets. Crucially, the central feature of this model is the assumption that commodities have information about the state of the global economy. It is thus important to examine the empirical foundations of this feature. In Chapter 2 we try to do so by studying the information content of six commodity sectors with respect to the stock markets and macroeconomic fundamentals of 70 countries between 1979 and 2016.

We find that stock market predictability from commodity futures markets is a pervasive global phenomenon that affects developed and emerging countries alike. Commodity futures returns have predictive power for country-level stock returns in 59 of the 70 countries in our sample, with most countries being predicted by several commodity sectors. Macroeconomic fundamentals are also related to past commodity futures returns in 62 countries.

Strikingly, our analysis of the economic channels of information transmission suggests that countries' dependence on commodity trade plays a limited role in explaining why some countries are predicted by certain commodity sectors and others are not. This suggests that the global information flows from commodity to stock markets cannot be explained by standard trade dependence arguments.

We find much stronger support for the view that predictability is tied to the ability of commodities to predict inflation rates, even after taking into account the effects of trade dependence and several other country characteristics. This is in line with recent theories which argue that commodities may be able to aggregate dispersed information about the state of the global economy in a complex manner that goes beyond trade dependence mechanisms (e.g., Sockin and Xiong (2015)).

We are also able to dispel the conventional wisdom that the information flow from commodity to stock markets is all about the energy sector and large countries that account for a disproportionate amount of the world's GDP and commodity demand.

To this end we develop novel measures that quantify the economic significance of cross-market information flows. We find that these information flows are evenly distributed across countries and commodity sectors. Agriculture and industrial metals sectors account for half of the global stock market variation that is predicted by commodity futures returns, ahead of the energy sector which accounts for 20%. Strikingly, no single country captures more than a relatively small share of the worldwide information flows from commodity to stock markets.

Overall, our results suggest that commodity futures markets play a unique information discovery role in financial markets by aggregating dispersed information about global stock markets and macroeconomic fundamentals.

Drawing up the bill: does sustainable investing affect stock returns around the world?

In Chapter 3 we conduct an extensive examination of the stock market performance of sustainable investing, also known as ESG ("Environmental, Social, and Governance") investing. We do so by studying whether or not ESG ratings have information about future stock returns. ESG ratings, which measure how well firms incorporate ESG considerations in their business activities, are a relevant metric because of their widespread use by practitioners and researchers.

The rise of sustainable investing is one of the most impressive trends in financial markets in recent memory. The United Nations' Principles for Responsible Investment (PRI), a network of institutional investors committed to incorporating ESG considerations in their decision-making, has over 3,000 signatories as of 2021. This is a threefold increase since 2013. Together they manage assets worth north of US $100 trillion. Notably, the trend is not yet slowing down. Total assets under management in ESG funds reached US $1.7 trillion in 2020, a 50% increase over the previous year (Mooney and Mathurin (2021)). According to The Economist (2021), two ESG investment funds are created each day as of 2021.

A common selling point for practitioners is that ESG investing is a royal road to higher risk-adjusted returns. This, however, is at odds with recent theoretical models which predict that, over the long-term and under relatively mild conditions, sustainable investing should not outperform on a risk-adjusted basis (e.g., Pedersen, Fitzgibbons and Pomorski (2020), Pastor, Stambaugh and Taylor (2020)). The main reasons for these theoretical predictions are that (i) sustainability-minded investors may be willing to accept lower returns to hold high ESG stocks, and (ii) sustainable investing can only deliver positive abnormal returns over the long-run if enough market inefficiencies persist over time.

Not surprisingly, governments around the world worry about greenwashing and its potentially negative effects on investment income and future pensions. These worries are worth more than words and have led to policy action. Examples of legislation aimed at tightening ESG disclosure standards include the 2020 amendments to the Employment Retirement Income Security Act of 1974 in the US, the Sustainable Investing Disclosure Regulations (SFDR) in the EU, and the Financial Conduct Authority's (FCA) recently proposed rules to tighten climate risk reporting requirements in the UK.

And yet, for all the hype and scrutiny sustainable investing gets, we still do not know whether it creates or destroys value - there are thousands of empirical studies on the topic with a wide range of conflicting findings.

Part of the problem is that these studies are very heterogeneous in terms of quality, methodology, time period studied, measures of sustainability used, and countries and industries covered. Recent meta-analyses on the topic (e.g., Friede, Busch and Bassen (2015)) have also been received with skepticism by at least some academics because they do not account for the heterogeneity of the underlying studies in a convincing manner (e.g., Dimson, Marsh and Staunton (2020), Matos (2020)).

Because of this, there is a need for a rigorous large-scale study of sustainable investing that explores how the relation between ESG ratings and future stock returns varies geographically, across industries, over time, and across ESG databases. We try to fill this gap in Chapter 3 by conducting a comprehensive examination of sustainable investing using a dataset covering various sustainability measures for 9,253 stocks in 46 countries over the last two decades. To our knowledge, this is the largest dataset assembled to date to study the stock market performance of sustainable investing.

Our main finding is that ESG ratings have little information about future stock returns. This holds when using a global sample of stocks, within most geographic regions, within sectors of economic activity, using different ESG ratings and combinations of those ratings, and when looking at different time periods. The main exception to this finding is that we find some evidence that the environmental and social components of ESG ratings positively predict cross-sectional variation in stock returns in emerging countries. We discuss several explanations for what might drive this finding in Chapter 3.

On the bright side, our results suggest that it may have been possible to pursue sustainable investment strategies in the last two decades without sacrificing financial returns. Our findings also suggest that sustainable assets may not suffer from widespread overvaluations - a major concern of investors and policymakers.

We caution, however, that these results are based on past performance and in Chapter 3 we discuss reasons for why sustainable investing might underperform going forward. In addition, despite our best efforts to conduct a comprehensive analysis, we acknowledge that there are ESG metrics and investment strategies that we do not consider. Nonetheless, our results provide evidence that it may be prudent to scrutinize claims that sustainable investing outperforms before embracing such claims.

On the dark side, even though our analysis is based on realized returns and not expected returns, our findings cast some doubt on the view that sustainable investing has so far been effective in reducing the cost of equity of sustainable firms. This suggests that sustainable investing may not yet have succeeded in providing incentives for firms to internalize the negative environmental and social impacts of their actions. While we hope that the future is brighter than the past in this regard, our results also serve as a reminder of the simple observation that our hopes do not always match reality. As such, our results do not deny the potential virtues of sustainable investing. But they do provide some food for thought for those who are confident that sustainable investing is a reliable and adequate substitute for government policy.

Social networks and corporate social responsibility

Chapter 4 complements Chapter 3 by studying sustainability-related information flows that are transmitted through firms' social networks rather than through financial markets. I refer to the efforts that firms make to integrate ESG considerations in their business models as corporate social responsibility (CSR) in Chapter 4. I do so to stress that Chapter 4 deals with sustainability from the perspective of the corporation – as opposed to Chapter 3 which deals with sustainability from an investment perspective. Nevertheless, I acknowledge that the terms CSR and ESG are often used in practice as synonyms.

CSR, like sustainable investing, is a source of fierce debate. At the heart of this debate is the fundamental question of whether firms exist only to maximize profits or also to improve the welfare of stakeholders, such as employees, communities, and the broader society.

The view that the welfare of stakeholders should be a major business objective has natural appeal, with some authors even arguing that well-governed firms can often increase firm value by being socially responsible (e.g., Flammer (2015a), Edmans (2020)). This is sometimes referred to as the *good governance* view of CSR (e.g., Ferrell, Liang and Renneboog (2016)). The shareholder value maximization paradigm, however, is a bedrock of modern capitalism that has arguably played its part in bringing about unprecedented economic growth in historical terms. As such, there is a sensible concern that a paradigm shift towards stakeholder capitalism can do more harm than good (e.g., Fama (2021)).

A particularly prominent concern that divides academic and public opinion is the possibility that stakeholder capitalism may lead to widespread managerial entrenchment - the so-called *agency view* of CSR. For example, managers might divert resources from value creating projects to engage in corporate philanthropy and maximize their own well-being and career prospects at shareholders' expense (e.g., Masulis and Reza (2014)). Related concerns include the possibility that (i) multi-stakeholder objective functions might make it hard to monitor and discipline managers, (ii) the interests of some stakeholders are arbitrarily prioritized over the interests of other stakeholders (e.g., Bebchuk and Tallarita (2020)), and (iii) a shift towards stakeholder capitalism might give unelected managers the power to decide on issues that are under the purview of democratically elected officials (e.g., Rajan (2020)).

This debate was sparked even further by recent events and ideas that have raised awareness about the negative social and environmental impact that firms may have on society. Examples include the discovery of the deaths of despair phenomenon (e.g., Case and Deaton (2020)), black lives matter, the #MeToo movement, the Greta Thunberg phenomenon, and the concern that shareholder-centric capitalism is incompatible with strong democratic institutions (e.g., Piketty (2020)) and the "common good" (e.g., Sandel (2020)).

When it comes to the debate between the good governance and agency views of CSR, the truth is perhaps somewhere in the middle - complex and context-dependent. As a consequence, there is deep interest in understanding what drives firms' CSR practices, how CSR decisions come about, and in which situations CSR is consistent with good corporate governance versus agency mechanisms.

Chapter 4 aims to shed some light on these questions by studying how CSR practices are transmitted across firms through the networks of their executives and directors. This is an interesting setting for two reasons. First, there are very good reasons on both sides of the good governance-agency debate for why social network effects in CSR could arise. This makes for a fair race between the two explanations. Second, it allows us to study a novel, specific, and well-identified mechanism through which CSR decisions may come about. This is particularly relevant from the perspective of understanding the role played by the board of directors in shaping CSR - a topic we know relatively little about despite the fact that boards and CSR are both important topics subject to substantial government regulations (e.g., Adams (2017), Roe et al. (2020)).

In the context of social network effects in CSR, the good governance view is that firms can use the social networks of their executives and directors to obtain information about how to optimally design CSR projects and get a competitive advantage over industry rivals.

According to the agency view of CSR, however, social networks are fertile ground for executives and directors to use CSR to maximize their own private well-being at the expense of profit creation. First, business leaders may internalize their social peers' CSR ideals through mechanisms of social interaction and persuasion (e.g., Akerlof and Kranton (2000)). If so, they might decide to act according to those ideals even if that is not in the best interest of shareholders and even some stakeholders.

Second, business leaders may choose to behave similarly to their social peers to get peer esteem and advance their careers (e.g., Bénabou and Tirole (2011a), Levit and Malenko (2016)). To illustrate, take the example of a director who is not inclined to support environmentally-friendly firm policies. If the predominant belief in her social network is that firms should support those policies, what is this director to do? One option would be to ignore her social peers' beliefs and push for policies that she believes are in the best interest of shareholders. The reputation of opposing environmentally-friendly positions, however, would be frowned upon by her social peers who could then be less inclined to have her as a fellow board member or golf companion. Conformity might be an easier option.

Using a rich dataset covering various types of social connections between more than 80,000 top executives and directors of US firms between 2001 and 2016, I provide evidence that CSR practices spread through the social networks of firms' directors. These social network effects are economically large. When firms' social peers increase their CSR performance by one standard deviation, the average firm responds by increasing its own CSR by 16%.

Remarkably, these social network effects in CSR are not widespread across all types of firms. Rather, they are only present for firms that have three specific characteristics. First, these are firms pursuing product differentiation strategies for which CSR is more likely to add value. This suggests that social network effects occur when firms can benefit from learning from their social peers.

Second, these are firms that are strategically positioned in the corporate social network to obtain valuable information. This suggests that social network effects occur when firms are able to learn.

Third, these are firms in which the incentives of managers and shareholders tend to be more aligned as measured by several corporate governance metrics, such as the fraction of independent directors on the board and the CEO pay-performance sensitivity. This suggests that social network effects occur when there are good governance mechanisms in place to ensure that managers working in firms that can learn and that benefit from learning do indeed try to learn - as opposed to trying to benefit themselves at the expense of shareholders.

In sum, social transmission of information through social networks seems to shape CSR decision-making in a way that is consistent with the good governance view of CSR. Assuming that the CSR practices captured by my CSR measures increase the welfare of stakeholders, this social transmission of information generates a social multiplier in CSR that may amplify the positive externalities of CSR on society.

Furthermore, under the plausible assumptions that (i) social network effects in CSR are consistent with good governance and benefit stakeholders, and (ii) firms do not internalize the positive impact of their CSR decisions on the CSR decisions of their social peers, my results suggest that there may be underinvestment at the society level in at least some forms of CSR.

A possible implication is that some firms might choose not to invest in CSR because they lack social network access to valuable information about CSR. From this perspective, my results provide some motivation for the CSR interventions in small and medium enterprises conducted by the United Nations Industrial Development Organization (UNIDO). One of the key goals of these interventions is to inform firms about the potential benefits of CSR practices.

A word of caution is warranted, however. My results do not imply that CSR is never a manifestation of agency problems. The results simply indicate that this is unlikely to be the dominant reason behind social network effects in CSR. My results also do not suggest that CSR is a perfect substitute for government regulations. Nonetheless, my findings suggest that firms can to some extent pursue socially useful CSR projects without compromising on good governance. Given the ample scope for agency motives to give rise to social network effects in CSR, this is a most hopeful finding.

Declaration of contributions

In this section I declare my contributions to the different chapters that comprise this dissertation and acknowledge the contribution of others.

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Chapter 2

The Information Content of Commodity Futures Markets

We study the information content of commodity futures returns with respect to stock market returns around the world, using an extensive dataset covering 70 countries and six commodity sectors over the course of a sample period between 1979 and 2016. To the best of our knowledge, we are the first to show that the information flow from commodity to stock markets is a pervasive global phenomenon and that the information content of commodity sector returns extends well beyond countries' dependence on commodity trade. Overall, our findings are consistent with the idea that commodity markets play an important role in aggregating dispersed global information.

Commodities are a major source of income for many countries around the world. Proceeds from trading in primary commodities amount to about 7% of the world GDP (van der Ploeg and Venables, 2012). Though this dependence is more acute in emerging countries, developed countries are also highly exposed to commodities. For instance, up to 30% of the GDP in countries like Australia, Canada and Norway is tied to the trade of primary commodities.[1]

In spite of this, very little is known about the relation between various commodity sectors and stock markets around the world. Previous literature has tended to focus on individual commodities, in particular oil, and on developed countries.[2] We aim to fill this gap with our comprehensive analysis of a broad set of countries and commodity sector indices over an extended period of time.

In theory, futures prices may convey information that is relevant to stock markets via a commodity trade dependence channel and an information channel that is dependent upon the informativeness of commodity prices. Commodity price changes can be seen as terms of trade shocks for commodity dependent countries (Chen, Rogoff and Rossi, 2010). Indeed, given that some countries rely heavily on trading commodities, commodity price fluctuations are likely to affect export-dependent countries in terms of revenues and import-dependent countries in terms of costs (Classens and Duncan, 1994). In addition, countries are also indirectly exposed to trade dependence on commodities by way of economic linkages with commodity dependent countries. For instance, bilateral trade and financial linkages are known to affect business cycle synchronization and lead to economic spillovers (e.g. Frankel and Rose (1998), Kalemli-Ozcan, Papaioannou and Peydró (2013), Cesa-Bianchi, Imbs and Saleheen (2018)), which is reflected in stock return predictability across trade-linked countries (Rizova, 2010). According to this view, commodity prices may contain information that is relevant to a country's stock market either because that country is dependent on commodity trade or because it is economically linked to countries that are.

[1] Source: Authors calculations based on UNCTADstat data from 1995 to 2016.
[2] See, e.g., Driesprong, Jacobsen and Maat (2008), Kilian (2009, 2014), Hu and Xiong (2013), and Jacobsen, Marshall and Visaltanachoti (2018).

In addition to this trade dependence channel, previous research has shown that commodity futures markets are also a valuable source of information about the strength of the global economy, as they aggregate dispersed information about commodity demand and supply.[3] As such, the information content of commodity prices should extend well beyond countries' dependence on commodity trade (e.g. Hu and Xiong (2013), Sockin and Xiong (2015)). Accordingly, the ability of commodity markets to predict stock markets should also depend on the extent to which a commodity can convey information about a country's macroeconomic fundamentals. While these two channels are not mutually exclusive, it is thus far unclear as to which one is dominant with respect to most countries in the world economy and whether or not this varies across commodity sectors.

We find that in 59 of the 70 countries in our sample, country stock market returns are predicted by the past commodity futures returns of at least one commodity sector. The majority of those stock market returns are, in fact, predicted by two to four commodity sectors. This is true for both emerging markets and developed countries. Furthermore, all commodity sectors predict a wide range of countries in our sample. The Energy, Industrial Metals, and Livestock & Meats sectors predict the highest percentage of countries, ranging between 33 and 44% of all the countries in our sample, while the Precious Metals and Agriculture sectors predict around 20-25%. These results remain robust even when controlling for known stock market predictors. Hence, from a statistical significance standpoint, all sectors contain information that is relevant to stock markets around the world.

[3] For example, Samuelson (1965), Black (1976), Danthine (1978), Bray (1981), Barsky and Kilian (2002, 2004), Sockin and Xiong (2015), and Brogaard, Ringgenberg and Sovich (2018) argue that futures markets facilitate aggregation of information. However, Stein (1987), Sockin and Xiong (2015), and Brogaard, Ringgenberg and Sovich (2018) show that, under certain circumstances, the trading in futures markets may also diminish the extent to which futures prices are informative.

However, it is not only that commodity markets predict stock markets in many countries, it is also that the economic magnitude of this predictability is large and, for most countries, several commodity sectors contain important economic information that is not subsumed by the others. In fact, when looking within each country, we find that there are few countries for which the economic magnitude of predictability is dominated by a single sector. This is surprising given that many countries are disproportionately more dependent on trade in some commodity sectors than in others. We also show that the economic magnitude of predictability is not concentrated in the largest economies; rather it is spread evenly across the globe. For example, while the US and China have accounted for more than 30% of the world GDP in the recent past, each of them accounts for less than 7% of the global stock market variation that is predicted by commodity markets.

Furthermore, we decompose the variation in stock market returns around the world that is predicted by commodity futures markets into commodity sector shares. We find the Industrial Metals, Agriculture and Energy sectors to be the most important economically, capturing 28%, 25% and 20% of the global stock market response, respectively, when we simultaneously shock all commodity sectors in all countries. However, the shares of the remaining two sectors are still large at 11% for Precious Metals and 16% for Livestock & Meats. The fact that the shares of different sectors are so comparable is very surprising, as it is not consistent with the importance of these sectors in terms of global production and trade. For example, the 2018 world-production weights of the S&P GSCI index allocate 59% to Energy, 18% to Agriculture, 11% to Industrial Metals, 8% to Livestock and 5% to Precious Metals.[4] Furthermore, we find that the predictability of stock market returns on the basis of commodity

[4]S&P Dow Jones Indices press release from November 9, 2017 available at www.spdji.com.

returns is not strongly related to the dependence of countries' GDP on trade in a given commodity sector, as we see similar evidence for the presence of predictability for countries with high and low export and import dependence on our commodity sectors. Therefore, commodity sector returns must convey information that extends beyond production and trade dependence and that is relevant to stock markets around the world.

To understand the channels of information transmission, we run cross-country regressions of predictability measures on several country-level, commodity-related and macroeconomic variables. We find that the magnitude of predictability of stock market returns on the basis of commodity sector returns is strongly negatively related to the country's stage of development and, to a lesser degree, positively related to the dependence of the country's GDP on the exports and imports of commodities. Indeed, only for the Energy and the Agriculture sectors is the predictability of stock market returns significantly related to this measure of trade dependence. We also allow for the possibility that economic linkages between countries that are highly dependent on commodity trade may indirectly affect predictability. We take into consideration both financial (bilateral foreign portfolio investment) and trade (total bilateral trade) linkages in our construction of novel indirect measures of trade dependence. However, there is even less evidence that indirect trade dependence channels play an important role here.

We find strong evidence that commodity sector returns contain information about future changes in market fundamentals in a wide range of countries, i.e., the commodity sector returns predict inflation or industrial production growth rates in more than 80% of the countries in our sample. Furthermore, with the exception of the Precious Metals sector, the ability of commodity sectors to predict inflation (and, to a lesser degree, industrial production) in a given country is strongly related to their ability to predict stock market returns in that country, even after accounting for commodity trade dependence. This supports the idea that commodity markets convey truly global and economically meaningful information that is relevant to financial markets and the real economy and that extends beyond simple trade dependence and input/output cost effects. Overall, the ability of both country characteristics and commodity sector returns to forecast market fundamentals goes a long way in explaining cross-country differences in commodity-stock market predictability (i.e., we strongly reject the null hypothesis that these variables are not important and we find that the R^2 ranges between 20% and 50% in our cross-country regressions).

Our empirical results remain robust when subjected to a wide range of changes to our specifications. Commodity sector returns convey information about stock markets beyond well-known stock market predictors, including the short rate (e.g., Ang and Bekaert (2007), Rapach, Strauss and Zhou (2013)), Hong and Yogo's (2012) commodity market open interest and predictors of the strength of the global economy, namely the Kilian Index, the Baltic Dry Index and the S&P 500 futures returns (Kilian (2009), Hu and Xiong (2013)). We also show that our results are robust to using real returns instead of nominal returns and to controlling for exchange rates. In our baseline regression, estimated on a country-by-country basis, we use overlapping observations, but our results are robust when we use non-overlapping observations, gross returns instead of excess returns, when controlling for contemporaneous commodity sector returns and when focusing on different subsamples. We also rule out the possibility that our coefficients vary over time, thereby addressing concerns about alternative explanations related to structural breaks, crises and financialization of commodity markets. Finally, we find similar evidence of predictability when running our predictive regressions on a pooled sample of countries, as opposed to a country-by-country basis.

We contribute to several strands of literature. The literature that is substantively closest to our work documents the predictability of stock market returns on the basis of commodity futures returns. Previous research, however, has focused predominantly on the role of oil futures prices in developed economies (e.g., Driesprong, Jacobsen and Maat (2008), Kilian (2009, 2014)), with few exceptions. Jacobsen, Marshall and Visaltanachoti (2018) study the ability of the Industrial Metals sector to predict stock market returns in a sample of 11 developed countries and Hu and Xiong (2013) study the ability of oil, copper and soybean futures returns to predict stock market returns in five emerging and developed countries in Asia. Hong and Yogo (2012) study the predictability of U.S. stock returns on the basis of commodity market open interest, rather than returns. We contribute to this stream of literature by examining a comprehensive set of commodities and countries over a longer period of time, as well as by studying the economic relevance of predictability, and the channels of information transmission.

We also contribute to the literature that analyzes the relation between commodity prices and macro fundamentals. Commodity prices are known to feed into the production of manufactured goods (Garner, 1989), affect inflation (e.g., Breeden (1980), Erb and Harvey (2006), Gorton and Rouwenhorst (2006), Cologni and Manera (2008), and Bekaert and Wang (2010)), and precede most economic recessions in the United States (e.g., Hamilton (2009)). Black (1976), Bray (1981) and Sockin and Xiong (2015) show that commodity futures prices can reveal a great deal and can, therefore, provide useful information for commodity production and processing. Moreover, Kilian (2009), Hu and Xiong (2013) and Sockin and Xiong (2015) argue that commodity markets also contain information about the future strength of the global economy. However, most of these studies focus exclusively on the U.S. market, and we do not know whether or not these findings have global relevance in the sense of being applicable to other countries. We show that commodity sector returns also predict inflation and industrial production growth rates and that this predictability exists in many countries around the world, both emerging and developed.

A related question is why there is a delay in information from commodity markets reaching stock markets? First, commodity markets facilitate price discovery (e.g., Working (1948), Garbade and Silber (1983), and Hong and Yogo (2012)) and may, thus, contain relevant information that reaches other markets with a delay. Second, limited market participation (e.g., Hirshleifer (1988)) and slow information diffusion among investors with limited information-processing capacity (as in, for example, Merton (1987a) and Hong and Stein (1999)) may cause a delay in information transmission. Most of the studies that have been carried out on the issue of slow information diffusion look at lead-lag effects in stock markets between firms or industries in a single-country context (e.g. Hou and Moskowitz (2005), Hong, Torous and Valkanov (2007), Cohen and Frazzini (2008), and Menzly and Ozbas (2010)) or in a cross-country, international setting (Rapach, Strauss and Zhou, 2013). One exception here is Pan and Poteshman (2006) who find that information diffuses slowly from option markets to stock markets. We contribute to this literature by providing evidence of information diffusing slowly across markets, as we show that the ability of commodity sector returns to predict stock market returns is related to their ability to predict macro fundamentals.

Finally, our results also speak to the classic literature on the informativeness of commodity prices. On the one hand, Black (1976), Danthine (1978), Bray (1981) and Sockin and Xiong (2015), among others, show that commodity markets provide valuable information by aggregating dispersed information and even providing information about the strength of the global economy. On the other hand, Stein (1987), Sockin and Xiong (2015), and Brogaard, Ringgenberg and Sovich (2018) show that futures markets can be contaminated with informational noise and can actually reduce the welfare of firms that rely on futures prices for guidance in planning and making production decisions. We show that commodity sector returns systematically forecast stock market returns and macro fundamentals. This constitutes new evidence that commodity futures markets play an important information discovery role in financial markets and the real economy.

2.1. Data and variable definitions

2.1.1. Commodity market returns

We use daily commodity prices and open interest for the 28 most liquid, exchange-traded commodities from the Commodity Research Bureau (CRB). We compute (uncollateralized) futures returns using a roll-over strategy of first or second nearest-to-maturity contracts. We roll out of the first nearest contract (and into the second nearest contract) at the end of the month before the month prior to maturity. In this way, we guard against the possible confounding effect of the erratic price and volume behavior that is commonly observed close to maturity. We use the short end of the futures curve because these contracts are typically the most liquid. Furthermore, to increase liquidity and reduce the impact of non-synchronous trading, we exclude daily returns if we do not observe positive trading volume on that day. We also compute the growth rate of commodity market interest in accordance with Hong and Yogo (2012).

Commodities are partitioned into six sectors: Energy, Industrial Metals, Agriculture I & II, Livestock & Meats, and Precious Metals. We sub-divide the agriculture sector into two different sectors based on the correlation structure of individual commodities, as this sector is the most heterogeneous.[5] For each sector, we compute the equally-weighted returns of all commodities in that sector. We also construct an equally-weighted index of all 28 commodities (EWI) and use the production-weighted S&P GSCI Total Return Index (GSCI) sourced from Global Financial Data.

Detailed information on the composition of each sector, the name of the exchange in which each contract is traded, sample periods and delivery months of each contract, is provided in Table A.1 in the Appendix.

2.1.2. Stock market returns

We use daily, country-level price indices (code MSRI) from the MSCI data. All of these indices are free-float-adjusted, market-capitalization weighted. Excess returns are expressed in national currency, in line with Solnik (1993), Ang and Bekaert (2007), Hjalmarsson (2010), and Rapach, Strauss and Zhou (2013). We compute excess returns with respect to country-specific proxies of the risk-free rate specified in the Global Financial Data (GFD). Depending on the availability of data on the risk-free rate, we either use the *Total Return T-bill Index* or *Total Return Daily T-Bill* series constructed by the GFD, 3-month treasury bill yields, interbank interest rates, overnight interest rates or deposit rates. Table A.2 in the Appendix specifies the risk-free rate proxy used for each country. We also extract daily prices for the S&P 500 futures from Global Financial data.

[5]This subdivision roughly corresponds to the *Grains* and *Softs* sectors in Gorton and Rouwenhorst (2006).

2.1.3. Trade dependence variables

We use annual data on total bilateral trade (export and import) flows from the International Monetary Fund (IMF) Direction of Trade Statistics (DOT) data on bilateral export and import flows across countries as well as the United Nations Conference on Trade and Development (UNCTADstat) granular data on export and import flows of specific goods. This data is available from 1995 onwards. We match each of our 28 individual commodities to three-digit Standard International Trade Classification (SICT) codes in UNCTADstat and retrieve yearly data on commodity-specific export and import flows from each country to the rest of the world.[6] We also use annual data on country-level, bilateral financial flows from the International Monetary Fund's (IMF) Coordinated Portfolio Investment Survey (CPIS). Data on portfolio investment security holdings, discriminating between short-term debt, long-term debt, equity and total investment (equity plus debt) holdings is available from 2001 onwards.

We define the direct trade dependence of country i on commodity sector s as:

$$TD_{i,s} = T^{-1} \sum_{t=1}^{T} \frac{X_{i,t,s} + M_{i,t,s}}{Y_{i,t}} \tag{2.1}$$

where $X_{i,t,s}$ is the total value of the export flows of all goods matched to commodity sector s from country i to the rest of the world in year t; $M_{i,t,s}$ is the total value of the import flows of all goods matched to commodity sector s to country i from the rest of the world; and $Y_{i,t}$ is the GDP of country i in year t. $TD_{i,s}$ measures the importance of the trade in a given commodity sector relative to the country's GDP.

[6] We describe our matching procedure in detail in Appendix Section A.1.

We are also interested in capturing each country's indirect trade dependence on each commodity sector. We define the indirect trade dependence of country i by measuring the trade dependence of a country's trading partners or countries with which country i has the strongest financial links. Our choice of these measures is informed by the bilateral trade dependence measures used the international business cycle synchronization literature (see, for instance, Imbs (2004) and Frankel and Rose (1998)).

First, we define the trade and financial linkages between countries in the following way:

$$T_{i,k,t} = \frac{X_{i,k,t} + M_{i,k,t}}{Y_{i,t}}, \tag{2.2}$$

$$F_{i,k,t} = \frac{A_{i,k,t} + L_{i,k,t}}{Y_{i,t}}. \tag{2.3}$$

$T_{i,k,t}$ measures the importance of trade flows between country i and its trading partners relative to country i's GDP. $X_{i,k,t}$ ($M_{i,k,t}$) is the total export (import) flows from country i to country k in year t. Financial linkages between countries, $F_{i,k,t}$, are measured analogously, but instead of the trade flows between countries, we use bilateral financial flows between countries. $A_{i,k,t}$ is the asset side of the investment exposure of country i with respect to country k, that is the dollar value of portfolio investment in country k held by country i; $L_{i,k,t}$ is the liability side of the investment exposure.[7] The indirect trade dependence of country i on commodity sector s is then defined as:

$$ITD_{i,s} = T^{-1}K^{-1}\sum_{t=1}^{T}\sum_{k=1}^{K} T_{i,k,t} \times \frac{X_{k,t,s} + M_{k,t,s}}{X_{w,t,s} + M_{w,t,s}}, \tag{2.4}$$

$$IFD_{i,s} = T^{-1}K^{-1}\sum_{t=1}^{T}\sum_{k=1}^{K} F_{i,k,t} \times \frac{X_{k,t,s} + M_{k,t,s}}{X_{w,t,s} + M_{w,t,s}}. \tag{2.5}$$

[7] We use "derived liabilities" variables in CPIS to construct our measures.

The indirect trade dependence of country i on commodity sector s, $ITD_{i,s}$, is the average of the product of the trade importance of country i's trading partner relative to its GDP and the importance of that trading partner in the world trade of commodity s. The more important country i's trading partner is in the world trade of a particular commodity sector or the more important that trading partner is relative to country i's GDP, the higher the indirect trade dependence. The indirect financial dependence of country i on commodity sector s, $IFD_{i,s}$, is defined in an analogous way. The stronger the financial linkages between the countries, or the more important that country is in the world trade of the given commodity sector, the higher the indirect financial dependence of country i on commodity sector s.

2.1.4. Market fundamentals

We use two variables to capture the overall state of a country's economy: the inflation rate and real industrial production growth rates. We rely on the Global Financial Data (GFD) to extract the monthly consumer price index and industrial production series. We use the consumer price index to compute inflation rates and to deflate industrial production growth.

We exclude the bottom and top 1% of outliers from the inflation rate series and the industrial production growth rate series, as we observe inflation rates as high as 1789% and industrial production growth rates of 26700% in our sample.

2.1.5. Summary statistics

Table 2.1 presents summary statistics for our data. Panel A reports the annualized means and standard deviations for the commodity indices; the within-sector correlations, which are average correlations across all pairs of individual commodity returns composing a given sector; and across-sector correlations, which are calculated as the average correlation across all sectors (based on either sector or individual commodity returns). Panel B reports average excess returns and standard deviations for groups of countries. We follow the MSCI database classification of countries and split our 70 countries into 47 emerging and 23 developed countries. We further categorize our 70 countries based on whether their direct export (import) dependence for each of the six commodity sectors is above or below the median direct export (import) dependence in the sample. Panel C provides means and standard deviations for our economic variables. Our sample period runs from November 1979 to February 2016. We observe the trade and economic variables for a shorter sample period, however, which we specify in Table A.3 in the Appendix.

Commodity returns in Panel A vary a great deal across sectors, ranging from negative average annual returns for Agriculture sectors to more than 6% p.a. return for the Industrial Metals sector. Looking at the correlations, we see that, with the exception of the Agriculture II sector, the commodities within each sector are more strongly correlated with each other than they are with the commodities outside that sector. This is the main reason why we have kept the agriculture sector split into two sub-sectors.

Of the 70 countries in our sample, 47 are classified as emerging and 23 as developed countries (Panel B). Emerging countries have lower average excess returns, but higher standard deviation than the developed countries. We also split our 70 countries into several subgroups based on whether the direct export (import) dependence of each country for each of our sectors is above or below the median. For example, we find an average annual excess return of 1.2% for the 35 countries whose export of energy commodities is above median, versus 1.9% p.a. for the 35 countries whose export of energy commodities is below the median.

Table 2.1. Summary statistics

Panel A presents descriptive statistics for the six commodity sector returns, the S&P GSCI Total Return Index (GSCI) and the equally-weighted index (EWI). Correlations under the heading “Within" are the average of pairwise correlations between individual commodities making up a given sector. Correlations under the heading “Across (sec)" are the average of pairwise correlations between a commodity sector and the remaining sectors. Correlations under the heading “Across (ind)" are defined analogously, except that the correlations are computed at the individual commodity level rather than at the sector level. In Panel B, we show the descriptive statistics for the country excess returns. We split the countries into emerging countries, developed countries and countries with a value of sector-specific exports or imports relative to GDP above or below the sample median. Panel C presents descriptive statistics for economic indicators at a monthly frequency. The sample period is from November 1979 to March 2016.

Panel A: Commodity Sectors Returns (Annualized)

	Mean	Std	Correlations Within	Correlations Across (sec)	Correlations Across (ind)
Energy	3.7%	28.8%	54%	20%	12%
Industrial Metals	6.2%	25.5%	100%	30%	17%
Agriculture I	-1.7%	18.5%	46%	29%	12%
Agriculture II	-0.5%	15.9%	8%	27%	10%
Livestock & Meats	0.2%	16.0%	46%	12%	12%
Precious Metals	3.0%	23.6%	60%	29%	14%
EWI	-0.1%	12.6%	16%	54%	
GSCI	2.5%	18.9%			

Panel B: Country Excess Stock Returns (Annualized)

	Total	Emerging	Developed
No countries	70	47	23
Avg. Excess Return	1.5%	0.9%	2.8%
Avg. Std	26.5%	29.5%	20.5%

Avg. Excess Return	Energy	Industrial Metals	Agriculture I	Agriculture II	Livestock & Meats	Precious Metals
Export > Median	1.2%	1.6%	1.1%	0.9%	1.3%	2.8%
Export < Median	1.9%	1.5%	2.0%	2.2%	1.9%	0.3%
Import > Median	0.5%	0.9%	1.3%	-0.4%	0.5%	0.9%
Import < Median	2.6%	2.2%	1.8%	3.5%	2.6%	2.2%

Avg. Std	Energy	Industrial Metals	Agriculture I	Agriculture II	Livestock & Meats	Precious Metals
Export > Median	28.8%	26.6%	28.7%	26.2%	25.7%	27.9%
Export < Median	24.3%	26.5%	24.4%	26.9%	27.4%	25.2%
Import > Median	26.2%	26.4%	25.3%	25.6%	25.4%	25.1%
Import < Median	26.9%	26.7%	27.8%	27.5%	27.7%	28.0%

Panel C: Economic Indicators

	Total	Emerging	Developed
		Inflation Rate	
No countries	67	46	21
Avg.	1.01%	1.32%	0.34%
Avg. Std	1.97%	2.61%	0.56%
		Real Industrial Production Growth	
No countries	62	41	21
Avg.	0.23%	0.19%	0.31%
Avg. Std	4.38%	4.94%	3.29%

The difference on the import side for energy commodities is even bigger, with a 0.5% return for countries whose import of energy commodities is above the median, versus 2.6% for those below the median. In general, commodity dependent countries whose direct export or import dependence is above the median tend to have lower average excess returns than those below the median. This is reminiscent of the well-known natural resource curse (van der Ploeg (2011)).

In Panel C we report summary statistics for our economic variables. Due to data limitations, we do not observe economic indicators for all countries in our sample. We are missing inflation information for three countries and industrial production for eight countries. Emerging countries have much higher inflation rates and smaller real industrial production growth rates than developed countries. Both economic variables are much more volatile for the emerging countries than for the developed countries.

2.2. Do commodity returns predict stock market returns?

We analyze the predictability of stock market returns around the world using country-level regressions of excess stock market returns on lagged commodity futures returns, controlling for other stock market predictors. Our baseline regression is as follows:

$$r_{i,t:t+19} = \beta_{i,0} + \sum_{s}(\beta_{i,s,1} r^{c}_{s,t-21:t-2} + \beta_{i,s,2} r^{c}_{s,t-41:t-22}) + \phi_i r_{i,t-20:t-1} + \varepsilon_{i,t:t+19}, \tag{2.6}$$

where $r_{i,t:t+19}$ denotes the monthly excess return of country i compounded from 20 daily returns between time t and $t+19$; $r^{c}_{s,t-21:t-2}$ and $r^{c}_{s,t-41:t-22}$ are lagged monthly commodity futures returns for sector s compounded from daily prices. We refer to these returns as one-month and two-month lag returns, respectively. We skip one full day between stock market return and commodity return given that, at present, many of our commodities are traded almost 24 hours a day.[8] We control for lagged stock returns, as autocorrelation in the series of stock returns can generate spurious evidence of predictability in the presence of contemporaneous correlation between commodity and stock returns (Boudoukh, Rishardson and Whitelaw (1994); Chordia, Roll and Subrahmanyam (2005)). We consider the following sectors: Energy, Industrial Metals, Agriculture I & II, Livestock & Meats and Precious Metals.

Panel A of Table 2.2 summarizes the key results from running regression (2.6) country-by-country for all 70 countries. In columns three to ten, for each commodity sector and the overall indices, we report the total number (T) and percentage (%) of countries for which we find significant slope coefficients at the 10% level, as well as the number of positive (P) and negative (N) coefficients that are statistically significant at the 10% level, separately for each lag and jointly across horizons. We use Newey-West standard errors with 19 lags to account for overlapping observations. Column 11 contains the number of countries that are predicted by at least one of the six sectors (excluding EWI and GSCI). The last two rows report overall regression statistics: average, median, maximum and minimum values of the individual R^2s, and the number of countries for which the Wald test of joint significance of all commodity slopes rejects the null hypothesis at the 10% level.

[8]For example products traded via CME ClearPort Clearing are traded from 6:00 p.m. to 5:00 p.m. EST with only a one hour break each day.

Table 2.2. Baseline predictive regressions summary

This table summarizes the results of the following regression for each country *i*:

$$r_{i,t:t+19} = \beta_{i,0} + \sum_s (\beta_{i,s,1} r^c_{s,t-21:t-2} + \beta_{i,s,2} r^c_{s,t-41:t-22}) + \phi_i r_{i,t-20:t-1} + \varepsilon_{i,t:t+19}$$

Panel A shows the number of countries for which monthly stock market excess returns ($r_{i,t:t+19}$) are predicted by sector-*s* one-month ($r^c_{s,t-21:t-2}$) and two-month ($r^c_{s,t-41:t-22}$) lagged commodity futures returns at the 10% significance level (based on Newey-West standard errors with 19 lags). *T*, *%*, *P* and *N* stand for total number, percentage, number of positive and number of negative countries (out of 70), respectively. We also present the counts of significant coefficients across all lags, for at least one sector and for at least one lag. The last two rows show the descriptive statistics for the regression R^2s and *p(Wald) < 10%* which denotes the number of countries for which we reject the null hypothesis that all commodity sector returns fail to predict stock market excess returns. Panel B shows the counts of significant coefficients for at least one lag for a sub-sample of countries with sector-specific exports or imports relative to GDP above or below the sample median, in developed and emerging countries. The sample period is from November 1979 to March 2016.

		Energy	Industrial Metals	Agriculture I	Agriculture II	Livestock & Meats	Precious Metals	EWI	GSI	At least one sector
Panel A: Full Sample Country Counts										
$r^c_{s,t-21:t-2}$	T	22	13	4	11	16	6	6	13	49
	%	31%	19%	6%	16%	23%	9%	9%	19%	70%
	P	4	8	3	11	14	5	6	7	
	N	18	5	1	0	2	1	0	6	
$r^c_{s,t-41:t-22}$	T	10	18	11	11	9	10	14	17	46
	%	14%	26%	16%	16%	13%	14%	20%	24%	66%
	P	2	17	3	8	6	10	14	17	
	N	8	1	8	3	3	0	0	0	
All Horizons	T	1	5	0	4	2	2	3	6	
	%	1%	7%	0%	6%	3%	3%	4%	9%	
At least one horizon	T	31	26	15	18	23	14	17	24	59
	%	44%	37%	21%	26%	33%	20%	24%	34%	84%
	P	6	21	6	15	19	13	17	18	
	N	25	5	9	3	5	1	0	6	
R^2	mean	5.50%	median	3.83%	max	22.76%	min	1.26%		
p(Wald) < 10%	T	35	50%							

Table 2.2. Baseline predictive regressions summary

(continued)

		Energy	Industrial Metals	Agriculture I	Agriculture II	Livestock & Meats	Precious Metals	EWI	GSI
Panel B: Subsamples (Country Counts)									
Export > Median	T	13	10	8	10	11	10	7	10
	%	37%	29%	23%	29%	31%	29%	20%	29%
	P	1	6	2	10	9	10	7	6
Export < Median	T	18	16	7	8	12	4	10	14
	%	51%	46%	20%	23%	34%	11%	29%	40%
	P	5	15	4	5	10	3	10	12
Import > Median	T	14	10	9	11	10	6	9	14
	%	40%	29%	26%	31%	29%	17%	26%	40%
	P	2	8	2	9	9	6	9	11
Import < Median	T	17	16	6	7	13	8	8	10
	%	49%	46%	17%	20%	37%	23%	23%	29%
	P	4	13	4	6	10	7	8	7
Emerging	T	16	20	10	10	17	7	14	15
	%	34%	43%	21%	21%	36%	15%	30%	32%
	P	6	18	6	7	13	6	14	15
Developed	T	15	6	5	8	6	7	3	9
	%	65%	26%	22%	35%	26%	30%	13%	39%
	P	0	3	0	8	6	7	3	3

Several results emerge from our analysis. First, we find strong evidence of predictability across a large number of countries and sectors, which is our first piece of evidence in support of the global relevance of the information content of commodity sector returns. The majority of countries in our sample (84%) are predicted by at least one commodity sector at at least one horizon, while 70% are predicted with a one-month lag and 66% with a two-month lag. The Energy, Industrial Metals, and Livestock & Meats sectors predict the highest percentage of countries at at least one horizon, ranging from 44% for Energy to around 35% for Industrial Metals and Livestock & Meats. Precious Metals and Agriculture sectors predict around 20% to 26% of all the countries in our sample.

Second, looking at the R^2s, commodity sector returns predict stock market returns with an average R^2 of 5.50%, with a high of 22.76%. These values are economically large. Using data for 18 countries from 1970 to 1989, based on international, monthly predictive regressions of country excess stock market returns on six non-commodity world market predictors, Ferson and Harvey (1993) find an average adjusted R^2 of 7.2% across countries. When extending the predictive model to include lags of the dividend yield, short-term interest rate, term yield and local excess stock market return, they find an average adjusted R^2 of 8.1%. Despite focusing only on commodity world market predictors and trying to explain a much larger number of local variables (stock market excess returns), our predictive model is comparable in terms of fit to that of Ferson and Harvey (1993).[9]

[9]For a list of R^2s found in other studies, refer to Table I of Ferson, Sarkissian and Simin (2003). The reported R^2s therein range from less than 1% to 7.8%. For a discussion of how small R^2s over short return horizons translate into large R^2s over longer horizons, refer to Fama and French (1988) and Cochrane (2008). From a portfolio allocation perspective, Kandel and Stambaugh (1996) and Fleming, Kirby and Ostdiek (2001) show that even models with R^2s as small as 0.24% can be associated with high sensitivities of portfolio weights with respect to the predictive variables.

Accordingly, the joint test for the significance of all slope coefficients shows that for 35 countries (50%), we cannot reject the null hypothesis that commodity returns are important predictors of stock market returns.

Third, we observe both positive and negative slopes for almost all sectors, with Energy coefficients being predominantly negative and Industrial Metals and Livestock & Meats coefficients generally being positive. The overall commodity indices predict the stock markets with a positive sign in most cases. If futures prices matter most in terms of the cost channel, we would expect the signs to be related to the export (import) dependence of each country on a given commodity sector. The cost channel predicts a negative impact of increases in commodity prices for countries that rely on the import of commodities via higher input costs, and a positive impact for the exporters due to increased sales price. The fact that we observe predominantly positive signs for most sectors and for the overall indices may suggest that commodity futures markets matter not only in terms of the cost channel but perhaps also in terms of the information channel through which commodity futures markets convey important information about the future state of the global economy (e.g., Danthine (1978), Sockin and Xiong (2015)). We analyze this point in more detail in the subsequent sections.

Finally, predictability also varies across horizons. Energy predicts, largely, with a one-month lag, while Industrial Metals and the Agriculture sectors predict up to a two-month lag. However, there are very few countries that are predicted by commodity sectors at both horizons. As such, the results across horizons reflect the fact that the information from a given sector may reach different countries with different degrees of delay due to, for example, differences in cross-country market efficiency and/or dependence of a country's economy on a given commodity sector.

In Panel B, we split our 70 countries into several groups. First, we divide up our countries based on whether the value of commodity exports (imports) of each country relative to its GDP is above or below the median for each commodity sector. We find similar predictability across these subgroups of countries. Moreover, within each subgroup, we observe both positive and negative slope coefficients. These results suggest that direct trade dependence may not be the only channel through which information from commodity markets reaches stock markets.

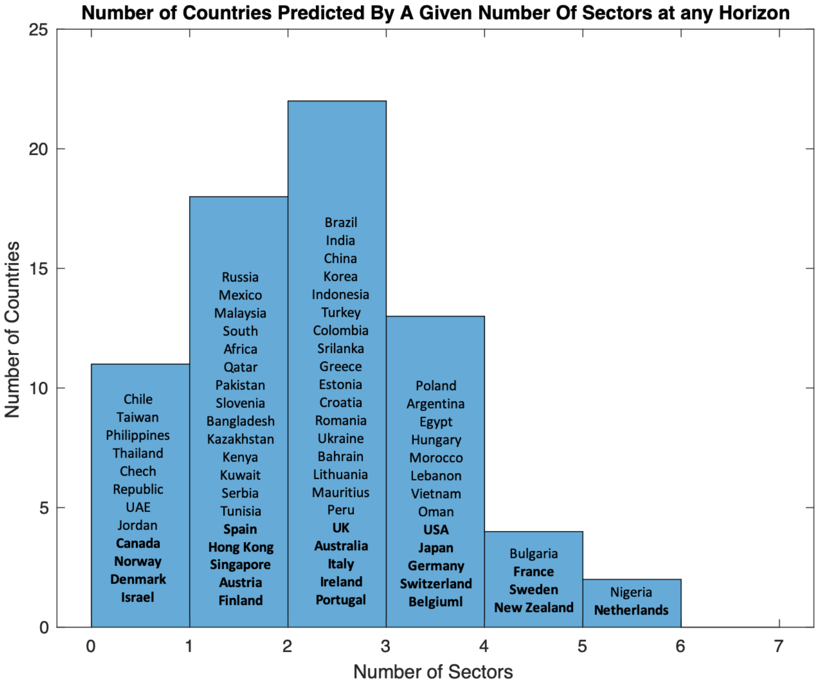
Next, we differentiate between emerging and developed countries, and find similar evidence of predictability for both groups of countries. For the emerging countries, we once again observe both positive and negative slope coefficients within a given sector, while the developed countries seem to be more homogeneous, with the signs of the slope coefficients being either all positive or all negative.

Figure 2.1 further illustrates which countries are predicted by a given number of sectors at at least one horizon. The majority of the countries (35 out of 70) are predicted by three to four commodity sectors: 10 of these countries are developed and 25 are emerging. 11 out of 70 countries are not predicted by any commodity sector and Nigeria and the Netherlands are the only countries predicted by all six sectors.

Overall, all commodity sectors are predictive for a wide range of countries in our sample. This shows that commodity futures markets aggregate dispersed information with a truly global predictive value and that this (global) value is not confined to the Energy and Industrial Metals sectors. These results build upon findings from the previous literature that predominantly focuses on oil futures and developed countries (e.g., Driesprong, Jacobsen and Maat (2008), Kilian (2009, 2014)), with some exceptions (e.g., Hu and Xiong (2013), Jacob-

Figure 2.1. Predictability across countries

The histogram shows the number of countries for which monthly stock market excess returns are predicted by a given number of commodity sector futures returns with either a 1-month or a 2-month lag. The figure also shows which countries fall into each category and distinguishes between emerging and developed countries (in bold).



sen, Marshall and Visaltanachoti (2018)). We find that in addition to Energy, other sectors, Agriculture in particular, predict a substantial number of stock market returns and that whether predictability is present or not seems unrelated to a country's stage of development or its direct export (import) dependence in a given commodity sector. The fact that we find strong predictability stemming from Agriculture to stock market returns in the different subgroups of countries shows that, contrary to widely held assumptions about this sector, the information it conveys seems relevant not only to agriculture-based economies, which are typically less developed, but also to developed countries and countries whose direct export (import) dependence on agriculture commodities is low. The predictability of stock market returns on the basis of commodity returns is thus widely applicable across countries and sectors. In Section 2.5, we show that the evidence of predictability is robust even with the addition of a variety of controls to our baseline regression and that the results are not driven by time variation in the coefficient estimates.

2.3. Is predictability from all commodity sectors and in all countries important?

The analysis presented above is informative about the number of countries predicted by each sector and the overall goodness-of-fit of our model. As such, it yields only an incomplete picture. For example, we would like to be able to understand whether the predictable part of country-specific and global stock market return variation responds substantially more to shocks to the Energy sector relative to other sectors or whether all sectors contribute to a comparable share of that predictable variation. In fact, despite the fact that all sectors predict a large number of countries in a statistical sense, finding that Energy has an overwhelmingly dominant share would hardly be surprising given its much higher production value relative to other sectors. It is also possible that the bulk of the information content in commodity markets pertains to the stock markets of big players in the global economy and international trade, like the US and China. This would imply that the economic importance of the information content of commodity markets with respect to stock markets is not truly global.

In order to answer the question of whether predictability is concentrated in key sectors or key countries, we develop four measures of economic significance:

$$
\begin{aligned}
WS_{i,s} &= \frac{\sum_h |\beta^*_{i,s,h}| \mathbf{1}_{i,s,h}}{\sum_s \sum_h |\beta^*_{i,s,h}| \mathbf{1}_{i,s,h}}, && (2.7)\\
BS_{i,s} &= \frac{\theta_i \sum_h |\beta^*_{i,s,h}| \mathbf{1}_{i,s,h}}{\sum_i \left[\theta_i \sum_h |\beta^*_{i,s,h}| \mathbf{1}_{i,s,h}\right]}, && (2.8)\\
GC_i &= \frac{\theta_i \sum_s \sum_h |\beta^*_{i,s,h}| \mathbf{1}_{i,s,h}}{\sum_i \left[\theta_i \sum_s \sum_h |\beta^*_{i,s,h}| \mathbf{1}_{i,s,h}\right]}, && (2.9)\\
GS_s &= \frac{\sum_i \theta_i \left[\sum_h |\beta^*_{i,s,h}| \mathbf{1}_{i,s,h}\right]}{\sum_s \sum_i \theta_i \left[\sum_h |\beta^*_{i,s,h}| \mathbf{1}_{i,s,h}\right]}, && (2.10)
\end{aligned}
$$

where $|\beta^*_{i,s,h}|$ is the absolute value of the standardized regression coefficient obtained from regression (2.6) with respect to sector *s*, country *i*, and horizon *h*; *h* corresponds to one-month and two-month lag returns as defined in equation (2.6); $\mathbf{1}_{i,s,h}$ is an indicator variable equal to unity if the coefficient $\beta^*_{i,s,h}$ is significant at the 10% level; and θ_i is an adjustment to yield infinite-horizon interpretation. This is important, as less efficient stock markets will react more slowly to the same information, so an accurate economic significance analysis should take that into account. This variable is defined as $\theta_i = \frac{1}{1-|\phi_i|}$, where ϕ_i is estimated in regression (2.6). $|\phi_i|$ can be interpreted as a market inefficiency metric. The more autocorrelated the series of stock returns are, i.e., the farther away $|\phi_i|$ is from 0, the larger θ_i will be. In the case of full market efficiency, in the sense that past stock returns are completely irrelevant to predicting next period returns, it must be the case that $\phi_i = 0$ and, as a consequence, $\theta_i = 1$.[10]

The interpretation of all measures is intuitive. For each sector, the distribution of the within-country measure ($WS_{i,s}$) is plotted in Figure 2.2a. $WS_{i,s}$ quantifies which sectors are the most prominent within each country. It cap-

[10] Derivation of the infinite-horizon adjustment is provided in Appendix Section A.2.

tures the share of country i's stock market response to a one standard deviation shock to all commodity sectors and at all lags that is attributable to commodity sector s. We further define sector s to strongly dominate other sectors in country i if and only if $WS_{i,s} > 75\%$.

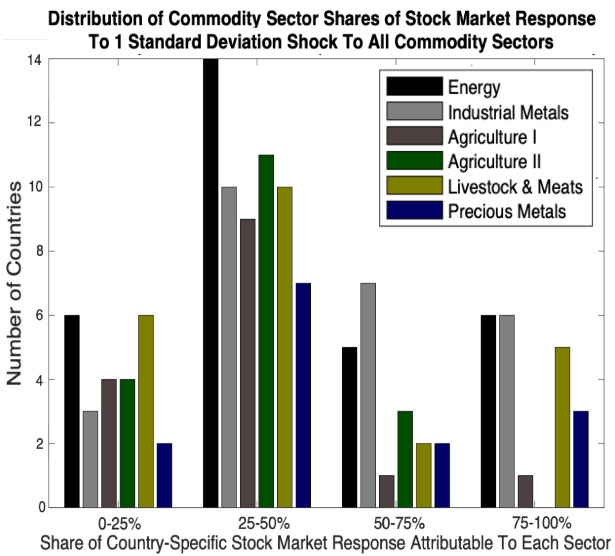
We see that only for a small fraction of countries does the response of a particular sector dominate the responses of the other sectors. For example, the Energy sector is predictive for many countries, however, this sector strongly dominates all other sectors in only six out of 31 countries (19% of the countries it is predictive for). The picture is very similar for Industrial Metals, Livestock & Meats and Precious Metals, which also strongly dominate in around 20% of the countries they predict. For the Agriculture sectors, we find strong dominance in at most one country per sector. Hence, for those countries, there is a single commodity sector that contains the majority of the relevant information that flows from commodity markets to that country. However, for the majority of the countries, i.e., around 80% of the countries whose stock markets are predicted by non-Agriculture sectors and almost 100% of the countries predicted by the Agriculture sectors, the information contained in that particular sector does not strongly dominate the other sectors. As such, when looking within countries, the economic magnitude of predictability based on different sectors is rather evenly spread.

Figure 2.2b illustrates the distribution of the between-country sector measure ($BS_{i,s}$) for each sector; this value quantifies whether or not predictability from a given commodity sector is evenly distributed across countries. For all sectors, if we shock the commodity sector at all lags by one standard deviation, for the majority of the countries (between 65% and 100% depending on the sector), the share of the global stock market response attributable to each individual country is lower than 7.5%. Even though there are a few countries

Figure 2.2. Within-country and between-country measures of economic significance of predictability

In (a), we plot the within-country sector measures, $WS_{i,s}$, which capture the sector-*s* share of the response of the stock market of country *i* to a one standard deviation shock to all commodity sectors. The distribution of $WS_{i,s}$ depicts the relative importance of different sectors within countries. In (b), we plot the between-country sector measures, $BS_{i,s}$, which correspond to country-*i*'s share of the global stock market response to a one standard deviation shock to a specific commodity sector *s*. The distribution of $BS_{i,s}$ provides information about whether or not predictability on the basis of a given commodity sector is evenly distributed across countries.

(a) Within-country Sector Measure



(b) Between-country Sector Measure

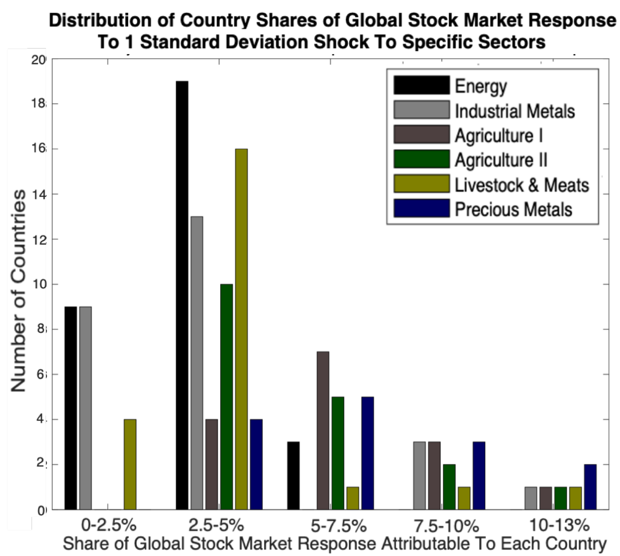
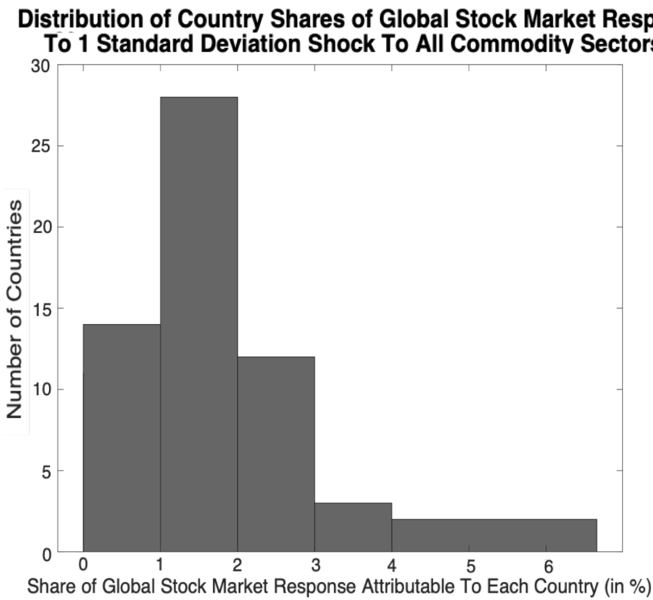


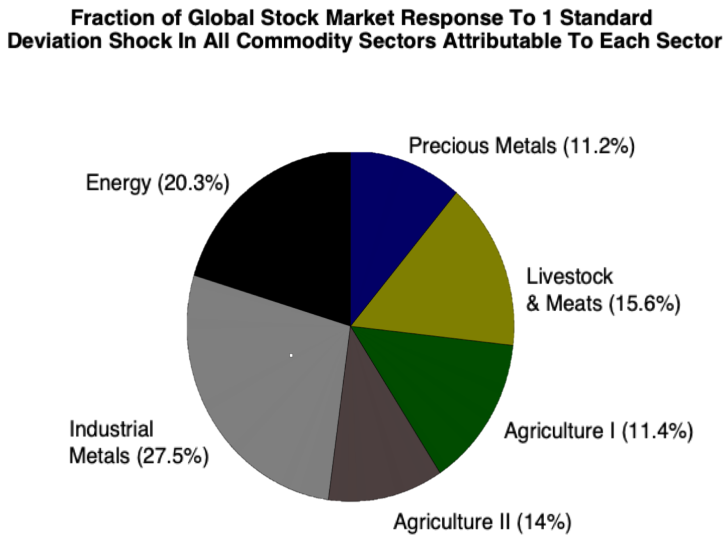
Figure 2.3. Global measures of economic significance of predictability

This figure shows the global stock market responses to a one standard deviation shock to all commodity sectors. In (a), we plot the between-country global commodity measure, GC_i, to assess whether or not the global economic magnitude of predictability on the basis of all commodity sectors is evenly distributed across countries. In (b), we plot the global sector measure, GS_s, to assess whether the global economic magnitude of predictability is evenly distributed across all sectors.

(a) Global Commodity Measure



(b) Global Sector Measures



for which predictability is dominated by the Energy sector within that country, when looking across countries, the country shares of the global response to an Energy shock is, at most, 7.5% (in three countries), while for 9 countries, the shares are less than 2.5%. For one country, the share of the global response to a commodity shock to either Agriculture I, Agriculture II, Livestock & Meats or Industrial Metals is, at most, 13%. In the case of Precious Metals, there are two such countries with shares of at most 13%. This suggests that all sectors are also, in large part, equally important across our countries.

This conclusion is further supported by Figure 2.3a, which shows the cross-country distribution of the between-country global commodity measure (GC_i).

This measure captures the share of the response of country i if we shock all commodity sectors in all countries at all horizons by one standard deviation. In other words, it quantifies whether predictability from all commodity sectors is evenly distributed across countries. The impact of all sectors taken together is evenly distributed across stock markets around the world, as no single country has a share of more than 7%. Out of the 59 countries that are predicted by at least one sector, 54 countries have shares of no more than 3% and, of those, 42 have shares of less than 2%.

Finally, Figure 2.3b shows the global sector measure (GS_s), which quantifies whether or not predictability across all countries is evenly distributed across sectors. This measure captures the share of the global stock market response that is attributable to sector s if we shock all commodity sectors in all countries at all horizons by one standard deviation. We find that all sectors are important globally. If we shock all sectors in all countries, the Agriculture I and II sectors jointly capture 25% of the global response, while all the other sectors capture around 20% each (Industrial Metals (27.5%), Energy (20.3%), Livestock & Meats (15.6%) and Precious Metals (11.2%)). This is surprising if we contrast these values with the production values of each commodity sector in the global economy. For example, in 2018, the S&P GSCI index allocated a share of 58.6% to Energy, 18.3% to Agriculture and 10.9% to Industrial Metals (copper, alone, has a share of only 4.4%), 7.5% to Livestock & Meats and 4.7% to Precious Metals. Hence, our results suggest that the importance of the information flows from many of our commodity sectors is different from their importance in terms of their global production value.

2.4. Economic channels of information transmission

In this section, we explore possible channels through which the information may flow from commodity to stock markets.

On the one hand, the information content of commodity markets in a given country is likely to depend on country characteristics, such as the degree of stock market efficiency and development and, crucially, the exposure of that country to commodity markets through commodity trade. Some countries heavily export and import a given commodity, thus making their economy directly dependent on that particular commodity. This includes direct terms of trade effects related to import costs and export revenues (Chen, Rogoff and Rossi, 2010), as well as more nuanced effects such as political stability and the ability to attract foreign direct investment (Classens and Duncan, 1994). Other countries do not depend directly on trading a given commodity, but may be indirectly exposed to other commodity-dependent countries via bilateral trade or financial linkages. These linkages are known to be important determinants of business cycle synchronization and economic spillovers (e.g. Frankel and Rose (1998), Kalemli-Ozcan, Papaioannou and Peydró (2013), Cesa-Bianchi, Imbs and Saleheen (2018)) and lead to stock return predictability across trade-linked countries (Rizova, 2010). Therefore, if a country's balance of payments, either through its current account (exports and imports) or capital account (foreign portfolio investment and foreign direct investment), is very sensitive to the state of the economy of another commodity-dependent country, the former country is indirectly exposed to commodities. According to this line of reasoning, the information content of commodity markets is largely a function of the dependence of a country on commodity trade.

On the other hand, if commodities aggregate dispersed information about the state of the global economy, their information content should extend well beyond countries' dependence on commodity trade (e.g. Hu and Xiong (2013), Sockin and Xiong (2015)). In this case, the ability of commodity markets to predict stock markets should strongly depend on the extent to which a commodity conveys other information about a country's market fundamentals, even after controlling for trade dependence and other country characteristics like stock market efficiency and stage of development. While there is no reason to regard these hypotheses as mutually exclusive, we aim to understand which effect dominates.

2.4.1. Commodity trade dependence channel

We start with cross-country regressions of the estimated predictability coefficients on several country characteristics. These characteristics relate to the stage of development of a country's stock market as well as direct and indirect trade-related exposures to commodity markets.

$$\beta_{i,s} = \gamma_{0,s} + \gamma_e E_i + \gamma_\theta \theta_i + \gamma_{TD,s} TD_{i,s} + \gamma_{ITD,s} ITD_{i,s} + \gamma_{IFD,s} IFD_{i,s} + \varepsilon_{i,s}, \quad (2.11)$$

where $\beta_{i,s}$ is an average of the absolute values of the slope coefficients across the two horizons from our per-country, predictive regressions provided in equation (2.6).[11] E_i is a dummy variable that equals one if a country is emerging rather than developed. θ_i is the infinite horizon adjustment variable that we use in computing our economic significance measures in Section 2.3. This variable is defined such that the higher θ_i is, the more autocorrelated the stock returns of country i are. Therefore, we can interpret this measure as a market efficiency measure. $TD_{i,s}$, $ITD_{i,s}$, and $IFD_{i,s}$ are direct and indirect trade dependence measures defined in Section 2.1.3. The motivation for including these variables stems from the international business cycle synchronization literature, including Frankel and Rose (1998) and Imbs (2004), which suggests that trade and finance integration can lead to cross-country spillovers and output synchronization. Due to the availability of trade data, the sample period is restricted to the period between January 1995 and February 2016 and we re-estimate the slope coefficients from our per-country, predictive regressions on this restricted sample.

In Panel A of Table 2.3, we report the results of regression (2.11) for each sector, which means that we use a maximum of 70 cross-country observations. In the last column, we pool all six sectors together and use sector fixed effects to focus on within-sector variation. We report the estimated coefficients, their t-statistics, the R^2 of the regressions and the p-values of the test of whether or not all the explanatory variables are jointly significant at the 10% level. We also report the number of observations included in each regression.

Overall, we find that our country characteristics are important in explaining cross-country differences in predictability. The average R^2 varies across sectors between 15% and 35%, except for Precious Metals where it is only 5.5%. In the pooled regression, we find an R^2 of 19%. For all sectors, ex-

[11] We use absolute values because, while we observe both positive and negative slope coefficients, our hypotheses are about the strength of the predictability (not about the sign). We take the average across the two horizons to reduce dimensionality and noise - given that a smaller fraction of countries is predicted separately at each horizon than jointly across both horizons.

Table 2.3. Cross-country regressions: predictability and country-level characteristics

This table shows the results from cross-country regressions relating predictability and country-level characteristics. Panel A shows the results from the following regression:

$$\beta_{i,s} = \gamma_{0,s} + \gamma_e E_i + \gamma_\theta \theta_i + \gamma_{TD,s} TD_{i,s} + \gamma_{ITD,s} ITD_{i,s} + \gamma_{IFD,s} IFD_{i,s} + \varepsilon_{i,s}$$

where $\beta_{i,s} = \frac{1}{2}\sum_{h=1}^{2} |\beta_{i,s,h}|$, i.e., the average of the absolute values of the slope coefficients across the two lags *k* from the per-country predictive regressions specified in equation (2.6). E_i is a dummy variable that equals one if country *i* is emerging. θ_i is the infinite horizon adjustment variable that we use to compute the economic significance measures in Section 2.3. $TD_{i,s}$, $ITD_{i,s}$, and $IFD_{i,s}$ are direct and indirect trade dependence measures defined in Section 2.3. The last column shows the results of a pooled regression with sector fixed effects and with the imposition of equal coefficient estimates across sectors. Panel B is based on a similar regression excluding the indirect dependence measures and distinguishing between the export and import sides of direct trade dependence. T-statistics are based on heteroskedasticity-consistent standard errors.

	Energy	Industrial Metals	Agriculture I	Agriculture II	Livestock & Meats	Precious Metals	Pooled SFE
			Panel A: Direct and Indirect Trade Dependence				
coefficients							
$\gamma_{0,s}$	0.10	0.07	-0.07	-0.20	-0.14	-0.11	-0.09
E_i	0.02	0.05	0.01	0.03	0.05	0.01	0.03
θ_i	-0.08	-0.03	0.11	0.22	0.20	0.15	0.10
$TD_{i,s}$	0.08	-0.25	2.13	0.71	-1.30	-0.04	0.09
$ITD_{i,s}$	50.54	-31.56	26.97	9.37	-56.57	-4.10	-4.50
$IFD_{i,s}$	-18.38	10.85	-8.30	-24.09	-15.01	-0.13	-6.20
t-stats							
$\gamma_{0,s}$	1.80	0.70	-0.81	-1.87	-0.99	-1.07	-1.49
E_i	2.00	3.73	0.40	2.21	2.21	0.69	8.14
θ_i	-1.42	-0.30	1.25	2.19	1.49	1.60	1.66
$TD_{i,s}$	2.20	-0.53	2.02	2.22	-0.95	-0.14	2.32
$ITD_{i,s}$	1.56	-0.79	0.57	0.18	-0.74	-0.09	-0.23
$IFD_{i,s}$	-2.10	0.64	-0.71	-1.48	-0.71	-0.02	-1.60
R^2	35.41%	20.30%	16.55%	33.45%	23.37%	5.51%	19.40%
p(Wald)	0.00	0.01	0.04	0.00	0.00	0.60	0.00
No obs	69	69	69	69	69	69	414
			Panel B: Direct Export and Import Trade Dependence				
coefficients							
$\gamma_{0,s}$	0.07	0.06	-0.05	-0.16	-0.12	-0.10	-0.08
E_i	0.03	0.05	0.01	0.05	0.06	0.01	0.04
θ_i	-0.05	-0.01	0.08	0.17	0.17	0.14	0.08
Export $TD_{i,s}$	0.09	-0.19	4.22	0.73	-1.02	-0.04	0.09
Import $TD_{i,s}$	0.06	-0.65	-0.24	0.52	-3.33	-0.08	0.00
t-stats							
$\gamma_{0,s}$	1.28	0.52	-0.61	-1.59	-0.92	-1.04	-1.42
E_i	3.52	3.95	1.15	3.55	3.23	0.75	9.24
θ_i	-0.96	-0.12	1.13	1.81	1.33	1.62	1.60
Export $TD_{i,s}$	2.98	-0.40	3.75	1.51	-0.62	-0.04	2.40
Import $TD_{i,s}$	0.67	-0.29	-0.19	0.66	-1.31	-0.08	0.00
R^2	28.99%	19.42%	25.96%	29.57%	21.71%	5.64%	19.02%
p(Wald)	0.00	0.01	0.00	0.00	0.00	0.43	0.00
No obs	70	70	70	70	70	70	420

cept Precious Metals, as well as in the pooled regression, we reject the null hypothesis that our country characteristics are insignificant in explaining the cross-country variation in return predictability.

When looking at the results of the pooled regression, we find predictability to be stronger in emerging countries, as compared to developed countries. This is true for all but two sectors: Agriculture I and Precious Metals. Therefore, despite the fact that in previous sections we find that we can predict a similar percentage of countries in both groups, commodity sector returns move stock market prices more in emerging markets than they do in developed countries. Second, predictability is stronger in countries whose trade dependence on a given commodity sector is higher. This is not surprising given that high trade dependence on a given commodity sector exposes that countries' income, tax revenues and political stability to commodity markets. The surprising outcome is that we find significant effects only for the Energy and Agriculture sectors, which suggests that at least a substantial part of the information in global commodity markets is not explained by trade dependence effects. This is further supported by the fact that the indirect trade dependence measures do not seem to play a role either. In contrast, if we look at country i's financial partners, the more they depend on Energy, the less predictable country i's stock market returns are on the basis of Energy returns. However, neither of these indirect measures are significant in the pooled regression.[12]

In Panel B we split the direct trade dependence measure into export and import respectively.[13] We find that most of the effect of direct trade dependence comes from the export side, as we find predictability on the basis of the Energy

[12] In the case of Energy, the results are consistent with theoretical models and empirical evidence of trade (financial) integration leading to more (less) output synchronization. For trade, refer to, for instance, Frankel and Rose (1998) and Imbs (2004). For financial integration, refer to Kalemli-Ozcan, Papaioannou and Peydró (2013), Cesa-Bianchi, Imbs and Saleheen (2018), and references therein.

[13] Due to the limitations in the data, we are not able to reliably split indirect trade dependence measures.

and Agriculture I sectors to be stronger in countries in which a larger share of their GDP is tied to the export of Energy and Agriculture I commodities. This is also the case for the pooled regression. Import dependence does not seem to matter much, as it is insignificant for all sectors as well as in the pooled regression. This is in line with the fact that, for some countries, export dependence on primary commodities is significantly higher than import dependence. This is especially the case for oil-dependent countries, such as Russia or Kuwait.

Our results, thus far, indicate that predictability of stock market returns on the basis of commodity sector returns exists across a wide array of different countries and that it is stronger in emerging economies. Trade dependence on commodities, though important for three sectors, does not seem to offer a full explanation of the observed commodity-stock market predictability. This suggests that commodities might convey information that is relevant to stock markets around the world and that goes beyond trade dependence on commodities.

2.4.2. Do commodities carry information beyond trade dependence?

Next, we analyze what kind of information this might be. Previous research has shown that commodity prices facilitate price discovery (e.g., Working (1948), Garbade and Silber (1983), and Hong and Yogo (2012)) and, therefore, may convey relevant information that reaches other markets with a delay. Limited market participation (e.g., Hirshleifer (1988)) can also cause a delay in information diffusion to the markets. Hong, Torous and Valkanov (2007) show that cross-market predictability can be generated between segmented markets such that investors are not able to process all information from both markets (as in models of slow information diffusion among investors with limited information-processing capacity (e.g., Merton (1987a), Hong and Stein (1999)).

Commodity markets have historically been rather segmented from stock markets, being populated by commodity producers and consumers willing to hedge their price risks, and specialized commodity speculators willing to assume that risk for a premium (e.g., Goldstein, Li and Yang (2013), Boons, Roon and Szymanowska (2014)). As such, commodity sector returns may predict stock market returns if they convey information about future changes in market fundamentals or economic activity that takes time for stock market investors to process.

To test this hypothesis, we consider two economic indicators, namely, the inflation rate and the real industrial production growth rate. We use the inflation rate because changes in commodity prices are known to precede changes in general price level, affecting them indirectly as they feed into the production of manufactured goods (Garner (1989)) and directly as they feed into inflation itself (e.g., Breeden (1980), Erb and Harvey (2006), Gorton and Rouwenhorst (2006), Cologni and Manera (2008), and Bekaert and Wang (2010)). The real industrial production growth rate captures the information that is relevant to the users and producers of commodities. For example, Black (1976), Bray (1981), Sockin and Xiong (2015) and Brogaard, Ringgenberg and Sovich (2018) show that futures prices may provide information that is useful to commodity production and processing. Moreover, Kilian (2009), Hu and Xiong (2013), and Sockin and Xiong (2015) argue that commodity markets may also provide information about the future strength of the global economy which, in turn, can also affect commodity production planning as well as stock market fundamentals.

To set the stage, we run predictive regressions similar to equation (2.6), but with the aim of predicting the two economic indicators we consider. In particular, for each of our economic indicators, we run the following long-horizon regression per country:

$$Z_{i,t:t+h} = \alpha_i + \sum_s \lambda_{i,s,h} r^c_{s,t:t-1} + \phi_i X_{i,h} + \mu_{i,t:t+h}, \tag{2.12}$$

where $Z_{i,t:t+h}$ is the growth rate of the economic indicator Z between months t and $t+h$ in country i. For each economic indicator, we run this regression for the three horizons, h, at 1, 6 and 12 months. The controls, $X_{i,h}$, include the lagged excess stock market return of country i, and up to three lags of the economic indicator, as in Hong, Torous and Valkanov (2007). We use Newey-West standard errors with $h+1$ lags.

Table 2.4 summarizes the results. In Panel A, we report the total number of countries for which commodity sectors returns predict future inflation rates and production growth rates at at least one horizon at the 10% significance level for each commodity sector and jointly across sectors. We also repeat the number of significant slope coefficients from predicting stock markets returns as in equation (2.6), but re-estimate them on a sample period restricted to January 1995 to February 2016. This is dictated by the availability of data on trade variables and economic indicators.[14] Finally, at the bottom of that panel, we present the number of countries for which a given commodity sector predicts both the stock market and that country's economic indicator at at least one horizon. Panel B splits the countries for which commodity sectors predict their economic indicators into emerging and developed countries. Panel C divides countries according to whether their ratios of sector-specific exports and imports to GDP is above or below the median.

First, in Panel A, commodity sector returns predict market fundamentals in a wide range of countries. More than 80% of countries see their inflation

[14] We report full results of the estimation on this shorter sample period in Panel C of Table A.4 in the Appendix.

and industrial production growth rates predicted by at least one commodity sector at at least one horizon. Different commodity sectors seem to convey different information about the future state of the economy, however. Energy seems to be the most important predictor of country specific inflation rates, while Agriculture sectors are the most important in terms of predicting industrial production growth rates. While Energy is the most important predictor of inflation rates, Industrial Metals, Agriculture, and Precious Metals also convey information about inflation rates in a large number of countries.

Second, almost all countries in which stock market returns are predicted by at least one of the commodity sectors also see their economic variables predicted by these sectors. For 60 countries, commodity sector returns jointly predict stock market returns and future inflation rates and, for 57 countries, they predict stock market returns and industrial production growth rates. This joint predictability, however, varies per sector and per economic variable. Energy and Industrial Metals also predict stock market returns in at least half of the countries in which they predict both economic variables. The Agriculture I sector predicts stock market returns in around a third of the countries in which it predicts both economic variables, while Precious Metals predicts a half of the stock market returns of countries in which it predicts inflation rates, and a third of the countries in which it predicts industrial production growth rates. All the other sectors jointly predict stock market returns and economic variables in relatively few countries.

Third, the information that commodity sector returns convey about country-specific inflation and industrial production growth rates is spread out across emerging and developed countries (Panel B) and across countries with direct export (import) dependence above and below the median (Panel C). This result reinforces our earlier evidence that neither the development stage of

Table 2.4. Predictive regressions of economic indicators

This table summarizes the results of the following long-horizon regression for each country *i*:

$$Z_{i,t:t+h} = \alpha_i + \sum_s \lambda_{i,s,h} r^c_{s,t:t-1} + \phi_i X_{i,h} + \mu_{i,t:t+h}$$

where $Z_{i,t:t+h}$ is the growth rate of the economic indicator Z between months t and $t+h$ in country *i*. $r^c_{s,t:t-1}$ is the lagged one-month futures market return of commodity sector s. The economic indicator is either the inflation rate, the real industrial production growth rate or excess stock market return. For each economic indicator, we run this regression at the 1, 6, and 12 month horizons. The controls, $X_{i,h}$, include the lagged excess stock market return and three lags of the economic indicator. Panel A displays the number (T) and percentage (%) of countries for which commodity returns have predictive power over a given set of economic indicators for at least one horizon h. Panel B shows country counts by development stage. Panel C shows country counts for subsamples of countries with ratios of imports or exports of a given commodity sector to GDP above or below the sample median. The counts are based on the 10% significance level and Newey-West standard errors with $h+1$ lags. The sample period is from January 1995 to March 2016.

	Energy	Industrial Metals	Agriculture I	Agriculture II	Livestock & Meats	Precious Metals	At least one sector
			Panel A: Full Sample Country Counts				
Inflation T	45	25	26	19	11	29	62
Inflation %	64%	36%	37%	27%	16%	41%	89%
Production T	16	17	32	24	17	15	59
Production %	23%	24%	46%	34%	24%	21%	84%
Returns T	37	45	24	15	33	32	68
Returns %	53%	64%	34%	21%	47%	46%	97%
Ret & Infl T	23	15	10	3	5	16	60
Ret & Prod T	7	12	9	6	5	5	57
			Panel B: Development Stage Subsample Country Counts				
Inflation							
Developing T	28	18	17	14	6	15	41
Developed T	20	6	10	3	2	18	21
Production							
Developing T	8	10	15	15	10	10	21
Developed T	5	10	17	11	7	2	15
			Panel C: Trade Dependence Subsample Country Counts				
Inflation							
Export > Median	22	11	14	8	6	13	
Export < Median	23	14	12	11	5	16	
Import > Median	20	12	14	10	7	18	
Import < Median	25	13	12	9	4	11	
Production							
Export > Median	8	10	15	8	7	10	
Export < Median	8	7	17	16	10	5	
Import > Median	6	7	13	12	8	9	
Import < Median	10	10	19	12	9	6	

the stock markets nor trade dependence can fully explain the predictability we document here.

Now that we established the extent to which commodity market sectors have information about future economic activities, we test whether the predictability of countries' stock market returns is related to the predictability of countries' economic indicators in the following cross-country regression:

$$\beta_{i,s} = \gamma_{0,s} + \Phi_s \lambda_{i,s} + \gamma_e E_i + \gamma_\theta \theta_i + \gamma_{TD,s} TD_{i,s} + \varepsilon_{i,s} \tag{2.13}$$

where $\lambda_{i,s}$ is the average, across the three horizons, of the absolute values of the slope coefficients from predicting our economic variables in the per-country regressions given in equation (2.12), analogous to the way we define $\beta_{i,s}$ in regression (2.11). We control for the country characteristics that we find significant in estimating regressions given in equation (2.11). E_i is a dummy variable that equals one if a country is emerging rather than developed; θ_i is our measure of market efficiency; and $TD_{i,s}$ is the direct trade dependence measure defined in Section 2.1.3. Due to the availability of the trade data, the sample period is restricted to the period between January 1995 and February 2016 and all slope coefficients from our per-country predictive regressions are estimated on this abbreviated sample.

In Table 2.5, we report the results of regression (2.13) for each sector, meaning we use 70 cross-country observations, at most. In the last column, we pool all six sectors and use sector fixed effects to focus on within sector variation. We report the estimated coefficients, their t-statistics, the R^2 of the regressions and the p-values of the test of whether or not all the explanatory variables are jointly different from zero. We also report the number of observations included in each regression. Given that we observe economic indicators for a different number of countries, we first estimate this regression separately for the infla-

Table 2.5. Cross-country regressions: slow information diffusion and the informational content of commodity markets

This table summarizes the results of the following cross-country regression:

$$\beta_{i,s} = \gamma_{0,s} + \Phi_s \lambda_{i,s} + \gamma_e E_i + \gamma_\theta \theta_i + \gamma_{TD,s} TD_{i,s} + \varepsilon_{i,s}$$

where $\lambda_{i,s} = \frac{1}{3}\sum_{h\in\{1,6,12\}} |\lambda_{s,i,h}|$, i.e., the average of the absolute values of the slope coefficients across the three horizons h, obtained by predicting a given economic indicator in the per-country regressions specified in equation (2.12). E_i is a dummy variable that equals zero if a country is developed. θ_i is our measure of market efficiency. $TD_{i,s}$ is the direct trade dependence measure defined in Section 2.1.3. In Panels A and B, $\lambda_{i,s}$ is based on the inflation rate and the real industrial production growth rate, respectively. In Panel C, we include $\lambda_{i,s}$ for both the inflation rate and the real industrial production growth rate as explanatory variables. The last column shows the results of a pooled regression with sector fixed effects and with the imposition of equal coefficient estimates across sectors. The t-statistics are based on heteroskedasticity-robust standard errors.

	Energy	Industrial Metals	Agriculture I	Agriculture II	Livestock & Meats	Precious Metals	Pooled SFE
			Panel A: Inflation				
coefficients							
$\gamma_{0,s}$	0.03	0.05	-0.04	-0.10	0.10	0.05	-0.02
E_i	0.02	0.04	0.00	0.03	0.04	0.00	0.03
θ_i	-0.02	-0.01	0.07	0.11	-0.04	0.00	0.02
Inflation $\lambda_{i,s}$	0.37	0.86	0.82	0.89	1.05	0.44	0.81
$TD_{i,s}$	0.10	-0.11	1.80	0.81	-2.62	-0.11	0.11
t-stats							
$\gamma_{0,s}$	0.47	0.49	-0.45	-1.02	0.73	0.53	-0.55
E_i	3.11	2.62	0.10	2.42	2.08	0.24	6.43
θ_i	-0.28	-0.14	0.81	1.15	-0.30	-0.01	0.63
Inflation $\lambda_{i,s}$	1.65	1.55	1.86	3.39	2.22	1.02	4.58
$TD_{i,s}$	3.17	-0.22	1.71	2.95	-2.09	-0.42	2.69
R^2	32.43%	20.79%	18.87%	38.62%	27.45%	2.46%	22.95%
p(Wald)	0.00	0.01	0.01	0.00	0.00	0.81	0.00
No obs	67	67	67	67	67	67	402

	Energy	Industrial Metals	Agriculture I	Agriculture II	Livestock & Meats	Precious Metals	Pooled SFE
			Panel B: Industrial Production				
coefficients							
$\gamma_{0,s}$	0.01	0.11	-0.03	-0.11	0.22	-0.01	-0.01
E_i	0.02	0.05	0.01	0.05	0.05	0.00	0.03
θ_i	0.00	-0.08	0.06	0.11	-0.16	0.06	0.01
Production $\lambda_{i,s}$	0.24	0.35	0.10	0.09	0.08	0.04	0.09
$TD_{i,s}$	0.08	-0.40	2.58	1.11	-2.26	-0.08	0.10
t-stats							
$\gamma_{0,s}$	0.15	0.89	-0.26	-0.87	1.42	-0.12	-0.16
E_i	2.67	3.37	0.71	3.51	3.19	0.37	8.13
θ_i	-0.05	-0.66	0.53	0.95	-1.07	0.58	0.24
Production $\lambda_{i,s}$	2.35	1.60	0.73	1.07	0.54	0.43	1.85
$TD_{i,s}$	2.68	-0.71	2.34	3.52	-1.70	-0.29	2.52
R^2	35.70%	20.85%	15.36%	38.79%	25.88%	2.04%	19.71%
p(Wald)	0.00	0.01	0.05	0.00	0.00	0.88	0.00
No obs	62	62	62	62	62	62	372

Table 2.5. Cross-country regressions: slow information diffusion and the informational content of commodity markets

(continued)

	Energy	Industrial Metals	Agriculture I	Agriculture II	Livestock & Meats	Precious Metals	Pooled SFE
			Panel C: Inflation and Industrial Production				
coefficients							
$\gamma_{0,s}$	0.00	0.10	0.02	-0.14	0.16	-0.01	-0.02
E_i	0.02	0.03	0.00	0.03	0.03	0.00	0.02
θ_i	0.00	-0.07	0.01	0.13	-0.11	0.05	0.01
Inflation $\lambda_{i,s}$	0.34	1.34	1.05	1.02	1.43	0.32	0.96
Production $\lambda_{i,s}$	0.23	0.33	0.06	0.04	0.04	0.04	0.07
$TD_{i,s}$	0.09	-0.16	1.97	1.11	-2.40	-0.08	0.12
t-stats							
$\gamma_{0,s}$	-0.01	0.83	0.15	-1.23	1.09	-0.05	-0.35
E_i	2.26	2.11	-0.12	2.67	1.97	0.19	5.62
θ_i	0.08	-0.65	0.08	1.27	-0.76	0.47	0.30
Inflation $\lambda_{i,s}$	1.53	2.17	2.12	3.64	2.79	0.70	5.68
Production $\lambda_{i,s}$	2.31	1.53	0.47	0.45	0.30	0.42	1.38
$TD_{i,s}$	2.87	-0.30	1.77	3.90	-1.91	-0.30	2.76
R^2	38.28%	26.99%	21.63%	50.52%	34.95%	2.88%	26.48%
p(Wald)	0.00	0.00	0.02	0.00	0.00	0.89	0.00
No obs	62	62	62	62	62	62	372

tion rates in Panel A and the industrial production growth rates in Panel B. Panel C presents the results when we use both economic variables in one regression.

The results in Panel A show that the country's stock market predictability is significantly related to the predictability of its inflation rates for all sectors separately (with the exception of Industrial Metals and Precious Metals) and in the pooled regression, controlling for other country characteristics. The slope coefficients are all positive, which means that the better a given sector predicts the inflation rate of a country, the better it also predicts its stock market returns. In the pooled regression the effect is extremely statistically significant, with a

t-statistic of 4.58. In contrast, the t-statistic for trade dependence in the pooled regression, albeit high, is much smaller at 2.69.

The predictability of real industrial production growth rates (Panel B) is less strongly related to the predictability of stock market returns, as it is only significant for the Energy sector and in the pooled regression. In fact, the evidence in Table 2.4 also shows that commodity sector returns predict the industrial production growth rate in fewer countries , as compared to the inflation rate. It is also interesting to recall that the Agriculture sector predicted industrial production growth in the largest number of countries. It seems, however, that predictability on the basis of the Agriculture sector is more related to its ability to forecast future inflation rates (Panel A) and whether or not a country's trade depends on Agriculture commodities (Table 2.3), rather than to its ability to predict future industrial production growth.

Panel C shows that the relation between a sector's ability to predict economic indicators and stock market returns is very similar when we include both indicators in the regressions. The main difference is in the Industrial Metals sector. Unlike in Panel A, a country's stock market predictability is now also related to the predictability of its inflation rate by this sector. For all sectors, except Precious Metals, we reject the null hypothesis that country characteristics and commodity sectors' ability to forecast economic variables are not important in terms of explaining cross-country variation in stock market predictability. For five out of six sectors, the R^2 varies between 20% and more than 50%, while in the pooled regression we find an R^2 of 26%. Overall, we find that our country characteristics and the ability of commodity sectors to forecast economic variables explain quite a large fraction of cross-country variation in stock market predictability from commodity sector returns.

2.5. Robustness checks

In this section, we show that our results are robust to the inclusion of stock market predictors from the literature as well as Hong and Yogo's (2012) commodity market open interest predictor. We also provide evidence that our results are not driven by time-varying coefficients, which allows us to rule out explanations that are purely related to the financialization of commodity markets, such as Basak and Pavlova (2016), business cycle variation in predictability (Jacobsen, Marshall and Visaltanachoti, 2018) and other structural breaks and exogenous shocks like crises. Furthermore, we show that our results are robust to controlling for the dollar exchange rate of each country and to using real commodity and stock market returns instead of nominal returns. This helps alleviate concerns that our results are mechanically driven by the inflation component of nominal returns or that the results are confounded by exchange rate predictability.

2.5.1. Controlling for other stock market predictors, exchange rates and inflation

Hong and Yogo (2012) argue that open interest may be more informative in terms of predicting asset returns than futures prices and may, thus, constitute a new channel for information transmission from commodity markets to stock markets. They find that movements in open interest predict commodity returns and to a lesser degree stock, bond and currency returns. In light of this, we control for aggregate commodity open interest in our predictive regressions.[15]

We analyze the role of commodity market interest in Panel A of Table 2.6. Consistent with their weaker results related to stock market predictability, we find that the role of commodity market open interest is rather limited in terms

[15] We follow Hong and Yogo (2012) in constructing the growth rate of commodity market interest.

of being able to predict stock market returns in the countries in our sample. The results of our analysis of predictability on the basis of commodity sector returns remains virtually unchanged when we control for commodity open interest. Moreover, open interest's ability to predict almost always coincides with the ability of sector returns to predict; as such, it does not represent a separate channel of information transmission.

Next, in Panel B, we analyze our evidence of predictability by subjecting our analysis to two types of controls: the well-known country-specific stock market return predictors and predictors of the strength of the global economy.

Given the data limitations inherent to working with so many developing countries, we are only able to consistently use one known predictor of stock market returns across all countries, namely the short rate (e.g., Ang and Bekaert (2007); Rapach, Strauss and Zhou (2013)).[16] To approximate the strength of the global economy, we use two proxies: the Kilian Index (Kilian, 2009) and the S&P 500 futures returns (Hu and Xiong, 2013).[17]

Our results, reported in Panel B, are robust with respect to these additional controls. The average R^2 increases by one and a half percentage points relative to the baseline regressions, which is consistent with the fact that these controls are accepted predictors of stock markets behavior; however, the predictability on the basis of commodity sector returns remains significant in a comparable number of countries.

[16] In unreported results, conditional upon data availability, we experimented with the inclusion of the dividend yield, as well, and found that the results are robust.

[17] We thank Lutz Kilian for making this data available on his website. To overcome the mismatch between the daily frequency of the analysis and the monthly frequency of the Kilian index, we assign the month-t index value to all days of month t. The results are robust to using the Baltic Dry Index (sourced from Global Financial Data), which is available at a daily frequency. We report results using the Kilian index because data for the Baltic Dry Index is only available from 1999 onwards.

Table 2.6. Predictive regressions with additional controls and pooled regressions

This table summarizes the results of the following regression for each country i:

$$r_{i,t:t+19} = \beta_{i,0} + \sum_{s}(\beta_{i,s,1} r^c_{s,t-21:t-2} + \beta_{i,s,2} r^c_{s,t-41:t-22}) + \phi_i r_{i,t-20:t-1} + \kappa_i X + \varepsilon_{i,t:t+19}$$

where X is a set of controls. It shows the number of countries for which monthly stock market excess returns ($r_{i,t:t+19}$) are predicted by sector-s one-month ($r^c_{s,t-21:t-2}$) and two-month ($r^c_{s,t-41:t-22}$) lagged commodity futures returns at the 10% significance level (based on Newey-West standard errors). T and *%* stand for total number and percentage of countries (out of 70). In Panel A, we control for lagged stock returns and open interest growth. In Panel B, we also control for the short rate, the Kilian Index and the S&P 500 futures returns. Both panels A and B show descriptive statistics for the regression R^2 and counts of significant coefficients for at least one sector and for at least one lag. *p(Wald)* $<$ *10%* denotes the number of countries for which we reject the null hypothesis that all commodity sector returns fail to predict stock market excess returns. Panel C shows the coefficient estimates and the Driscoll and Kraay (1998) t-statistics for pooled regressions estimated on groups of countries with the same coefficient sign (P for positive and N for negative) for each commodity sector based on the first two lags. The sample period is from November 1979 to March 2016.

		Energy	Industrial Metals	Agriculture I	Agriculture II	Livestock & Meats	Precious Metals	EWI	GSI	At least one sector
Panel A: Controlling for lagged stock returns and open interest (Country Counts)										
$r^c_{s,t-21:t-2}$	T	21	13	4	12	16	7	7	13	49
	%	30%	19%	6%	17%	23%	10%	10%	19%	70%
$r^c_{s,t-41:t-22}$	T	9	19	11	11	8	10	13	18	47
	%	13%	27%	16%	16%	11%	14%	19%	26%	67%
At least	T	29	27	15	20	22	15	16	25	62
one horizon	%	41%	39%	21%	29%	31%	21%	23%	36%	89%
R^2	mean	5.84%	median	3.88%	max	28.78%	min	1.27%		
p(Wald) $<$ 10%	T	37	53%							

Table 2.6. Predictive regressions with additional controls and pooled regressions

(continued)

		Energy	Industrial Metals	Agriculture I	Agriculture II	Livestock & Meats	Precious Metals	EWI	GSI	At least one sector
Panel B: Controlling for lagged stock returns, open interest, short rate, Kilian Index and S&P 500										
$r^c_{s,t-21:t-2}$	T	22	13	3	12	15	7	7	10	46
	%	31%	19%	4%	17%	21%	10%	10%	14%	66%
$r^c_{s,t-41:t-22}$	T	12	18	15	12	8	10	10	13	51
	%	17%	26%	21%	17%	11%	14%	14%	19%	73%
At least one horizon	T	30	26	18	21	21	15	14	19	66
	%	43%	37%	26%	30%	30%	21%	20%	27%	94%
R^2	mean	7.03%	median	4.68%	max	32.03%	min	1.63%		
p(Wald) $<$ 10%	T	44	100%							
Panel C: Using real returns and controlling for exchange rate (N=54) (Country Counts)										
$r^c_{s,t-21:t-2}$	T	19	10	5	9	10	7	2	11	37
	%	35%	19%	9%	17%	19%	13%	4%	20%	69%
$r^c_{s,t-41:t-22}$	T	9	17	6	8	5	7	7	9	38
	%	17%	31%	11%	15%	9%	13%	13%	17%	70%
At least one horizon	T	26	24	11	15	15	11	9	17	54
	%	48%	44%	20%	28%	28%	20%	17%	31%	100%
R^2	mean	9.13%	median	7.53%	max	30.89%	min	2.08%		
p(Wald) $<$ 10%	T	49	70%							

Table 2.6. Predictive regressions with additional controls and pooled regressions

(continued)

		Energy	Industrial Metals	Agriculture I	Agriculture II	Livestock & Meats	Precious Metals	EWI	GSI	At least one sector
		Panel D: Pooled Regression Estimates (Sign-based Subsamples)								
$r^c_{s,t-21:t-2}$	P: coef	0.03	0.09	0.02	0.08	0.05	0.03	0.06	0.05	
	P: t-stat	1.24	2.68	0.55	2.39	1.86	1.47	1.19	1.58	
	N: coef	-0.06	-0.04	-0.05	-0.04	-0.06	-0.03	-0.03	-0.06	
	N: t-stat	-3.07	-1.65	-1.38	-0.80	-1.17	-0.61	-0.44	-1.89	
$r^c_{s,t-41:t-22}$	P: coef	0.04	0.06	0.07	0.08	0.06	0.03	0.08	0.08	
	P: t-stat	1.46	2.01	2.19	2.59	2.04	1.07	1.81	2.55	
	N: coef	-0.04	-0.03	-0.07	-0.04	-0.09	-0.03	-0.04	-0.03	
	N: t-stat	-1.73	-1.25	-2.09	-0.95	-1.70	-0.36	-0.67	-0.93	
	P: No obs	14	29	15	35	33	39	42	28	
	N: No obs	31	5	24	9	7	6	5	18	

An additional concern is that our results may be driven by the common component of inflation across countries and that inflation is serially autocorrelated (e.g. Ciccarelli and Mojon (2005)). Moreover, recent evidence suggests that commodity returns may be informative for the exchange rates of commodity-exporting countries (e.g. Kohlscheen, Avalos and Schrimpf (2016)). It is thus possible that our tests fail to disentangle stock market predictability from predictability from inflation and/or exchange rate predictability. As shown in Panel C, our qualitative results are robust to using real commodity returns and real stock returns and to controlling for the dollar exchange rate of each country, thus alleviating these concerns.

Our results withstand a range of additional tests. Thus far, our analysis has been based on running 70 per-country predictive regressions, specified in equation (2.6). In Panel D, we show that our results are robust when running pooled predictive regressions instead. We report the estimated coefficients and t-statistics using asymptotic standard errors, calculated following Driscoll and Kraay (1998), which are robust to heteroscedasticity and general forms of cross-sectional and temporal dependence when the time dimension becomes large.

Given the heterogeneity observed in the signs of our slope coefficients, we cannot pool all 70 countries together. Instead, we split our countries based on the signs of the slope coefficients from the country-specific regressions, and estimate pooled regressions on a group of countries with the same sign for each commodity sector based on the first two lags. We report the number of countries that are pooled together for each commodity sector. Depending on the sector, we were able to run the pooled regression on as many as 30-40 countries. The results are consistent with those reported in Panel A of Table 2.2, with Energy, Industrial Metals, Agriculture II and Livestock & Meats predicting stock market returns at one- and two-month lags, Agriculture I predicting at the two-month lag and Precious Metals not predicting.

Table A.4 in the Appendix shows the robustness of running our baseline regression, specified in equation (2.6), country-by-country, for all 70 countries when using non-overlapping observations (Panel A), gross returns (Panel B), and in a sample using only post-1995 observations beyond which point trade dependence data is available (Panel C).

In Table A.5 in the Appendix, we allow for both contemporaneous and lagged commodity sector returns up to three-month lag, in line with evidence on slow information diffusion presented in the literature. For example, Rapach, Strauss and Zhou (2013), and Jacobsen, Marshall and Visaltanachoti (2018) find that about 70-80% of the predictive information across markets and/or industries is incorporated contemporaneously and the rest is incorporated after a delay. In line with these findings, we find a stronger contemporaneous relation between commodity sector returns and stock market returns around the world, with 99% of the countries exhibiting a significant contemporaneous relation between at least one of the commodity sectors and stock market returns. More importantly, our predictive results are robust to including this contemporaneous effect, as more than 60% of our countries see stock market returns that are still predicted by commodity sector returns, with a delay up to three months.

2.5.2. Time-varying stock market predictability from commodity sector returns

Previous literature has identified two reasons for why the commodity-stock market predictability might potentially vary over time: ongoing globalization and integration of markets and business cycle variation.

Historically, commodity markets were thought to be segmented from stock markets, as evidenced by the relative inability of stock market risk factors to explain the cross-section of commodity futures returns (e.g. Dusak (1973), Bessembinder (1992), Bessembinder and Chan (1992), Erb and Harvey (2006)). Investors willing to be exposed to commodities typically did so either via physical investments in commodities or via commodity-related equity investments (Lewis (2007)). Around 2003-2004, however, we observe a sharp increase in the presence of traditionally equity-based institutional investors in commodity futures markets, either directly or via commodity index funds. Tang and Xiong (2012) refer to this as the "financialization" of commodity markets. As a consequence, the relation between commodity and stock markets may have changed.

Tang and Xiong (2012) find that correlations between commodity markets and stock markets in emerging countries that were virtually zero before 2004, started to increase gradually in the 2004-2009 period, peaking at around 60% in 2009. Basak and Pavlova (2016) study a model that features financial institutional investors alongside traditional futures market participants and find that prices and volatilities of all commodity futures increase, as do equity-commodity correlations, due to the presence of financial index traders. Boons, Roon and Szymanowska (2014) show that, after 2004, commodity and stock markets became linked due to investors' need to hedge commodity price risk. Hu and Xiong (2013) find little predictability of East Asian stock markets on the basis of commodity futures returns before 2005 and very strong predictability with positive signs in the period 2005-2012. This increased activity of financial investors in the commodity markets may have also affected the informational content of commodity prices (e.g., Sockin and Xiong (2015), Brogaard, Ringgenberg and Sovich (2018)), though some papers argue that price fluctuations and volatility are still driven by fundamental changes in demand and supply, and are only exacerbated by the inflow of financial traders (e.g. Sanders and Irwin (2010), Stoll and Whaler (2010), Kilian and Murphy (2014), Hamilton and Wu (2015) and Brunetti, Büyükşahin and Harris (2016)).

Finally, predictability may also vary over the business cycle. Jacobsen, Marshall and Visaltanachoti (2018) show that Industrial Metals predict US stock returns with a positive sign in recessions and a negative sign in expansions.

In Table 2.7, we analyze whether or not predictability in our 70 countries and six commodity sectors' returns varies over time. For ease of comparison, we include our main results for the full sample once again in Panel A. Panel B shows that the number of countries whose stock market returns are predicted by our commodity sector returns is either similar or smaller when we restrict our sample to the time period after 2005. This suggests that our evidence of predictability is not an artifact of the financialization of commodity markets.

Panel C presents the Hansen (1992) tests for parameter instability in our baseline predictive regressions. These tests do not require ex ante specified break points, which allows for more flexible testing than does our subsample analysis above. The table presents number of countries for which we find parameter instability, separately for each commodity slope coefficient at each lag and jointly for all commodity sectors at all lags (or all lags for the overall commodity indices). On an individual basis, we see indications of possible instability in very few countries. For the Industrial Metals sector, there is evidence of instability in 10 countries. For all other sectors and lags, we see possible instability for, at most, six countries, and often none. Jointly across sectors and horizons, we find possible instability for two countries, and none when looking at the overall indices.

Table 2.7. Time-variation in the predictive regressions

This table summarizes the results of the following regression for each country *i*:

$$r_{i,t:t+19} = \beta_{i,0} + \sum_{s}(\beta_{i,s,1} r^{c}_{s,t-21:t-2} + \beta_{i,s,2} r^{c}_{s,t-41:t-22}) + \phi_i r_{i,t-20:t-1} + \varepsilon_{i,t:t+19}$$

Panels A through D show the number of countries for which monthly stock market excess returns ($r_{i,t:t+19}$) are predicted by sector-*s* commodity futures returns at either the one-month ($r^{c}_{s,t-21:t-2}$) or two-month ($r^{c}_{s,t-41:t-22}$) lag (based on Newey-West standard errors and 10% significance level). *T*, *%*, *P* and *N* stand for total number, percentage, number of positive and number of negative countries (out of 70), respectively. We also show descriptive statistics for the regression R^2 and counts of significant coefficients for at least one sector and for at least one lag. *p(Wald) < 10%* denotes the number of countries for which we reject the null hypothesis that all commodity sector returns fail to predict stock market excess returns. Panel A shows the full sample counts without sample splits. In Panel B, we report the counts for the period after 2005 using the full sample of 70 countries. In Panel C, we show the number of countries for which we reject the null hypothesis of no time variation in the individual coefficients, as well as the null hypothesis that all coefficients are stable over time using the Hansen (1992) parameter instability test. The sample period is from November 1979 to March 2016.

		Energy	Industrial Metals	Agriculture I	Agriculture II	Livestock & Meats	Precious Metals	EWI	GSI
Panel A: Full sample Country Counts (N=70)									
	T	31	26	15	18	23	14	17	24
At least	%	44%	37%	21%	26%	33%	20%	24%	34%
one horizon	P	6	21	6	15	19	13	17	18
	N	25	5	9	3	5	1	0	6
R^2	mean	5.50%	median	3.83%	max	22.76%	min	1.26%	
p(Wald) < 10%	T	35	50%						

Table 2.7. Time-variation in the predictive regressions

(continued)

		Energy	Industrial Metals	Agriculture I	Agriculture II	Livestock & Meats	Precious Metals	EWI	GSI
Panel B: After 2005 Subsample Country Counts (N=70)									
At least one horizon	T	23	15	9	9	14	18	14	20
	%	33%	21%	13%	13%	20%	26%	20%	29%
	P	6	14	5	8	11	14	14	20
	N	17	1	4	1	4	4	0	0
R^2	mean	8.02%	median	6.64%	max	22.76%	min	3.59%	
p(Wald) < 10%	T	34	49%						
Panel C: Hansen (1992) Parameter Instability Tests (Country Counts)									
1-Month Lag	T	2	1	0	1	0	1	1	3
	%	2.86%	1.43%	0.00%	1.43%	0.00%	1.43%	1.43%	4.29%
2-Month Lag	T	4	9	0	0	0	1	1	3
	%	5.71%	12.86%	0.00%	0.00%	0.00%	1.43%	1.43%	4.29%
Jointly all coeffs	T	2						0	0
	%	2.86%						0.00%	0.00%

2.6. Conclusion

This paper shows that commodity sector returns convey relevant information pertaining to a wide range of countries and their market fundamentals. We analyze predictability in 47 emerging and 23 developed countries and find that 84% of their stock market returns are predicted by returns from at least one of the commodity sectors. In the majority of countries, stock market returns are predicted by two to four sectors. In more than 80% of the countries, commodity sector returns predict economic fundamentals, such as inflation and industrial production growth rates.

In addition, we extend our analysis beyond statistical significance and develop new intuitive measures of the economic importance of commodity-stock market predictability, which allow us to characterize the information content of commodity futures markets in novel and rich ways. We find that the economic importance of predictability is evenly distributed across the countries and sectors we include in our analysis and that this extends beyond the role of commodities in global production and trade. Despite the greater weight of Energy in terms of global production value relative to other sectors and the extreme dependence of some countries on the export and import of Energy, our measures of economic significance show that all sectors are important to a comparable extent. Similarly, even though the US and China, alone, account for over 30% of world GDP, we find that no country accounts for more than 7% of the global stock market variation predicted by commodity markets.

We also explore the economic channels of information transmission. We find strong evidence that the ability of commodities to predict stock market returns is strongly related to the degree to which they aggregate information about inflation rates, even after accounting for stock market efficiency, countries' dependence on commodity trade and stage of development. This is in contrast with the more limited extent to which countries' dependence on commodity trade explains cross-country differences in predictability, which is only relevant in terms of the Energy and Agriculture sectors. Even when accounting for the possibility that indirect dependence on commodity trade may affect predictability through non-commodity trade and financial linkages across countries, we are unable to find evidence that the commodity trade dependence channel plays a major role.

Overall, this paper deepens our understanding of commodity-stock market predictability by leveraging an extensive dataset spanning 70 countries, six commodity sectors and forty years to show that the information flow from commodity markets to stock markets is a pervasive global phenomenon. In addition, our evidence is consistent with the idea that all commodity sectors provide complementary and economically important information for stock markets around the world. This information role extends well beyond simple trade dependence and direct input/output cost channels. Commodity futures markets are able to aggregate macroeconomic information that is dispersed around the globe in a complex manner. Therefore, these markets play a unique and truly global information discovery role in financial markets and the real economy.

Chapter 3

Drawing Up the Bill: Does Sustainable Investing Affect Stock Returns Around The World?

We have witnessed a remarkable growth in sustainable or "Environmental, Social, and Governance" (ESG) investing in the past decade. According to the 2018 Global Sustainable Investment Review, sustainable investing represents 63% of professionally managed assets in Europe and 49% in Australia. Other regions are catching up rapidly. In Japan and the US sustainable investments grew by 38% and 307% between 2016 and 2018, respectively. Institutional investors such as pension funds account for most of this rise, underscoring the social welfare implications of sustainable investing. As such, sustainable investing has drawn the attention of policymakers worldwide - a recent example being the EU's Sustainable Investing Disclosure Regulations (SFDR), an attempt to curb greenwashing.

An important question is how sustainable investing affects stock returns. A popular view in both academic research and the financial industry is that it is possible to attain higher risk-adjusted returns with sustainable investing. For example, Friede, Busch and Bassen (2015) conclude that “the large majority of studies reports positive findings” and thus that “the business case for ESG investing is empirically very well founded”. Some prominent scholars disagree, however. For example, in a recent review Liang and Renneboog (2020) conclude that there is still no consensus about whether ESG investing helps or hurts performance.

Indeed, there are reasons for skepticism. First, many studies use a single ESG database even though these databases differ substantially from one another (e.g., Berg, Koelbel and Rigobon (2020)). Second, studies often use short sample periods, thus being prone to capture temporary outperformance due to, for example, unexpected shifts in investor demand for sustainable assets (e.g., Pastor, Stambaugh and Taylor (2020)). Third, at least a few prominent papers find that sustainable investing hurts performance (e.g., Hong and Kacperczyk (2009), Chava (2014), Bolton and Kacperczyk (2021)), in line with theoretical predictions (e.g., Pedersen, Fitzgibbons and Pomorski (2020), Pastor, Stambaugh and Taylor (2020)). Fourth, studies often focus on the US despite 60% of sustainable investing assets being managed elsewhere (GSIA (2018)). As such, the performance of sustainable investing in less developed equity markets with fewer ESG regulations is unclear. Other geographic factors may play a role too. For example, pro-social European investors may be more willing to trade off financial for social returns than their US counterparts are (e.g., Dyck et al. (2019)).

In short, it is still unclear how sustainable investings affects stock returns around the world. We aim to fill this gap by leveraging a comprehensive dataset covering 9,253 unique stocks traded in 46 countries between 2001 and 2020, and by measuring sustainability using ESG ratings from three major raters (Refinitiv, MSCI, and Sustainalytics). To our knowledge, this is the most comprehensive dataset assembled to date to study the stock market performance of sustainable investing.

We begin our study by examining whether or not ESG ratings predict cross-sectional variation in future stock returns using our global sample. Following Bolton and Kacperczyk (2021) we do so by means of panel regressions of stock returns on lagged ESG ratings. Since there is a strong industry- and country-level component in ESG ratings (e.g., Gillan, Koch and Starks (2021)), we aim to be conservative and include a rich variety of fixed effects.

Our main finding is that there is very little evidence that ESG is related to future stock returns once other stock characteristics and fixed effects are controlled for. This finding holds for all three rating agencies, in different time periods, within different sectors of economic activity, within different regions, and when using the global sample. Strikingly, the only rating that shows predictive power for future stock returns in the global sample is MSCI's *G* rating. The economic significance of this effect is also small. We estimate that increasing the *G* rating by one standard deviation is associated with 0.55% greater annualized returns. Moreover, this finding is not robust across alternative specifications.

We also explore the possibility that ESG ratings can be combined into *Composite* ratings that predict future returns better than individual ratings do. Following Serafeim and Yoon (2021), we create *Composite* ratings for each ESG dimension (E, S, G, ESG) by averaging over the three individual raters along the relevant dimension. Whether or not *Composite* ratings predict better is theoretically unclear. On the one hand, *Composite* ratings can have more predictive power for future value-relevant news than individual raters have by canceling out noise (e.g., Serafeim and Yoon (2021)). It is also conceivable that sustainability-minded investors as a group demand more of a stock when various raters give that stock a high rating, making it more likely that non-pecuniary preferences are priced in (e.g., Fama and French (2007)). This scenario is more likely to happen when *Composite* is high. On the other hand, *Composite* ratings may (i) mostly capture accessible information that is quickly incorporated into prices, (ii) cancel out the disagreement component of ratings that has predictive power for risk premia (e.g., Gibson, Krueger and Schmidt (2020)), or (iii) be noisy predictors of future returns. In line with the latter explanations, we find little evidence that *Composite* ratings are informative about future returns.

To understand whether or not sustainable investing affects returns differently around the world, we study the performance of sustainable investing within different regions: Asia-Pacific, Europe, Emerging countries, Japan, and North America. We conduct this analysis for the full sample period (2001-2020) but we also conduct separate analyses for the 2001-2012 and the 2013-2020 periods. Isolating the post-2013 period accommodates the possibility that the large inflow of funds into high ESG assets during this period might have led to temporary outperformance of sustainable investing (e.g., Pastor, Stambaugh and Taylor (2020)).

Overall, we find that our previous results using the global sample often hold when considering each region separately. There are, however, three nuances that occur during the post-2013 period. First, G ratings positively predict future stock returns in North America when we use MSCI and Sustainalyics ratings, but not when we use Refinitiv or *Composite*. This result is consistent with Pedersen, Fitzgibbons and Pomorski (2020) who find similar results for the US using a proxy for G based on accounting accruals. One possible explanation is that the ESG boom in the post-2013 period made investors more attentive to E and S at the expense of G.

Second, higher E is associated with lower future returns in Europe. This effect, however, is only significant at the 10% level post-2013 using MSCI ratings and over the full sample period using either MSCI's or Sustainalytics' ratings. Given the rising prominence of climate change concerns among Europeans, this result may reflect environmentally-minded investors accepting lower returns to hold green assets (e.g., Pastor, Stambaugh and Taylor (2020)).

Third, stocks with higher E and S ratings tend to perform better in emerging countries post-2013. The effect sizes are not negligible. For instance, a one standard deviation increase in MSCI's (Sustainalytics') E ratings is associated with 2.14% (1.48%) greater annualized returns. This result is consistent with Friede, Busch and Bassen (2015). In their meta-analysis, 65.4% of the underlying studies find a positive relation between sustainability and financial performance in emerging markets, almost double the 38% figure for developed markets.

Although we want to be cautious not to overinterpret individual results in a rich set of empirical analyses, we believe there may be good economic reasons for this finding. For example, the fact that emerging countries tend to have less efficient stock markets (e.g., Levine (2005)) makes it more likely that the information that ESG ratings may contain about future firm fundamentals is incorporated in prices with a delay. Emerging markets may also be particularly inefficient in processing ESG information because cross-rater disagreement in ESG ratings may be particularly high in these markets. This may lead to value-relevant information in ratings being incorporated more slowly into prices (e.g., Serafeim and Yoon (2021)). Our results might also be partly driven by investors not wanting to pay a premium to hold high ESG stocks with high cross-rater disagreement (e.g., Avramov et al. (2021)).

In line with these ideas, our summary statistics indicate that cross-rater disagreement tends to be high in emerging countries. For example, the full sample correlations between the E ratings of Sustainalytics-MSCI (Refinitiv-MSCI) are 26.56% (12.84%) in Latin America and 13.04% (11.30%) in Emerging Europe, Middle-East and Africa. The equivalent figures in Europe and North America are 31.70% (33.16%) and 38.28% (30.73%).

Our next goal is to analyze the extent to which the performance of sustainable investing varies across sectors of economic activity - a topic on which consensus is scant (e.g., Coqueret (2021)). We find that investors can pursue sustainable investing within any sector without affecting returns. The exception to the rule is the energy sector, where firms with higher G tend to have higher returns. This result is robust across ESG raters and is economically meaningful. A one standard deviation increase in G is associated with an annualized gain between 1.90% and 3.71% depending on the rater. Although we do not rule out this is a type I error, we note that the result is consistent with (i) the idea that investors may fail to fully appreciate the information that G has about future returns (e.g., Pedersen, Fitzgibbons and Pomorski (2020)), and (ii) the evidence in Giroud and Mueller (2010) that good governance is more likely to create value in non-competitive industries such as the energy industry.

Next, we evaluate the effects of screening out stocks with low ESG ratings. This is important because negative screens are still the most frequent sustainable investing strategy (e.g., Amel-Zadeh and Serafeim (2017)). This analysis also attenuates the concern that we may be overlooking a potentially important non-linearity in the relation between ESG ratings and returns. We consider both the case in which stocks perform worse than their sector peers do and the case in which they perform worse than the rest of the stocks in our sample do.

We find that low ESG stocks tend to perform worse than their better rated counterparts do, but this effect is often imprecisely estimated. A possible explanation is that low ESG ratings are noisy predictors of future value-destroying ESG incidents (e.g., Serafeim and Yoon (2021)) that generate adverse stock price reactions (e.g., Krüger (2015), Glossner (2018)).

These results suggest that investors may have been able to divest from low *ESG* stocks without sacrificing returns. We thus find little support for the hypothesis that negative screens hurt performance by putting downward pressure on prices and by leading investors to demand a risk premium for bearing diversifiable risks (e.g., Heinkel, Kraus and Zechner (2001), Hong and Kacperczyk (2009)). Rather, our results provide some support for models suggesting negative screens are likely ineffective (e.g., Davies and Wesp (2018), Broccardo, Hart and Zingales (2020)).

Throughout the paper we also repeat most analyses based on changes in ESG ratings (ESG momentum) following Nagy, Kassam and Lee (2016). We do not find robust evidence that ESG momentum predicts cross-sectional variation in stock returns.

Overall, our findings suggest that sustainable investing did not hurt performance during the last two decades. Three important implications follow from this. First, our results suggest that sustainable investing did not systematically reduce the cost of equity of sustainable firms in the past. This casts doubt on the view that sustainable investing is a reliable solution to push firms to internalize climate and social externalities (e.g., Pastor, Stambaugh and Taylor (2020), Fama (2021)), at least if we consider the past to be indicative of the future. Second, it might be possible to "do good without doing poorly". In other words, sustainable investing may not always imply worse risk-adjusted performance even over relatively long-periods of time. Third, the flat relation between ratings and returns also has a positive aspect, which is that the valuations of high ESG stocks may not be excessive - at least yet.

We caution, however, that our findings are silent about whether or not the documented relation between ESG ratings and stock returns will persist going forward. For example, one scenario is that investors' non-pecuniary preferences for sustainability are priced in, resulting in the underperformance of sustainable stocks. Innovations in the ESG ratings industry might also lead to a more timely release of ratings that are more informative about future profitability, which could lead to a temporary outperformance of ESG strategies. Over the long-term, however, if ratings improvements reduce uncertainty about the future payoffs of high ESG stocks the most, models of incomplete information would predict lower expected returns for these stocks (e.g., Merton (1987b), Avramov et al. (2021)). A reduction in greenwashing due to improvements in the informativeness of ESG ratings and increased accountability of socially responsible investment managers may also lead to lower returns by making it easier for pro-ESG preferences to be priced in (e.g., Liang, Sun and Teo (2020)).

We contribute to the vast literature studying the performance of sustainable investing strategies. Despite a wealth of research on this topic, we have not yet reached a consensus. For each of the many papers that find a negative relation between sustainability and stock returns (e.g., Fornell et al. (2006), Hong and Kacperczyk (2009), Edmans (2011), Bolton and Kacperczyk (2021)) there is another paper that finds the opposite (e.g., Derwall et al. (2005), Kempf and Osthoff (2007), Statman and Glushkov (2009), Khan, Serafeim and Yoon (2016)). This lack of consensus in the literature arises at least partly because these studies differ in quality, geographic coverage, time period studied, and sustainability measures used. We try to tackle these limitations by studying the performance of sustainable investing using different measures of sustainability over a long time period and many geographies and industries, all under the umbrella of the same methodological framework.

We also contribute to the relatively young literature that studies disagreement in ESG ratings. A few papers show that correlations of ESG ratings across different raters are fairly low (e.g., Chatterji et al. (2016), Berg, Koelbel and Rigobon (2020), Gibson, Krueger and Schmidt (2020), Avramov et al. (2021), Serafeim and Yoon (2021)). However, all of these studies focus on the US market, often over a short-time period. For example, Berg, Koelbel and Rigobon (2020) focus on a single year and Gibson, Krueger and Schmidt (2020) study the period 2010-2017. Our contribution is twofold. First, we show that cross-rater disagreement is not a US phenomenon but rather a global phenomenon that applies to rated firms all over the world. Second, we show that there is some geographic variation in cross-rater disagreement, with disagreement often being higher in some emerging countries.

3.1. Data and summary statistics

In this section we detail how we construct the dataset (Section 3.1.1) and present summary statistics (Section 3.1.2).

3.1.1. Data and sample

We construct a global dataset of monthly stock returns covering the period from January 1999 to December 2020. This period is chosen to match the availability of ESG data. The data is sourced from The Center for Research in Security Prices (CRSP), Compustat North America, and Compustat Global. According to Compustat and CRSP, together these databases cover over 98% of the worlwide market capitalization and are survivorship bias-free. We limit the sample to securities traded in major stock exchanges to attenuate concerns about stock illiquidity. Since the literature uses different criteria to define whether or not a stock exchange is minor, we limit our discretion by defining an exchange as minor if both Bessembinder et al. (2019) and Chaieb, Langlois and Scaillet (2021) do so.[18]

We compute stock returns and clean the data using the procedures detailed in Bessembinder et al. (2019). We retain securities classified as common or ordinary shares and exclude depositary receipts, preferred stock, warrants, investment funds, and investment trusts. To avoid double counting we select the primary issue for each firm using Compustat flags. In case there is more than one primary issue we retain the issue with the longest listing period and, if a tie remains, we select the issue in the headquarter country. The remaining filters include, among others: (i) the exclusion of stock-months with fewer than five daily observations with positive closing prices, (ii) the exclusion of stocks with fewer than six months of coverage, (iii) textual algorithms applied to firm names and business descriptions to identify and exclude misclassified non-common stock and investment funds and trusts, (iv) one algorithm to correct decimal errors in the data (e.g., 34.5 instead of 3.45), (v) various filters to correct mistakes (e.g., abrupt jumps) in the time-series of the number of shares outstanding, stock returns, and market capitalization, and (vi) adjustments for delisting returns and inactive securities that are erroneously flagged as active. Despite their comprehensiveness, these filters correct market capitalization and stock

[18]We acknowledge that many papers use Datastream as a source of data for international stock returns. We use Compustat Global instead. Our main motivation is that Chaieb, Langlois and Scaillet (2021) conduct an in-depth comparison of both databases and conclude that Compustat Global has considerably fewer errors than Datastream. Compustat Global also differs from Datastream in that it provides historical identifiers and it distinguishes between types of daily quotes (e.g., the difference between a closing price and a price that is carried forward). An additional advantage of Compustat Global is that accounting data is normalized and made comparable across countries by taking into account cross-country differences in accounting principles. Datastream has the advantage of having the 1970s as the starting date for the time-series of stock returns in some countries. The equivalent series in Compustat start in the 1980s. This is, however, inconsequential for our purposes because ESG ratings data does not exist in the 1970s. The first rater, Vigeo Eiris, was founded in 1983 (e.g., Berg, Koelbel and Rigobon (2020)).

returns data in fewer than 2% of the observations not excluded by the filters (Bessembinder et al. (2019)). For the full list of filters refer to Bessembinder et al. (2019).

In addition, we employ two additional filters outlined in Chaieb, Langlois and Scaillet (2021): (i) security name screens which correct misclassifications of security types in Compustat, and (ii) manual data corrections.[19] Once all the filters are applied the dataset covers 6,567,124 stock-month observations corresponding to 57,655 stocks traded in 65 exchanges spread over 50 countries.

We expand the dataset to include several standard variables used as controls in studies of cross-sectional predictability of international stock returns (e.g., Hou, Kho and Karolyi (2011), Fama and French (2017), Bolton and Kacperczyk (2021)), such as *size*, B/M, gross profitability, and momentum. Appendix Table B.1 provides detailed variable definitions and sources. As in previous literature (e.g., Fama and French (1992, 2017)), we assume it takes at least six months for fiscal year-end accounting data to become publicly available. Hence, we match stock return data between July of year t+1 and June of year t+2 to the latest fiscal year-end accounting data available at the end of calendar year t. *Size* (natural logarithm of market capitalization) is measured at the end of the calendar year prior to the year in which stock returns are measured. The values of market capitalization used to compute the book-to-market ratio are measured as of the end of the calendar year in which the fiscal year-end of the book value of

[19] An example of a misclassification that is corrected by the first filter of Chaieb, Langlois and Scaillet (2021) is that of Brazilian depositary receipts that are misclassifed as common stock. An example of a manual data correction done in the second filter is the following: "There is an error in the adjustment factor (*ajexdi*) [for one specific security] from 01/09/2007 to 20/03/2007, it should be 1 instead of 10, verified on Bloomberg". Another example of a manual data correction is the following: "The Canadian stock is delisted on January 1st 2017, there is a spike in the price on December 30th, 2016, and the time series ends on December 2nd, 2016, on Bloomberg. We remove it for December 2016. CSXF is also missing the total return adjustment for the 100-to-1 conversion on November 1st, 2013, which creates a 100+ % return. We remove it for November 2013". For the full list of 20 manual corrections refer to Chaieb, Langlois and Scaillet (2021).

equity measurement falls in. All control variables are winsorized at the 0.5% and 99.5% levels based on the whole sample distribution.

We merge the global dataset of monthly stock returns with the following ESG ratings products: MSCI Intangible Value Assessment (MSCI IVA), Refinitiv ESG, and Sustainalytics' ESG Risk Ratings. These rating products have been widely used in the literature (e.g., Berg, Koelbel and Rigobon (2020), Serafeim and Yoon (2021)). We use them for three reasons. First, they have wider global coverage relative to competitors. Second, they are among the most widely used ESG ratings by investors, with a recent survey finding Sustainalyics, MSCI and Refinitiv being ranked by investors as the first, third, and eight most useful ESG ratings, respectively (SustainAbility, 2020).[20] Third, these raters have different countries of origin which can lead to cross-rater differences in ESG ratings. For example, Eccles, Lee and Stroehle (2020) argue that Dutch rater Sustainalytics is more stakeholder-oriented than US counterpart MSCI due to their different origins. Another example is Gibson et al. (2020) who argue that raters from civil law countries (e.g., Sustainalytics and Refinitiv) may have a comparative advantage in identifying financially material social issues while raters from common law countries (e.g., MSCI) may rate better on governance dimensions. By choosing raters that differ along these dimensions, our analysis allows for cross-rater heterogeneity that might affect the returns to sustainable investing.

We use the separate environmental (E), social (S), and governance (G) dimensions of ESG provided by each rater. We re-scale the ratings to range from zero to 100 by multiplying by 10 when necessary as in previous literature (e.g., Christensen, Serafeim and Sikochi (2021), Serafeim and Yoon (2021)) and obtain ESG (ESG) ratings by averaging over E, S, and G. In the case of Sustain-

[20]Note that in the report Refinitiv ESG is identified as *Thomson Reuters ESG Scores*. The inconsistency arises because Thomson Reuters ESG was renamed Refinitiv ESG after the spin-off of Thomson Reuters' Financial & Risk Unit.

alytics and MSCI we use weighted averages that reflect the raters' assessment of the relative importance of each dimension for a given firm at a given point in time. In the case of Refinitiv such weights are not provided and we use a simple average. To ensure that ESG information is available as of December of the year prior to the year in which stock returns realize, we follow previous literature (e.g., Lins, Servaes and Tamayo (2017), Albuquerque et al. (2020),) and match the most recent ESG ratings available as of the end of year *t-1* to stock returns in year *t+1*. The ESG data used in this paper covers the period 1999 to 2018 and stock return data covers the period 2001 to 2020. We also compute ESG momentum variables for each dimension of ESG following Nagy, Kassam and Lee (2016). These are defined as the year-on-year change in ratings.

To allow for the possibility that combining information across raters enhances the predictive power of ESG ratings, we also create a fictitious rater (*Composite*) by separately averaging each dimension (*E*, *S*, *G*, and *ESG*) across raters. To attenuate the problem that statistical distributions of ratings differ across raters, we follow Gibson, Krueger and Schmidt (2020) and convert the ratings at each point in time to percentile ranks before averaging.

To ensure a minimum standard of representativeness in each country we only retain stocks traded in countries for which there is ESG data for at least 10 stocks during the sample period. This criterion excludes Jordan, Oman, Sri Lanka, and United Arab Emirates. The final dataset covers 9,253 stocks in 46 countries and operating in 73 industries from January 2001 to December 2020.[21] The coverage differs substantially across raters, however. The MSCI subsample is the most comprehensive. Its starting date is between January 2001 and January 2003 for most countries and covers 8,291 stocks in 45 countries. The Refinitiv subsample starts in January 2004 and includes 6,593 stocks in

[21] We use historical six-digit Global Industry Classification Standard Codes (GlCS) codes.

43 countries. The Sustainalytics subsample is smaller, starting in January 2011 for most countries and encompassing 4,371 stocks in 38 countries. Appendix Table B.2 reports the number of unique firms/stocks, industries, and sample period starting dates for each country-rater pair.

3.1.2. Summary statistics

Figure 3.1 summarizes how the unique firms/stocks in our sample are distributed across world regions and countries. 80% of the stocks are traded in developed countries and 20% in emerging countries. North American stock exchanges are home to 42% of sample stocks, followed by Europe and Emerging Asia with shares of 21% and 14%, respectively. Japan constitutes 8% of the sample stocks and the remaining stocks in other Asia-Pacific countries sum up to 9%. The remaining 6% of stocks are equally spread between Latin America (3%) and Emerging Europe, Middle-East and Africa (3%).[22]

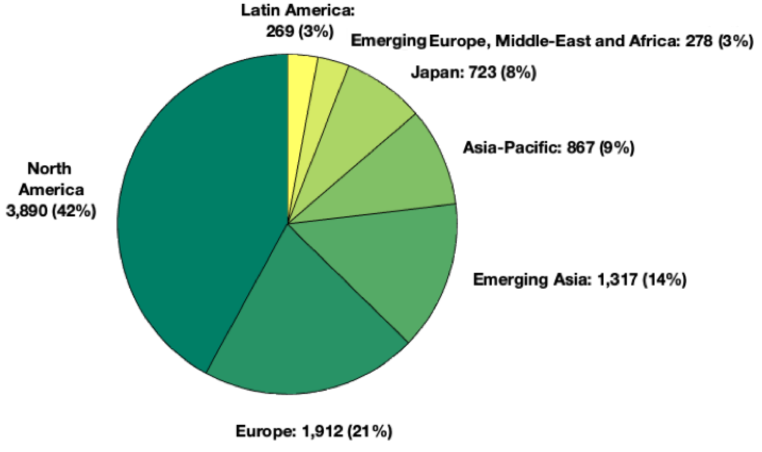
The distribution across countries is even more uneven, with 80% of sample stocks listed in 11 of the 46 countries. Other than the US which accounts for 38% of sample stocks, the most well-represented countries are Japan (8%), UK (7%), China (5%), Australia (4%), Canada (4%), Hong Kong (4%), India (3%), Germany (2%), Sweden (2%), and France (2%).

The heatmaps in Figure 3.2 illustrate the geographic distribution of 2018 *ESG* ratings across countries for the three raters and the cross-rater combination *Composite*. Since *ESG* changes slowly over time and coverage differs across raters, we use the last year of data to maximize cross-sectional coverage

[22] We separate Asia-Pacific from Japan throughout the paper to accommodate the finding in the literature that cross-sectional variation in Japanese and non-Japanese Asia-Pacific stock returns are explained by different asset pricing models (e.g., Fama and French (2017)). Japan is also often singled out as the most notorious exception to the ubiquity of momentum premia around the world (e.g., Fama and French (2012), Asness, Moskowitz and Pedersen (2013)), even though though some authors disagree with this interpretation of the evidence (e.g., Asness (2011)). The use of the region Emerging Europe, Middle-East and Africa (EEMA) follows Chaieb, Langlois and Scaillet (2021).

Figure 3.1. Sample coverage by region and country

The pie chart on top shows the number and percentage of unique firms/stocks in our sample that are publicly traded in each world region between January 2001 and December 2020. The bottom pie chart shows the number and percentage of sample stocks that are publicly traded in each country during the same period. To ensure readability only the 11 countries with most stocks are shown. The category *Other Countries* refers to the remaining 35 countries in the sample. For the complete list of countries and summary statistics per country and region see Appendix B.2.



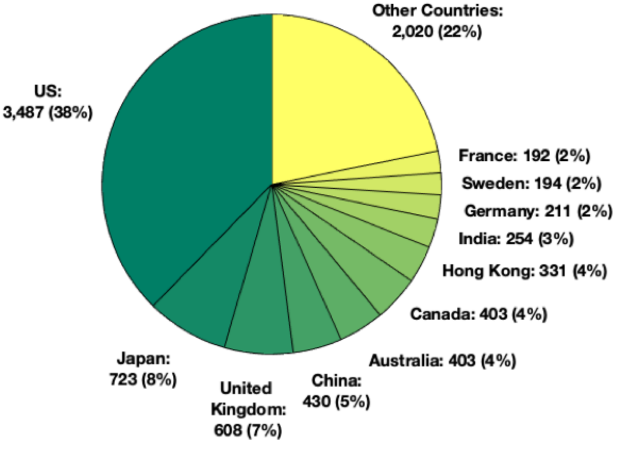


Figure 3.2. Geographical distribution of *ESG* ratings around the world in 2018

The figure shows the average *ESG* ratings/scores per country in 2018 according to three raters (Refinitiv, MSCI IVA, Sustainalytics) and a fictitious rater *Composite*. *Composite* combines the available *ESG* ratings of the other three raters by averaging over their ratings. We convert the ratings of the other three raters at each point in time to percentile ranks before averaging. The green end of the spectrum indicates high *ESG* ratings and the yellow end indicates low *ESG* ratings. For a list of average *ESG* ratings per country see Appendix Table B.3.

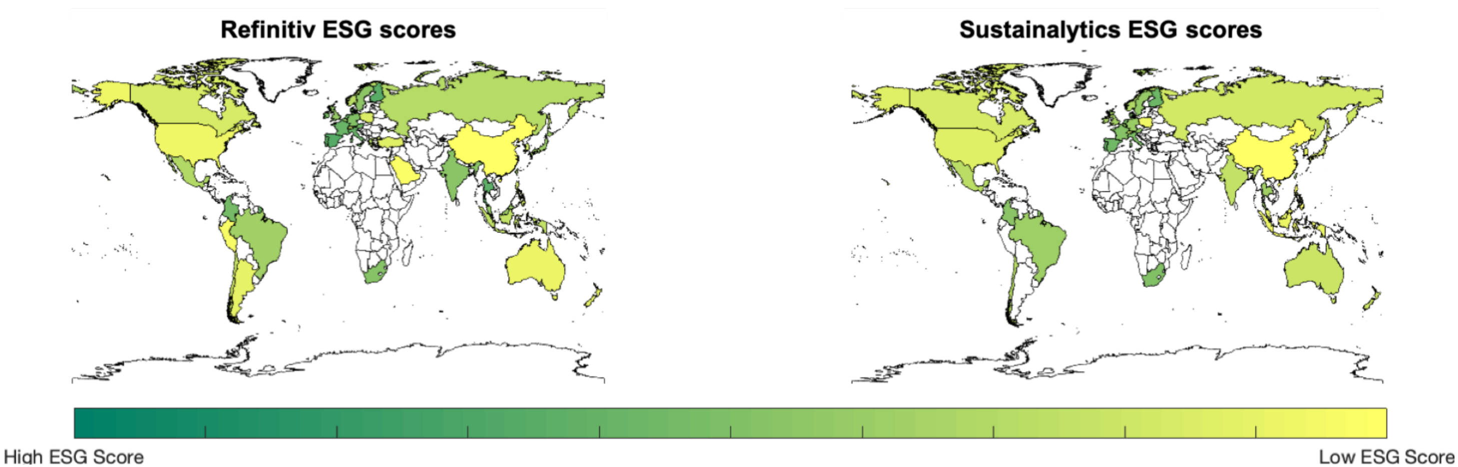
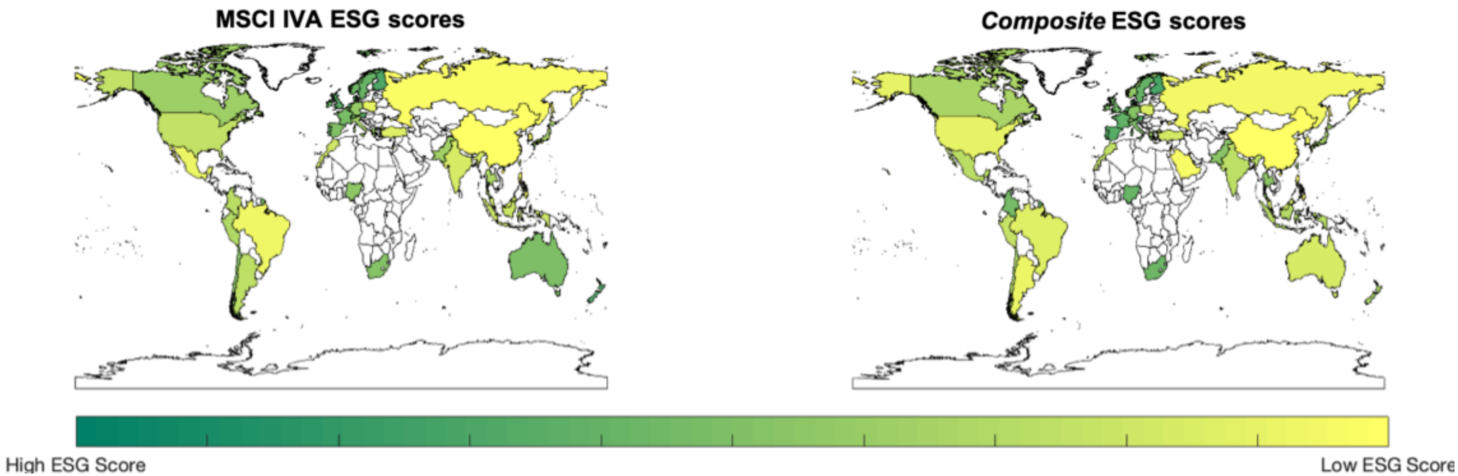


Figure 3.2. Geographical distribution of ESG ratings around the world in 2018

(continued)



and comparability across raters. A detailed table with mean values of *ESG* by country, region, and rater covering the entire sample period are provided in Appendix Table B.3.

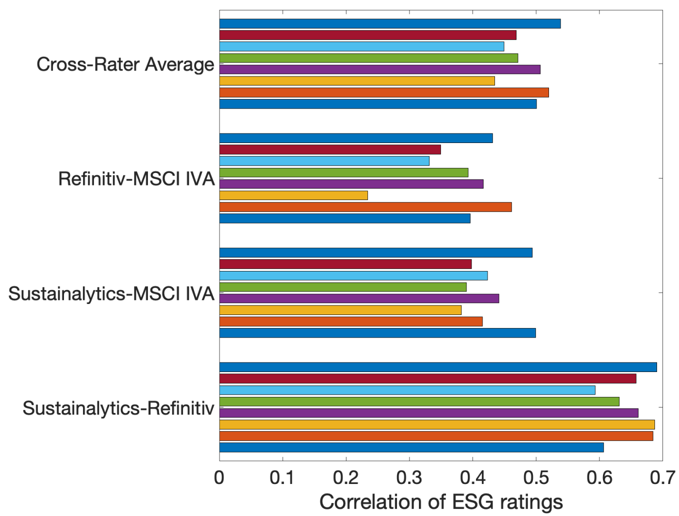
Comparing the heatmaps we see that ESG ratings tend to be highest in non-Eastern European countries, with Finland often topping the list, and lowest in some emerging countries such as China, Russia, and the Philippines. The US and the UK tend to have medium-low and medium-high ratings, respectively. This is consistent with the findings in Liang and Renneboog (2017) that ESG is lowest in countries with a strong history of socialism and highest in civil law countries. The latter countries typically have stronger pro-social community norms, lower likelihood of shareholder litigation against management, and stronger labor and environmental regulations - all forces likely to favor ESG (e.g., Liang and Renneboog (2017), Dyck et al. (2019), Ilhan et al. (2020)).

There is, however, substantial disagreement across raters in many cases. For example, Brazil and India's MSCI ratings are noticeably lower than those of the other two raters. Even more striking is that Australia's Refinitiv ratings place it on par with the US whereas MSCI ratings suggest Australia's ESG performance is close to that of non-Eastern Europe.

To better understand the extent to which raters' ESG ratings disagree, we compute the correlation between the ESG ratings of different raters within geographic regions and worldwide. Since ESG data coverage increases over time we follow Gibson, Krueger and Schmidt (2020) and compute correlations over the full sample period instead of computing the time-series means of within-period correlations. Figure 3.3 shows the results. Four findings emerge. First, for every dimension of ESG and for every region, the average correlation across raters is relatively low, often in the 0.1-0.5 range. Second, there is wide variation in correlations across rater pairs, with Sustainalytics and Refinitiv

Figure 3.3. Full sample correlations of ESG ratings among raters

The figure shows full sample correlations of ESG ratings across rater-pairs (*Refinitiv-MSCI IVA*, *Sustainalytics-MSCI IVA*, and *Sustainalytics-Refinitiv*) for each world region and worldwide. We also show the average correlations across all three rater-pairs (*Cross-Rater Average*). Each plot shows the correlations for one type of ESG rating: environmental (*E*), social (*S*), governance (*G*), and ESG (*ESG*). The correlations are computed using all firms in our sample and over the full sample period encompassing ESG ratings from 1999 to 2018. The starting point of each time-series differs across raters and countries as indicated in Appendix Table B.2.



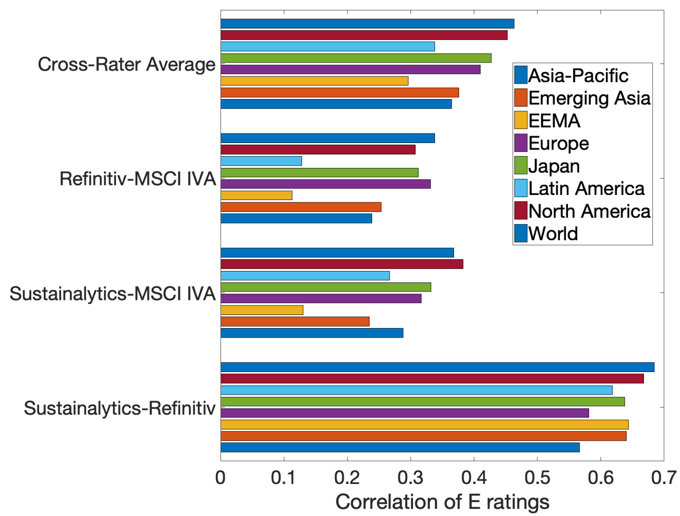
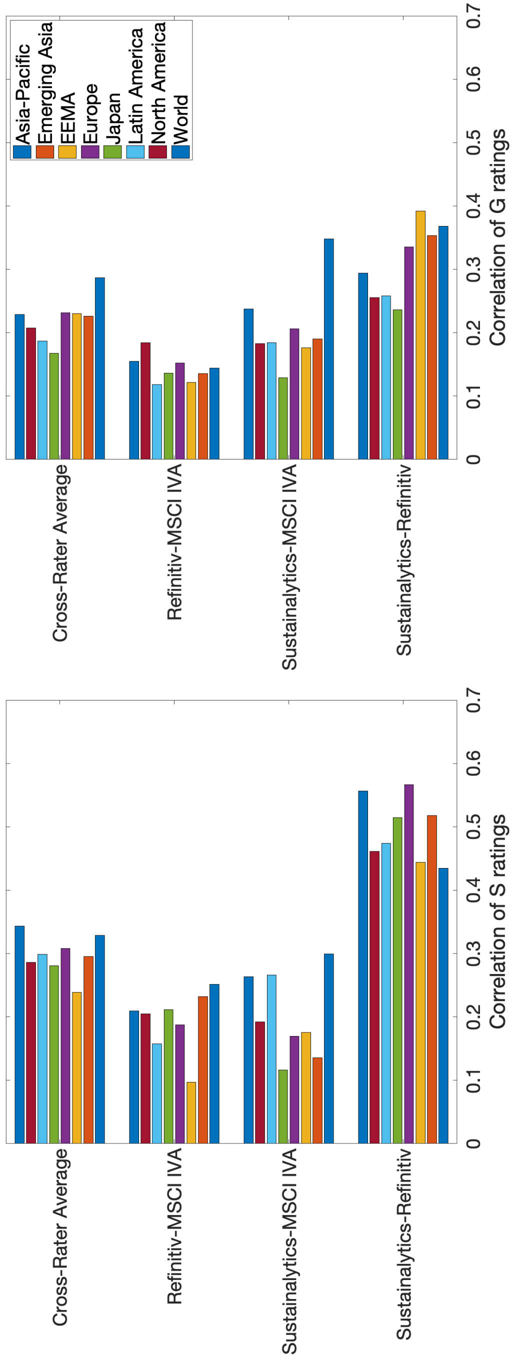


Figure 3.3. Full sample correlations of ESG ratings among raters

(continued)



displaying correlations close to 0.7 in some cases. MSCI diverges the most from the other two raters, with correlations often fluctuating between 0.1 and 0.4. This is consistent with Berg, Koelbel and Rigobon (2020) who find, based on a smaller US-only sample, that MSCI's choices of which ESG attributes to measure are the most idiosyncratic amongst six leading raters (e.g., whether or not to include electromagnetic radiation as part of *E*). Third, there is a tendency for raters to agree the most on the ratings of European and Asian firms and disagree the most with respect to firms in Latin America and Emerging Europe, Middle-East, and Africa (EEMA). This might be driven by cross-country differences in the quality of ESG disclosure.

Fourth, and also consistent with the findings obtained by Berg, Koelbel and Rigobon (2020) and Gibson, Krueger and Schmidt (2020) for the US, average correlations across raters are highest for *ESG* (0.43-0.54 depending on region), followed by *E* (0.3-0.46), *S* (0.24-0.34), and *G* (0.17-0.29). This pattern is to be expected since *ESG* ratings are likely to average out noise and *E* is often more objective than *S* and *G* (e.g, Gibson, Krueger and Schmidt (2020)). For example, whereas carbon emissions are an objective measure that most raters would agree should be used as pollution metric, it is less clear whether gender parity is better captured by gender seniority gaps, gender pay gaps, or the number of sexual harassment lawsuits. *G* is also prone to disagreement since (i) optimal governance arrangements are likely to differ across firms and countries (e.g., Bebchuk and Hamdani (2009), Black, Carvalho and Érica Gorga (2012), Homanen and Liang (2018)), and (ii) there is continuous ongoing debate about the nuances of several governance arrangements, perhaps most prominently about CEO compensation contracts (e.g., Bertrand (2009), Edmans and Gabaix (2009), Geiler and Renneboog (2010)) and board structure (e.g., Ferreira (2010), Adams (2016), Amihud, Schmid and Solomon (2017)).

Table 3.1 presents summary statistics for monthly returns and all control variables used throughout the paper. We report mean, medians, and standard deviations for the four samples we conduct empirical analyses on: Refinitiv, Sustainalytics, MSCI, and *Composite*. In Panel A we do not detect pronounced differences in the summary statistics across raters. Stocks in the Sustainalytics subsample are on average larger than those in MSCI, which is to be expected given that MSCI covers a much wider cross-section of stocks. This might also be partially driven by the fact that the Sustainalytics subsample starts 10 years later than MSCI's and the fact that average stock market capitalization, at least in the US, has been increasing at a fast pace since the 1980s (e.g., Gabaix and Landier (2008), Gutiérrez and Philippon (2017)). The average monthly returns and momentum returns for Sustainalytics are also slightly smaller than those of the other two raters, which squares with the evidence that returns and the momentum returns are larger among smaller stocks (e.g, Fama and French (2012), Asness, Moskowitz and Pedersen (2013), Israel and Moskowitz (2013)).

3.2. Methods

We study the extent to which various ESG metrics have information about future stock returns around the world. We do so by running panel regressions of monthly stocks returns on lagged ESG metrics available at the end of the previous year and a set of control variables. To focus on cross-sectional variation in stock returns we follow Bolton and Kacperczyk (2021) and include month fixed effects in all our regressions.

The ESG metrics are either the ESG ratings or the year-on-year changes in ESG ratings (ESG momentum) attributed to each firm by one of the four raters (Refinitiv, Sustainalytics, MSCI, and *Composite*) in one of the four ESG dimensions (*ESG*, *E*, *S*, and *G*). For a description of the control variables refer to Appendix Table B.1.

Table 3.1. Summary statistics

This table reports the mean, median, and standard deviation of stock returns and various stock characteristics (Panel A), ESG ratings (Panel B), and ESG momentum variables (Panel C). Variable definitions are available in Appendix Table B.1. The table also reports the starting date, the number of unique firms/stocks, and the number of stock-months in the sample. These statistics are provided for the full sample and for subsamples. The subsamples restrict the sample to those stock-months in year *t+1* for which ESG ratings from a specific rater are available in year *t-1*. This results in a subsample for each rater: the Refinitiv subsample, the Sustainalytics subsample, and the MSCI IVA subsample. The full sample is obtained by constructing a fictitious rater (*Composite*) which combines the available ratings of the other three raters. This is done by averaging the ratings across the other three raters. We convert the ratings of each of these three raters at each point in time to percentile ranks before averaging. There are four possible types of ESG ratings for any given rater: environmental (*E*), social (*S*), governance (*G*), and ESG (*ESG*). The full sample period is January 2001 to December 2020.

	Refinitiv Sample			Sustainalytics Sample			MSCI IVA Sample			*Composite Sample*		
No. Stocks	6,593			4,371			8,291			9,253		
No. Stock-Months	553,388			335,582			578,089			730,984		
Start Date	2004-Jan			2011-Jan			2001-Jan			2001-Jan		
Variable	Mean	Median	S.D.	Mean	Median	S.D.	Mean	Median	S.D.	Mean	Median	S.D.
Panel A: Stock Returns & Characteristics												
Returns (in % p.m.)	0.83	0.73	11.14	0.73	0.67	10.23	0.79	0.67	11.24	0.81	0.67	11.36
Size	8.21	8.22	1.52	8.43	8.43	1.46	8.09	8.11	1.59	7.91	7.92	1.60
B/M	0.76	0.52	1.02	0.79	0.54	1.08	0.73	0.51	0.98	0.77	0.52	1.06
B/M Dummy	0.02	0.00	0.13	0.02	0.00	0.13	0.02	0.00	0.14	0.02	0.00	0.14
Momentum	0.08	0.05	0.39	0.06	0.04	0.34	0.07	0.04	0.38	0.08	0.05	0.40
Total Volatility (in %)	9.86	8.43	5.61	9.03	7.86	4.89	9.89	8.41	5.71	10.11	8.61	5.79
Inverse Price Ratio	0.42	0.05	1.56	0.43	0.05	1.51	0.35	0.05	1.35	0.48	0.06	1.70
Leverage	0.24	0.22	0.18	0.25	0.23	0.18	0.24	0.22	0.18	0.24	0.22	0.19
Investment	0.11	0.05	0.31	0.08	0.05	0.25	0.10	0.05	0.28	0.11	0.05	0.31
Gross Profitability	0.28	0.24	0.23	0.27	0.23	0.21	0.29	0.25	0.24	0.29	0.24	0.24
R&D	0.02	0.00	0.05	0.02	0.00	0.04	0.02	0.00	0.06	0.02	0.00	0.06
Tangibility	0.29	0.23	0.24	0.30	0.25	0.24	0.29	0.22	0.24	0.29	0.23	0.24

Table 3.1. Summary statistics

(continued)

	Refinitiv Sample			Sustainalytics Sample			MSCI IVA Sample			*Composite Sample*		
No. Stocks	6,593			4,371			8,291			9,253		
No. Stock-Months	553,388			335,582			578,089			730,984		
Start Date	2004-Jan			2011-Jan			2001-Jan			2001-Jan		
Variable	Mean	Median	S.D.	Mean	Median	S.D.	Mean	Median	S.D.	Mean	Median	S.D.
Panel B: ESG Ratings												
E	31.49	25.96	28.47	54.79	53.04	13.73	48.40	47.60	20.63	47.46	46.33	25.33
S	40.43	37.04	23.22	56.08	54.96	11.43	43.86	45.70	19.76	47.89	46.88	24.29
G	48.34	48.49	22.65	62.87	62.40	10.32	50.91	51.00	22.90	48.36	48.13	23.75
ESG	40.09	37.28	20.33	57.22	56.00	9.87	47.26	46.00	22.85	47.39	46.03	25.66
Panel C: ESG Momentum												
E Momentum	2.53	0.00	9.72	1.05	0.00	5.16	0.59	0.00	10.88	1.21	0.22	11.29
S Momentum	2.25	0.65	9.09	0.78	0.00	5.08	1.09	0.00	12.90	0.97	0.10	13.64
G Momentum	1.25	0.57	13.89	0.39	0.00	4.62	1.05	0.00	18.23	0.70	0.00	16.27
ESG Momentum	2.01	1.14	7.37	0.78	0.00	3.61	0.61	0.00	11.87	1.22	0.65	11.62

The choice of fixed effects is an important consideration given the strong industry and country components of ESG scores (e.g., Gillan, Koch and Starks (2021)). A possible avenue is to use month, country, and industry fixed effects (e.g., Bolton and Kacperczyk (2021)). However, month fixed effects do not absorb industry- and country-level factors likely to be correlated with ESG. By the same token, industry and country fixed effects assume that there is no time variation in country- and industry-specific unobservables that may bias the results. This seems unlikely since various potentially relevant unobservables such as ESG regulations, climate sentiment, investors' tastes for ESG assets, and macroeconomic conditions are likely to vary simultaneously over time and across industries and countries.

We attenuate these concerns by using country-month and industry-month fixed effects. Our panel regressions with industry-month and country-month fixed effects are comparable to Fama and MacBeth (1973) regressions with industry and country dummies.[23] The fixed effects tighten the identification by allowing us to zoom in on cross-sectional predictability within industries and within countries. In other words, we compare firms exposed to similar macroeconomic shocks but with different levels of ESG. This is a relevant concern in our setting because industry- and country-level factors are important determinants of ESG scores likely to be correlated with a variety of macroeconomic unobservables (e.g., Borghesi, Houston and Naranjo (2014), Liang and Renneboog (2017), Gillan, Koch and Starks (2021))).[24] A related advantage is that

[23] An important difference, however, is that panel regressions tend to give more weight to time periods with more observations whereas Fama and MacBeth (1973) regressions equally weigh each period. Since ESG data coverage has improved substantially over time, it is possible that Fama and MacBeth (1973) underweighs those periods in which we can more reliably estimate the relation between ESG and stock returns. This is one of the reasons why we use panel over Fama and MacBeth (1973) regressions.

[24] An alternative to fixed effects is to demean the ESG metrics at the industry or country level while leaving the remaining variables unchanged. This approach can lead to inconsistent estimates, however. Fixed effect methods sidestep this concern (e.g., Gormley and Matsa (2014), Ferson (2019))

by comparing similar firms we partially alleviate the concern that the lack of financial integration across geographically dispersed firms reduces the effectiveness of our control variables (e.g., Fama and French (2017), Chaieb, Langlois and Scaillet (2021)).

Our identification strategy further purges out time-varying cross-industry and cross-country variation in customers and investors' ESG-related risk perceptions, tastes and sentiment (e.g., Engle et al. (2020), Sautner et al. (2020), Pastor, Stambaugh and Taylor (2020)) that may lead to spurious results. In this way our results are more likely to be indicative of the relation between ESG and stock returns going forward as opposed to mainly capturing short-term sample specific shocks. A case in point is the evidence in Chou and Kimbrough (2019) that climate sentiment varies not only over time but also across industries. A shift in sentiment in industries with low levels of pre-existing ESG could thus lead to the conclusion that low ESG stocks outperform when in fact the outperformance comes from high ESG stocks within that industry. Another concrete example is that most of the carbon risk premium documented by Bolton and Kacperczyk (2021) is priced within industries. Ignoring this would have lead to severe under-estimation of the economic significance of the carbon premium.

3.3. Results

In this section we investigate the relation between stock returns and lagged ESG metrics using our full sample covering 9,253 stocks traded in 46 countries between January 2001 and December 2020 (Section 3.3.1). We also examine how this relation varies across different world regions (Section 3.3.2) and sectors of economic activity (Section 3.3.3). In Section 3.3.4 we study negative ESG screens by zooming in on the performance of stocks with low ESG ratings.

3.3.1. Does sustainable investing affect stock returns?

To estimate the extent to which sustainable investing affects future stock returns, we run panel regressions of stock returns on lagged ESG ratings. Table 3.2 shows the results. Columns (1) through (4) in Panel A (Panel B) show the results when using *ESG*, *E*, *S*, and *G* ratings of Refinitiv (Sustainalyics), respectively. Columns (5) through (8) report the results for the specifications that use the ratings of MSCI (Panel A) and *Composite* (Panel B). All specifications use the entire sample and include industry-month and country-month fixed effects. The control variables are listed in the table and their detailed definitions can be found in Appendix Table B.1. t-statistics are reported in parentheses. Standard errors are double clustered at the stock and month levels.

We find little evidence for a robust relation between ESG ratings and future stock returns. The only metric with statistically significant predictive power is MSCI's *G* rating. The effect size is also modest: a one standard deviation increase in *G* is associated with an additional 0.55% in annualized returns. This result is at first sight striking given the extensive literature finding evidence that sustainable investing outperforms on a risk-adjusted basis (e.g., Friede, Busch and Bassen (2015)).[25] What generates this discrepancy? Putting aside the concern that many studies that find evidence of outperformance suffer from methodological issues, there are at least four other reasons that may explain the discrepancy in the findings.[26]

[25]We note that many of the stock characteristics we use as control variables are statistically insignificant. This is in line with the findings of Green, Hand and Zhang (2020) that only two out of the 94 stock characteristics in their study independently predict stock returns in non-microcap stocks since 2003. Gibson, Krueger and Schmidt (2020), who merge stock returns data with ESG ratings data, also find little evidence of stock return predictability based on the stock characteristics they use as control variables.

[26]See the discussion in Matos (2020) and Dimson, Marsh and Staunton (2020) about the methodological problems that plague this literature. For example, according to Dimson, Marsh and Staunton (2020) (i) two-thirds of studies do not lag ESG metrics and thus suffer from look-ahead bias, and (ii) only a minority of those studies are high-quality studies that go through intense peer-review in top journals.

Table 3.2. The returns to sustainable investing: baseline results

This table reports the results from panel regressions of monthly stock returns on lagged ESG ratings using the global sample of stocks. The sample period is from January 2001 to December 2020. The ESG ratings variable is one of the following ratings: environmental (*E*), social (*S*), governance (*G*), or ESG (*ESG*). All regressions include all control variables listed in Appendix Table B.1 as well as country-month and industry-month fixed effects. Following Bolton and Kacperczyk (2021), industries are defined based on six-digit Global Industry Classification Standard Codes (GICS) codes. Panel A (Panel B) shows the results obtained when using Refinitiv and MSCI IVA (Sustainalytics and *Composite*) ratings. t-statistics are shown in parentheses. Standard errors are double clustered at the stock and month levels. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

Panel A: ESG Ratings (Levels) of Refinitiv and MSCI IVA								
	Refinitiv				MSCI IVA			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ESG	0.000				0.001			
	(0.296)				(1.061)			
E		-0.000				0.000		
		(-0.049)				(0.287)		
S			0.001				0.001	
			(0.547)				(0.693)	
G				0.000				0.002**
				(0.465)				(2.202)
Size	0.033	0.027	0.031	0.029	0.050	0.051	0.050	0.049
	(0.939)	(0.748)	(0.947)	(0.807)	(1.510)	(1.526)	(1.495)	(1.463)
B/M	-0.017	-0.019	-0.018	-0.019	-0.004	-0.003	-0.002	-0.004
	(-0.504)	(-0.557)	(-0.525)	(-0.552)	(-0.115)	(-0.098)	(-0.068)	(-0.108)
B/M Dummy	-0.280*	-0.281*	-0.275*	-0.276*	-0.362**	-0.362**	-0.358*	-0.361*
	(-1.723)	(-1.730)	(-1.692)	(-1.694)	(-1.977)	(-1.972)	(-1.954)	(-1.967)
Momentum	0.578**	0.581**	0.580**	0.581**	0.458*	0.457*	0.458*	0.458*
	(2.094)	(2.104)	(2.097)	(2.110)	(1.656)	(1.655)	(1.657)	(1.659)

Table 3.2. The returns to sustainable investing: baseline results

(continued)

Panel A: ESG Ratings (Levels) of Refinitiv and MSCI IVA								
	Refinitiv				MSCI IVA			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total Volatility	0.007	0.007	0.007	0.007	-0.000	-0.000	-0.000	-0.000
	(0.295)	(0.291)	(0.286)	(0.285)	(-0.016)	(-0.014)	(-0.008)	(-0.013)
Inverse Price Ratio	0.004	0.003	0.004	0.004	-0.027	-0.027	-0.026	-0.027
	(0.145)	(0.127)	(0.152)	(0.142)	(-1.054)	(-1.048)	(-1.048)	(-1.057)
Leverage	-0.202	-0.208	-0.214	-0.216	-0.307	-0.306	-0.305	-0.306
	(-1.060)	(-1.078)	(-1.099)	(-1.128)	(-1.426)	(-1.420)	(-1.413)	(-1.420)
Investment	0.001	0.005	0.001	0.002	-0.012	-0.012	-0.011	-0.010
	(0.009)	(0.060)	(0.017)	(0.022)	(-0.127)	(-0.131)	(-0.120)	(-0.113)
Gross Profitability	0.462***	0.461***	0.467***	0.466***	0.370***	0.368***	0.362***	0.367***
	(3.937)	(3.919)	(3.975)	(3.957)	(2.961)	(2.952)	(2.905)	(2.937)
R&D	2.112**	2.088**	2.131**	2.124**	1.232	1.231	1.231	1.214
	(2.527)	(2.493)	(2.566)	(2.541)	(1.495)	(1.493)	(1.493)	(1.477)
Tangibility	-0.146	-0.146	-0.150	-0.150	-0.085	-0.087	-0.089	-0.085
	(-0.987)	(-0.988)	(-1.015)	(-1.015)	(-0.537)	(-0.542)	(-0.557)	(-0.531)
Industry-by-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-by-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	552,097	552,073	552,962	552,962	577,047	577,047	577,047	577,047
R-squared	0.389	0.389	0.389	0.389	0.376	0.376	0.376	0.376

Table 3.2. The returns to sustainable investing: baseline results

(continued)

Panel B: ESG Ratings (Levels) of Sustainalyics and *Composite*								
	Sustainalytics				*Composite*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ESG	0.002				0.000			
	(0.917)				(0.406)			
E		0.001				0.000		
		(0.733)				(0.486)		
S			0.001				0.000	
			(0.302)				(0.525)	
G				0.003				0.001
				(1.217)				(0.835)
Size	0.048	0.052	0.050	0.047	0.034	0.035	0.035	0.035
	(1.270)	(1.402)	(1.328)	(1.258)	(1.098)	(1.134)	(1.165)	(1.142)
B/M	0.009	0.010	0.009	0.009	-0.009	-0.008	-0.008	-0.008
	(0.246)	(0.291)	(0.262)	(0.248)	(-0.299)	(-0.283)	(-0.280)	(-0.285)
B/M Dummy	-0.390**	-0.389**	-0.389**	-0.390**	-0.421***	-0.420***	-0.417***	-0.418***
	(-2.072)	(-2.069)	(-2.065)	(-2.071)	(-2.714)	(-2.712)	(-2.691)	(-2.695)
Momentum	0.666**	0.664**	0.666**	0.666**	0.597**	0.596**	0.597**	0.597**
	(2.137)	(2.126)	(2.129)	(2.136)	(2.479)	(2.481)	(2.485)	(2.486)

Table 3.2. The returns to sustainable investing: baseline results

(continued)

Panel B: ESG Ratings (Levels) of Sustainalyics and *Composite*								
	Sustainalytics				*Composite*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total Volatility	-0.015	-0.015	-0.015	-0.015	-0.009	-0.009	-0.009	-0.009
	(-0.584)	(-0.580)	(-0.579)	(-0.583)	(-0.420)	(-0.420)	(-0.422)	(-0.424)
Inverse Price Ratio	-0.016	-0.016	-0.015	-0.016	-0.003	-0.003	-0.002	-0.003
	(-0.517)	(-0.502)	(-0.496)	(-0.514)	(-0.136)	(-0.126)	(-0.112)	(-0.122)
Leverage	-0.239	-0.236	-0.238	-0.238	-0.222	-0.222	-0.224	-0.225
	(-1.205)	(-1.181)	(-1.192)	(-1.194)	(-1.180)	(-1.175)	(-1.186)	(-1.193)
Investment	-0.077	-0.082	-0.078	-0.077	0.048	0.047	0.048	0.047
	(-0.828)	(-0.866)	(-0.820)	(-0.817)	(0.621)	(0.597)	(0.609)	(0.599)
Gross Profitability	0.350**	0.351**	0.350**	0.350**	0.478***	0.477***	0.476***	0.478***
	(2.144)	(2.145)	(2.147)	(2.135)	(4.338)	(4.323)	(4.327)	(4.331)
R&D	2.080**	2.105**	2.087**	2.082**	1.520**	1.520**	1.524**	1.520**
	(1.998)	(2.026)	(2.008)	(2.000)	(2.036)	(2.038)	(2.043)	(2.036)
Tangibility	-0.222	-0.222	-0.226	-0.224	-0.055	-0.055	-0.057	-0.056
	(-1.215)	(-1.220)	(-1.244)	(-1.228)	(-0.388)	(-0.390)	(-0.407)	(-0.399)
Industry-by-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-by-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	335,486	335,486	335,486	335,486	729,682	729,682	730,011	730,011
R-squared	0.378	0.378	0.378	0.378	0.360	0.360	0.360	0.360

First, some studies do not use ratings as a measure of sustainability (e.g., Edmans (2011)) or rely on exogenous variation in ESG that cannot be used by investors. For example, Flammer (2015a) finds that the random approval of shareholder-sponsored ESG proposals leads to positive abnormal returns on the vote day and a posterior increase in ESG ratings. This suggests stock markets incorporate the information in ratings before they are released.

Second, some of the studies that find that sustainability pays off focus on the period until the early 2000s (e.g., Derwall et al. (2005), Kempf and Osthoff (2007)). Anomalies, however, tend to disappear as markets learn over time (e.g., McLean and Pontiff (2016)). Consistent with this idea, Borgers et al. (2013) and Halbritter and Dorfleitner (2015) find that high ESG portfolios outperform low ESG portfolios until the early 2000s but not after.

Third, many studies use only data for one country over a short period of time, making it possible that they capture temporary outperformance due to unexpected shifts in consumer and investor tastes for sustainability (e.g., Pastor, Stambaugh and Taylor (2020)).

Fourth, part of the evidence for the outperformance of sustainable investing comes from meta-analyses (e.g., Friede, Busch and Bassen (2015)) which pool together studies that look at both accounting and market measures of performance. This is problematic because even if ESG ratings predict future profitability, they might not predict stock returns provided markets are efficient enough (e.g., Pedersen, Fitzgibbons and Pomorski (2020)).

Notwithstanding the congruence between our findings and those of the extant literature, an important concern is that our specifications are overly conservative due to the inclusion of high-dimensional fixed effects. Furthermore, by including fixed effects we effectively build a long-short portfolio that weighs each stock based on how its ESG rating compares to that of its country and industry peers.[27] As with many asset pricing tests, this leads to a joint hypothesis problem. We cannot separate the hypothesis of interest from the assumption that the underlying portfolio formation restrictions are adequate. To attenuate this concern we replicate our baseline results using only one of the following: month fixed effects, industry-month fixed effects, or country-month fixed effects.

To simplify the presentation we show the results in Figure 3.4. Each plot shows the regression coefficient estimates obtained when using a given ESG metric and different combinations of fixed effects. We use the full set of controls in all regressions. We depict 90% and 95% confidence intervals as whiskers. Three findings emerge.

First, ESG metrics fail to explain variation in stock returns across the board, including in specifications that use month fixed effects instead of industry-month and country-month fixed effects. Hence, the statistically insignificant relation between ESG and future stock returns is not driven by an overly conservative use of high-dimensional fixed effects.

Second, the predictive power of ESG tends to be stronger, albeit imprecisely estimated, within countries than across countries. This follows from the fact that the effect sizes are larger when we replace month fixed effects by country-month fixed effects. This suggests the performance of ESG strategies may depend on geography, a possibility we explore later.

[27] To see this note that the coefficient of interest could be obtained as follows. First, regress stock returns on all control variables and industry-month and country-month fixed effects and save the residuals (denote them $\hat{R}$). Second, regress the ESG metric on the same control variables and fixed effects and save the residuals (denote them $E\hat{S}G$). Third, regress $\hat{R}$ on the (transformed) ESG metric $E\hat{S}G$. This follows from the Frisch-Waugh-Lovell theorem (Frisch and Waugh, 1933). The coefficient of interest has zero mean and is given by $\hat{\beta} = \sum_{it} w_{it}\hat{R_{it}}$, where $w_{it} = \frac{E\hat{SG_{it}}}{\sum_{m,t} ES\hat{G}_{m,t}{}^2}$. Since the mean of $E\hat{S}G$ is zero at the industry-month and country-month level, it must follow that whenever $\hat{w_{it}} < 0$ (shorting) it is also the case that $\hat{w_{it}}$ has a (transformed) ESG score below that of its industry and country peers at time *t*. The parallel logic applies when $\hat{w_{it}} > 0$ (going long). For a related discussion in a more general context refer to Campbell (2017) and footnote 8 of Hou, Xue and Zhang (2020).

Figure 3.4. ESG ratings and stock returns: global sample

This figure summarizes the results from running panel regressions of monthly stock returns on lagged ESG ratings using the global sample of stocks. All regressions include all control variables listed in Appendix Table B.1 as well as one of the following fixed effects: (i) month fixed effects, (ii) country-month fixed effects, (iii) industry-month fixed effects, or (iv) country-month and industry-month fixed effects. Following Bolton and Kacperczyk (2021), industries are defined based on six-digit Global Industry Classification Standard Codes (GICS) codes. Each of the four plots in the figure shows the results of running regressions using one of the following four types of ESG ratings: environmental (*E*), social (*S*), governance (*G*), and ESG (*ESG*). We use ratings from four raters: Refinitiv, MSCI IVA, Sustainalytics, and *Composite*. *Composite* combines the available ratings of the other three raters by averaging their ratings. We convert the ratings of each of the other three raters at each point in time to percentile ranks before averaging. Each bar represents the regression coefficient on the ESG rating variable used in a given regression. Confidence intervals are depicted as whiskers around the point estimates. The longer (shorter) whiskers represent 95% (90%) confidence intervals. Standard errors are double clustered at the stock and month levels. The sample period is from January 2001 to December 2020.

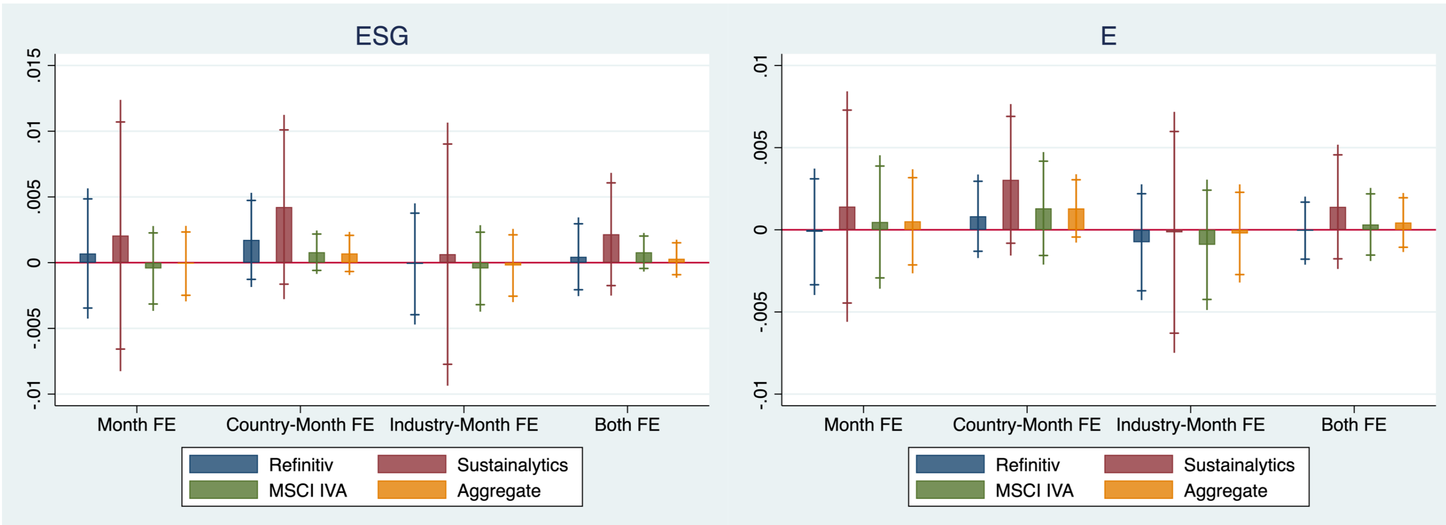


Figure 3.4. ESG ratings and stock returns: global sample

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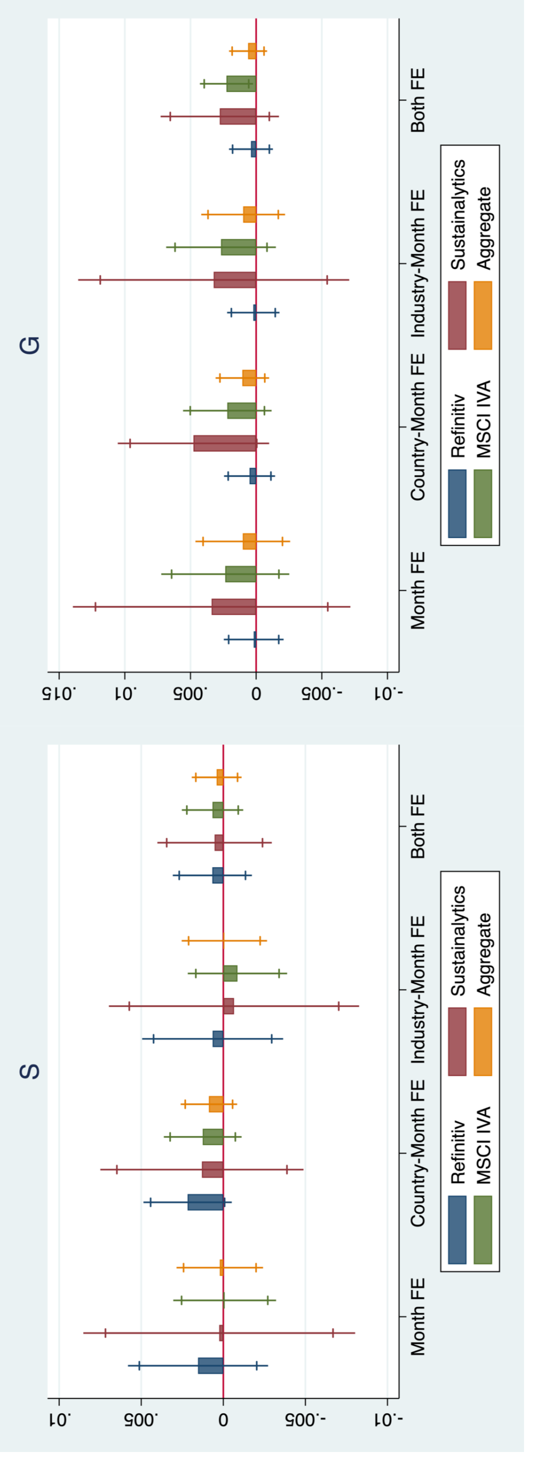

Third, the effect sizes shrink the most in the specifications that use industry-month fixed effects. This suggests that investors' pro-social preferences have modest asset pricing implications in our sample. To see why, note that pro-social preferences can in theory lead to lower expected returns for sustainable stocks (e.g., Pastor, Stambaugh and Taylor (2020)). The industry-month fixed effects, however, may make it hard to capture this effect because they do not distinguish unsustainable stocks that pro-social investors dislike from sustainable stocks that investors like but which happen to have lower ESG ratings than their industry peers do. It could thus be the case that removing industry-month fixed effects pushed the coefficients towards negative territory. The exact opposite happens.

Next, we present the results obtained when using ESG momentum variables instead of ESG ratings in Figure 3.5. There are a few instances in which ESG momentum has significant predictive power. When this is the case, the sign is always positive. These results are not robust across specifications, however. For example, in the specification with month fixed effects *ESG* momentum is a significant predictor at the 5% level if we employ Sustainalytics ratings but not if we use any of the other raters. Moreover, once we include country-month fixed effects the estimates become more imprecise and statistical significance is only obtained at the 10% level. The effect size is modest, with a one standard deviation increase in the rating being associated with an additional 1.18% in annualized returns. If we use industry-month fixed effects instead, the economic significance almost completely vanishes - that is, the coefficients hover around zero. A similar pattern is observed for *E*, *S* and *G* momentum.

It is also interesting that *Composite* does not have an edge over individual raters in terms of predictive power. This is the case whether we look at ESG momentum or at ESG ratings. In theory, however, pooling

Figure 3.5. ESG momentum and stock returns: global sample

This figure summarizes the results from running panel regressions of monthly stock returns on lagged ESG momentum using the global sample of stocks. ESG momentum is defined as the year-on-year change in ESG ratings. All regressions include all control variables listed in Appendix Table B.1 as well as one of the following fixed effects: (i) month fixed effects, (ii) country-month fixed effects, (iii) industry-month fixed effects, or (iv) country-month and industry-month fixed effects. Following Bolton and Kacperczyk (2021), industries are defined based on six-digit Global Industry Classification Standard Codes (GICS) codes. Each of the four plots in the figure shows the results of running regressions using one of the following four types of ESG momentum: environmental (*E*), social (*S*), governance (*G*), and ESG (*ESG*). We use ratings from four raters: Refinitiv, MSCI IVA, Sustainalytics, and *Composite*. *Composite* combines the available ratings of the other three raters by averaging their ratings. We convert the ratings of each of the other three raters at each point in time to percentile ranks before averaging. Each bar represents the regression coefficient on the ESG momentum variable used in a given regression. Confidence intervals are depicted as whiskers around the point estimates. The longer (shorter) whiskers represent 95% (90%) confidence intervals. Standard errors are double clustered at the stock and month levels. The sample period is from January 2001 to December 2020.

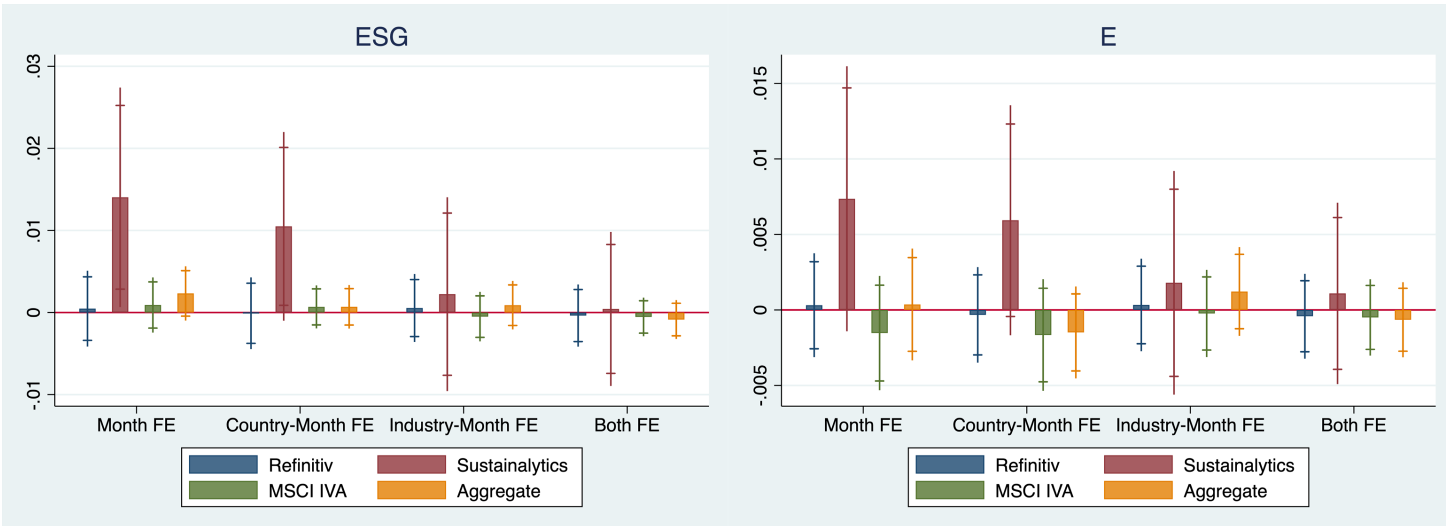

Figure 3.5. ESG momentum and stock returns: global sample

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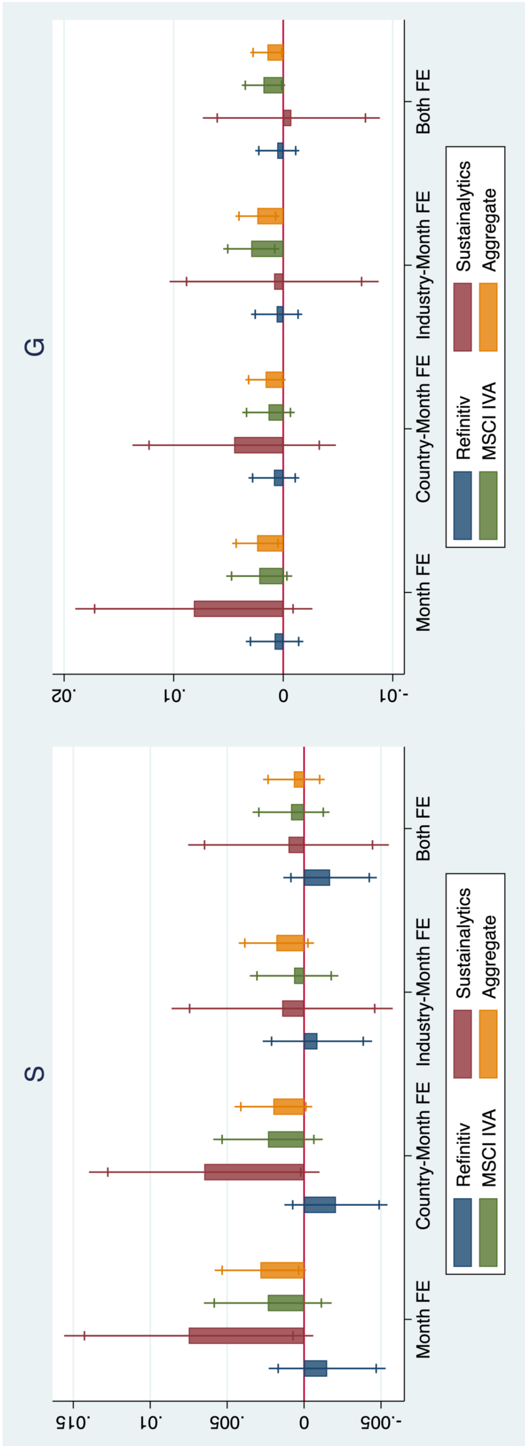

information across raters may enhance the predictive power of ESG metrics by canceling out noise (e.g., Serafeim and Yoon (2021)). In addition, since different investors rely on different raters, it is possible that the total demand for a stock by pro-social investors is higher (lower) if many raters give a high (low) rating to that stock. This is likely to be the case when *Composite* is high (low). Since pro-social preferences are only priced in if pro-social investors' demand is large enough (Fama and French (2007)), it follows that *Composite* can in theory be more informative about expected returns than individual raters are.

Our results do not provide evidence in favor of these views, but they are not necessarily inconsistent with them either. Instead, the results may simply reflect the existence of counter-veiling forces. For example, even if *Composite* is more informative about future cash flows than individual raters are, it might be that *Composite* captures information that is quickly impounded in prices. This may be the case because the information on which raters agree is likely to be the information that is less subjective and easier to obtain. Another explanation is related to the evidence in Gibson, Krueger and Schmidt (2020) that cross-rater disagreeement in ESG predicts risk premia. It is possible that *Composite* partly cancels out the disagreement component of ESG metrics that has predictive power for stock returns.

Overall, our results thus far suggest that sustainable investing based on ESG is not related to future stocks returns in the cross-section. This finding is consistent with existing asset pricing models and admits a number of interpretations. For example, one explanation for our findings is that (i) ESG metrics have value-relevant information but markets are efficient enough to eliminate evidence of predictability, and (ii) the demand of pro-social investors willing to accept lower expected returns for holding stocks represents a small enough share of total investor demand (e.g., Pedersen, Fitzgibbons and Pomorski (2020)).

It is also possible that ESG ratings do not systematically have value-relevant information and that investors recognize this (e.g., Pedersen, Fitzgibbons and Pomorski (2020)). It is reasonable to expect that this is the case for at least some ESG metrics since (i) there are hundreds of ESG ratings products that have low correlations among themselves, making it unlikely that all metrics have value-relevant information for a large number of firms (e.g., Berg, Koelbel and Rigobon (2020)), and (ii) ESG ratings may be contaminated by greenwashing (e.g. Yang (2019)), may be biased towards larger firms with better disclosure (e.g., de Silanes, McCahery and Pudschedl (2019)), may be backward-looking, and may rely too much on box-ticking methods (e.g., Edmans (2020), Matos (2020)).

3.3.2. Does the performance of sustainable investing vary across geographic regions?

In this section we explore whether or not the ability of ESG to predict cross-sectional variation in future stock returns is mediated by geography. In theory, there are several reasons why our results may differ geographically, such as differences in market efficiency across countries (e.g., Pedersen, Fitzgibbons and Pomorski (2020)), differential ability of ESG metrics to predict firm fundamentals due to cross-country differences in labor regulations (e.g., Edmans, Li and Zhang (2014)), and permanent differences in social norms across regions. The latter could, for instance, lead to lower returns to sustainable investing in pro-social Europe compared to North America (e.g., Pastor, Stambaugh and Taylor (2020)).

We proceed by repeating the baseline analysis for each of the following regions: Asia-Pacific, Japan, Europe, North America, and Emerging Countries. The region Emerging Countries includes all the emerging countries in Africa, Asia, Europe, Latin American, and Middle-East listed in Appendix Table B.2. We do not use Sustainalytics ratings in the pre-2013 specifications because there are little data. All specifications include all controls used before as well as industry-month and country-month fixed effects, with the exception of the specifications pertaining to Japan which exclude country-month fixed effects.

We also split the sample in two: one covering the period before 2013 and another covering the period from 2013 onwards. We do this to account for the possibility that the massive inflow of money into sustainable products since 2013 has led to an abnormally good performance of high ESG stocks in recent years in some regions. Such a finding would be consistent with the model of Pastor, Stambaugh and Taylor (2020) which predicts that sustainable stocks can outperform non-sustainable stocks temporarily due to shifts in customer and investor tastes for sustainable stocks. The 2013-2020 period is also a notorious post-crisis rebound period in which the S&P500, MSCI ACWI Index and Shiller's P/E ratio increased by 148%, 115%, and 59% after adjusting for dividends and stock-splits, respectively. The increase in wealth during this period might also have caused a demand surge for ESG as a luxury good (e.g., Bansal, Wu and Yaron (2018)). The full sample results are similar to the results using the post-2013 subsample. We report full sample results in Appendix Figures B.1 and B.2 for ESG ratings and ESG momentum, respectively.

Figure 3.6 shows the results from specifications using ESG ratings in levels. The left-hand side and the right-hand side plots show the results for the pre-2013 and post-2013 subsamples, respectively. Before 2013 there is little evidence of a robust relation between ESG ratings and future stocks returns and the effect sizes tend to be small. The regression coefficient estimates for MSCI's E and S ratings are significantly positive at the 10% level in Japan and Europe, respectively. All other coefficients are insignificant.

The finding that ESG ratings are very noisy signals for future stock returns also seems to carry over to the post-2013 period. There are three interesting nuances, however. First, G often predicts higher returns in North America in recent years, which is consistent with the findings of Pedersen, Fitzgibbons and Pomorski (2020) that investors in the US underestimate the extent to which G has information about future profitability (they measure G as accruals over assets). The fact that G seems to matter in North America and not in other regions may be due to optimal governance arrangements varying around the world in ways not accounted for by raters. For example, Black et al. (2019) find that "improvements" to board structure increase firm value in Brazil and Korea but not in India and Turkey. Given that current knowledge in academia about geographical heterogeneity in optimal governance is still limited, it is unlikely that raters successfully adjust ratings for that.

This finding is, however, unexpected given the survey evidence that North American investors pay substantial attention to G (e.g., Eccles, Serafeim and Krzus (2011), Aggrawal et al. (2011)). It is possible, however, that there was a shift in attention from G to E and S in recent years that generated underreaction to governance information. Another explanation is that our result is a chance result due to multiple hypotheses testing. Either way, to our knowledge there is no theoretical reason to expect that this effect should last going forward.

Second, higher E tends to predict lower future returns in Europe post-2013 but not pre-2013. Even though the effect is only significant at the 10% level based on MSCI ratings, it may reflect the rising importance given to environmental issues in Europe. This may lead investors to accept lower returns to

Figure 3.6. ESG ratings and stock returns: world regions

This figure summarizes the results from running panel regressions of monthly stock returns on the lagged ESG ratings of stocks traded in one of the following regions: Asia-Pacific, Japan, Europe, North America, and Emerging Countries. All regressions include all control variables listed in Appendix Table B.1 as well as country-month and industry-month fixed effects. Following Bolton and Kacperczyk (2021), industries are defined based on six-digit Global Industry Classification Standard Codes (GICS) codes. The plots on the left (right) show the results of running these regressions over the period January 2001 to December 2012 (January 2013 to December 2020). Each plot in the figure shows the results of running regressions using one of the following four types of ESG ratings: environmental (E), social (S), governance (G), and ESG (ESG). We use ratings from four raters: Refinitiv, MSCI IVA, Sustainalytics, and *Composite*. *Composite* combines the available ratings of the other three raters by averaging their ratings. We convert the ratings of each of the other three raters at each point in time to percentile ranks before averaging. Each bar represents the regression coefficient on the ESG rating variable used in a given regression. Confidence intervals are depicted as whiskers around the point estimates. The longer (shorter) whiskers represent 95% (90%) confidence intervals. Standard errors are double clustered at the stock and month levels.

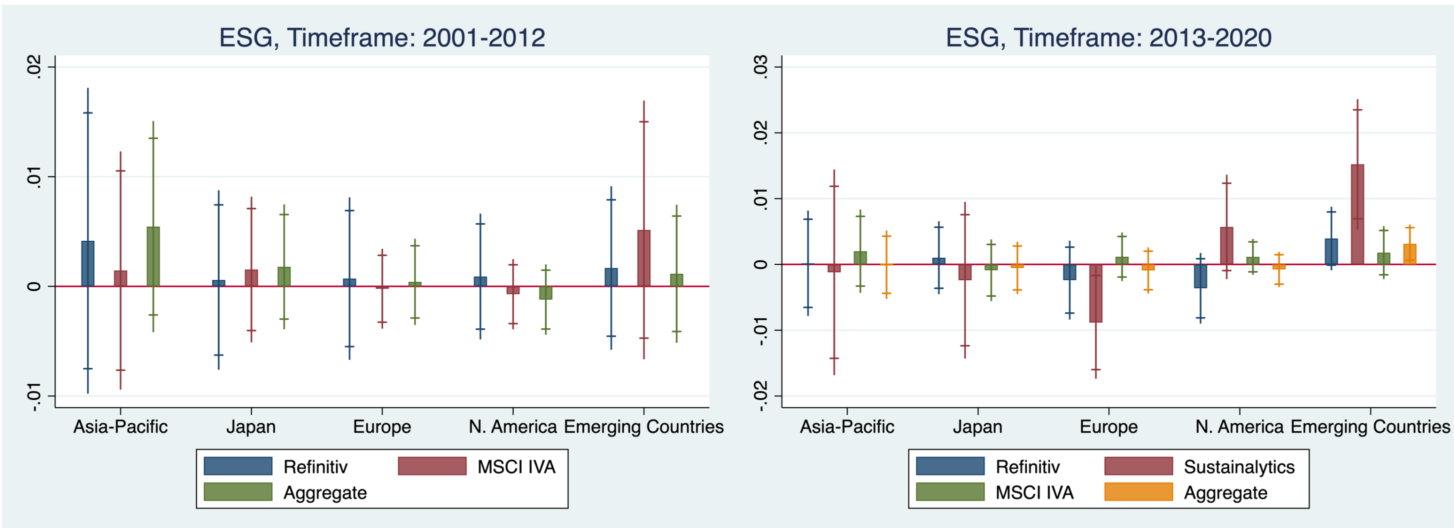

Figure 3.6. ESG ratings and stock returns: world regions

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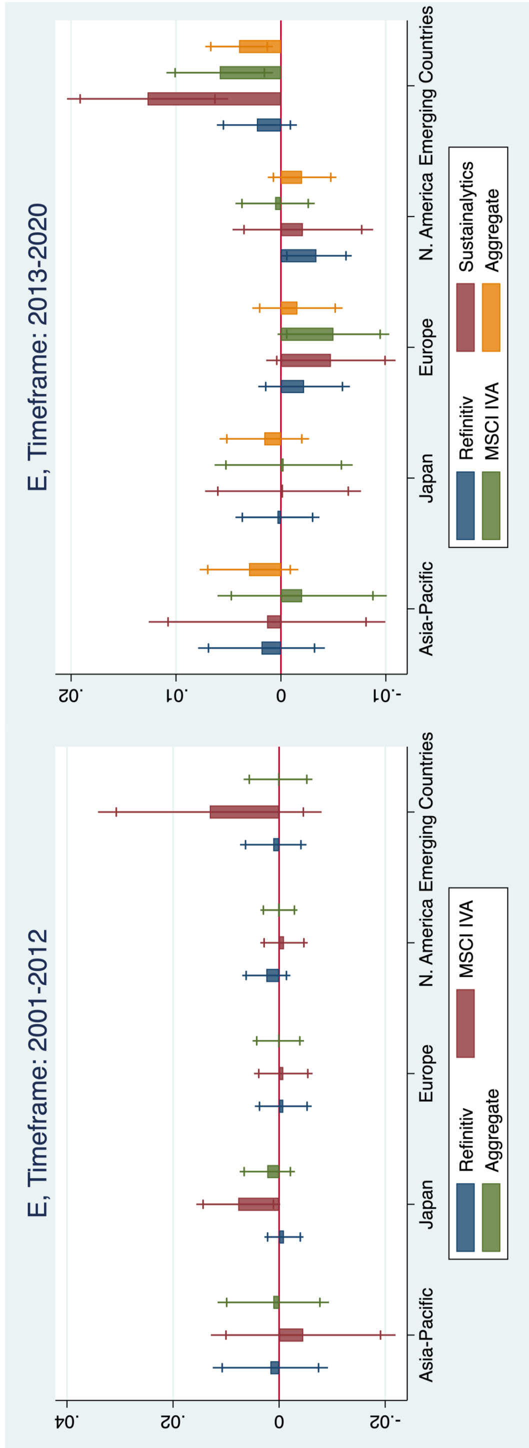

Figure 3.6. ESG ratings and stock returns: world regions

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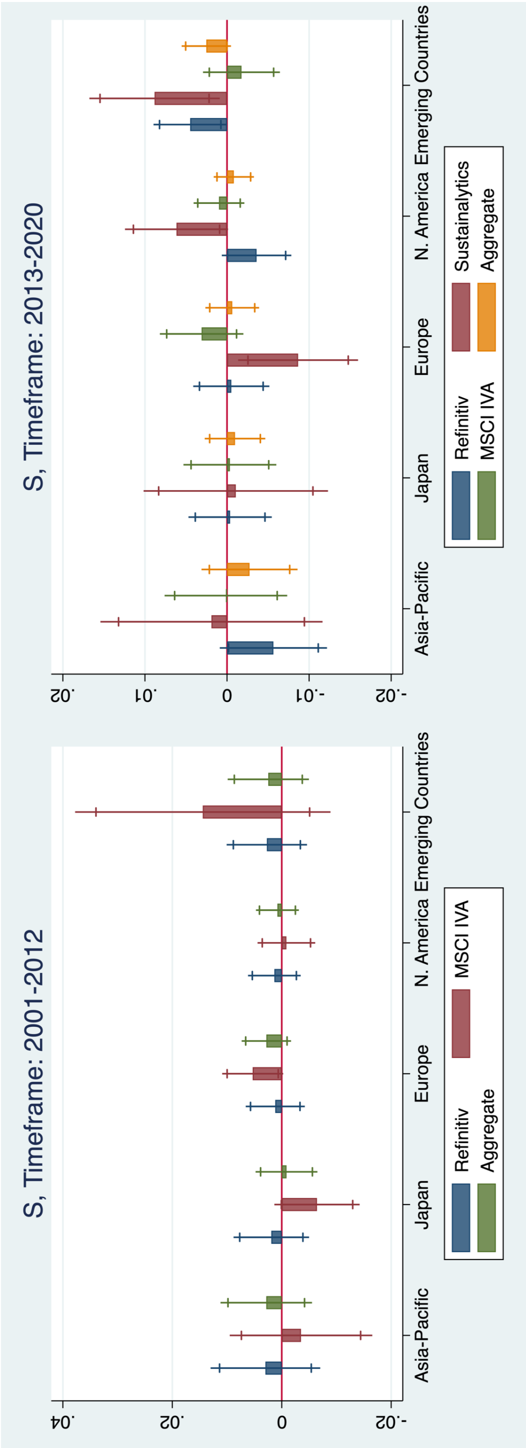

Figure 3.6. ESG ratings and stock returns: world regions

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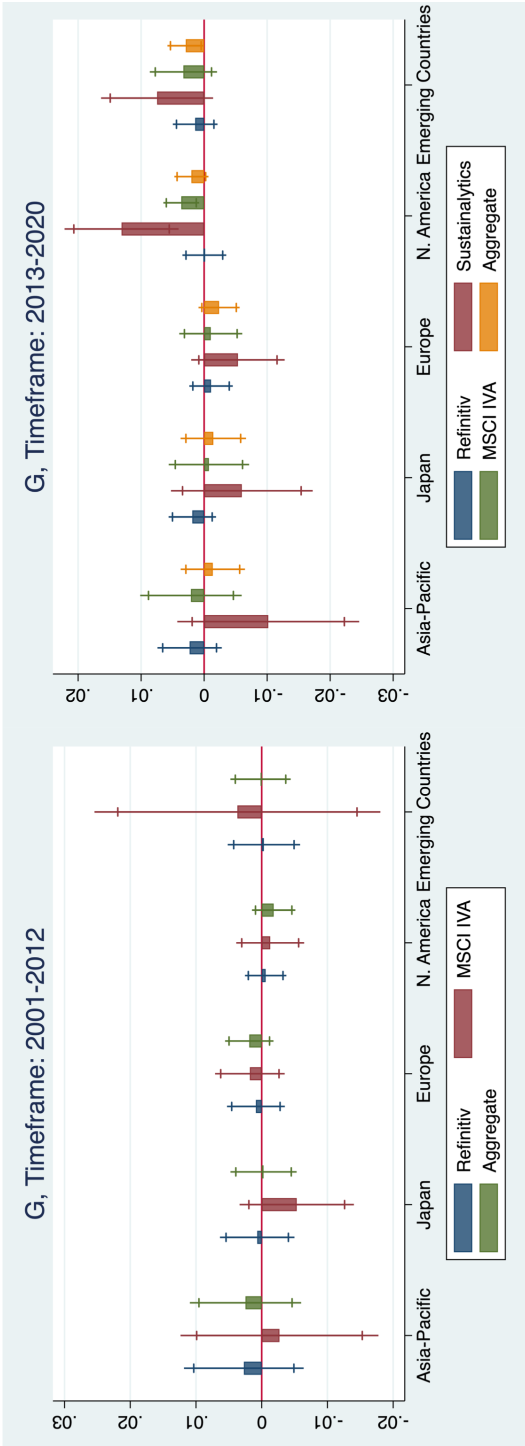

hold green stocks (e.g., Pastor, Stambaugh and Taylor (2020)). If so, it is possible, albeit uncertain, that this result will strengthen going forward.[28]

Third, more sustainable stocks (as measured by E and S) seem to perform better in emerging countries in most specifications. The effect sizes are modest, but not negligible. For example, a one standard deviation increase in the E rating of Sustainalytics (MSCI) is associated with a 2.14% (1.48%) gain in annualized returns. This result is consistent with the finding in the meta-analysis of Friede, Busch and Bassen (2015) that 65.4% of the sample studies find a positive relation between ESG and financial performance in emerging markets. In developed markets only 38% of the studies find such an effect.

What drives this effect? Since the effects are concentrated in the post-2013 period our results are consistent with the story that an unexpected rise in ESG concerns among investors has led to temporarily high realized returns in recent years (e.g., Pastor, Stambaugh and Taylor (2020)). Emerging markets could be particularly exposed to these demand surges because investors may regard sustainable stocks as an important hedge against adverse conditions in emerging countries. For example, some of these countries' geo-location exposes them to higher climate risks (Bathiany et al. (2018)) and their governments may be poorly positioned to respond to ESG concerns due to weaker institutions and tighter budget constraints.

We note, however, that it is also possible that the results are weaker in the pre-2013 period because ESG data coverage in emerging markets is better in recent years. Hence, our results may be partially driven by other more general factors. For example, it may be the case that predictability is stronger for emerging countries because their stock markets are less efficient than those of

[28]We also find that both MSCI's and Sustainalytics' E ratings are positively related to returns in Europe at the 10% significance level when using the full sample period. Note that for Sustainalytics the full sample period corresponds to the post-2011 period. The results are reported in Appendix Table B.1.

developed countries. Even if the stock markets of emerging countries are not less efficient in general, they may be less efficient than their developed counterparts when it comes to processing ESG information. This would be consistent with our finding in Figure 3.3 that cross-rater disagreement tends to be higher in emerging countries and with the finding of Serafeim and Yoon (2021) that the information in ESG ratings is less likely to be incorporated in prices when disagreement is high. In addition, Avramov et al. (2021) provide evidence that investors are less willing to pay a premium to hold high ESG stocks with high disagreement. This effectively mutes the non-pecuniary preferences channel of Pastor, Stambaugh and Taylor (2020) that makes high ESG stocks have lower stock returns.

It is also possible that ESG ratings are more informative about future firm fundamentals in emerging countries than in developed countries. For example, it might be the case that high pressure from regulators, investors, and customers for firms in developed countries to be sustainable and disclose ESG information leads firms in these countries to invest in non-material ESG (e.g., Christensen, Hail and Leuz (2018)).

Next, we turn to the ESG momentum results shown in Appendix Figure B.3. There are similarities with the results obtained using ESG ratings in levels, but overall there is less evidence for a robust relation between ESG momentum and stock returns. For example, when using MSCI ratings we find that North American firms with higher *G* momentum tend to have higher returns in the post-2013 period. This result is, however, not robust to using other raters. We also find that higher *E* momentum predicts lower returns in Europe in the post-2013 period when we use *Composite* ratings but not when we use other raters.

In sum, our results show that stocks with higher ESG ratings do not have higher stock returns in most world regions. This is true over the full sample but also if we split the sample around 2013 when there was an explosion in money inflows into ESG assets. This suggests that valuation of stocks with high ESG ratings is not yet excessive, broadly in line with the opinions of investors in the survey of Krueger, Sautner and Starks (2020). The main exception is that higher E and S ratings are associated with higher future returns in emerging countries in the post-2013 period. This is likely explained by a combination of weaker market efficiency in these countries, a temporary demand surge in investment flows due to rising ESG concerns in recent years and, perhaps, ESG ratings being more informative about future firm fundamentals in emerging countries.

3.3.3. Does the performance of sustainable investing vary across sectors?

We now turn to the question of whether or not investing in stocks that are ESG leaders in their sector of economic activity can enhance returns. Since our previous tests explore within-industry variation in ESG scores, our results thus far suggest that these so-called best-in-class strategies are unlikely to succeed in delivering positive abnormal returns. However, in the same way our results in Section 3.3.2 suggest that an ESG strategy tilted towards emerging markets might have worked in the past, it is also possible that there are sector tilts that have worked particularly well or particularly poorly in the past.

To investigate whether or not this is the case we explore the relation between ESG metrics and future stock returns within each sector. We consider the following ten GICS sectors: energy, materials, industrials, consumer discretionary, consumer staples, healthcare, financials, information technology, communication services, and utilities. We do not study the real estate sector because this sector only accounts for 0.3% of the sample size after removing real estate investment trusts and applying the remaining filters described in Section 3.1.1. We use the same specifications as before.

We find no relation between future stock returns and ESG ratings for the following sectors: consumer staples, healthcare, information technology, and utilities. For communication services, financials, and industrials there is only one significant coefficient in each case. In two of those three cases the coefficient is negative. We present these results in Appendix Figure B.4 to conserve space. We also find that ESG momentum is not robustly associated with future returns in any sector. We show these results in Appendix Figure B.5.

Figure 3.7 shows the results for the consumer discretionary, materials, and energy sectors, where we find somewhat stronger results based on ESG ratings. Nonetheless, given the amount of hypotheses tested, we stress that there is a chance that at least some of these results are false positives.

For the consumer discretionary sector we find a positive and significant relation between future stocks returns and *S* and *ESG* when using the MSCI ratings, but not when using other raters. This is reminiscent of the finding by Servaes and Tamayo (2013) that firms with higher customer awareness can create value by, for example, building customer loyalty through ESG investments. The consumer discretionary sector is the kind of sector providing non-essential goods for which building customer loyalty through ESG investments may work as a way to reduce the price elasticity of demand. We also find that higher *E* and *ESG* is associated with higher future returns in the materials sector. This finding could be partly due to the *E* being material in the materials sector (e.g., Khan, Serafeim and Yoon (2016)). This effect, however, only holds based on Refinitiv ratings.

Figure 3.7. ESG ratings and stock returns: sectors of economic activity

This figure summarizes the results from running panel regressions of monthly stock returns on the lagged ESG ratings of stocks traded in one of the following sectors of economic activity: consumer discretionary, materials, and energy. These sectors are defined based on Global Industry Classification Standard Codes (GICS) codes. All regressions include all control variables listed in Appendix Table B.1 as well as country-month and industry-month fixed effects. Following Bolton and Kacperczyk (2021), industries are defined based on six-digit GICS codes. We consider four types of ESG ratings: environmental (*E*), social (*S*), governance (*G*), and ESG (*ESG*). We use ratings from four raters: Refinitiv, MSCI IVA, Sustainalytics, and *Composite*. *Composite* combines the available ratings of the other three raters by averaging their ratings. We convert the ratings of each of the other three raters at each point in time to percentile ranks before averaging. Each plot in the figure shows the results of running regressions for a given sector. Each bar represents the regression coefficient on the ESG rating variable used in a given regression. Confidence intervals are depicted as whiskers around the point estimates. The longer (shorter) whiskers represent 95% (90%) confidence intervals. Standard errors are double clustered at the stock and month levels. The sample period is from January 2001 to December 2020.

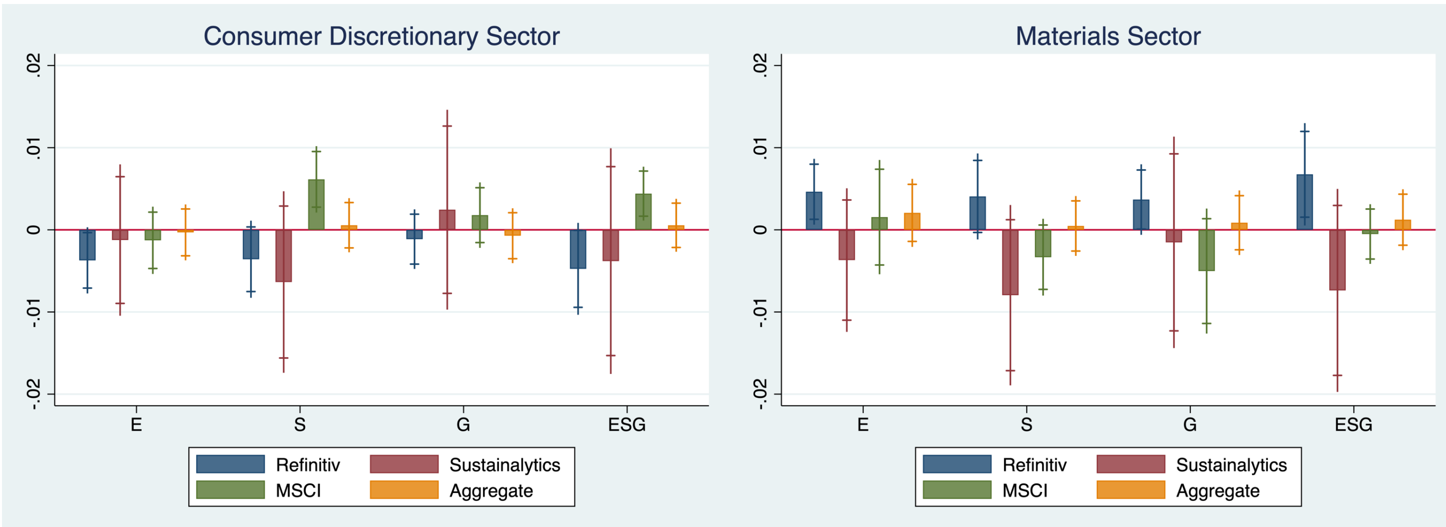

Figure 3.7. ESG ratings and stock returns: sectors of economic activity

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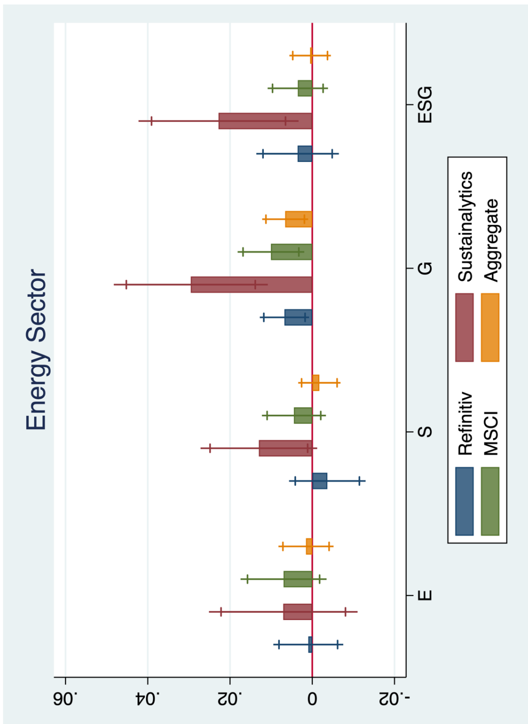

The only robust effect we find occurs in the energy sector. *G* is positively related to future stock returns in all specifications at the 5% significance level or better. A one standard deviation increase in *G* is associated with additional annualized returns between 1.90% and 3.71%. This is somewhat remarkable given that cross-rater correlations are quite low for *G*. One possible explanation is that investors fail to price in the value-relevant information in *G* as suggested by Pedersen, Fitzgibbons and Pomorski (2020). Indeed, there is some suggestive evidence for the idea of investor inattention in the energy sector. For example, Cohen, Gurun and Nguyen (2020) find that ESG investors shun stocks in the energy sector despite some of these firms being major sources of high quality green innovation. Under the assumption that this green innovation is value-enhancing, it may be the case that these under-appreciated energy firms are also the firms with the highest *G* scores within the sector.

More generally, *G* may have important information about future profitability in the energy sector because of its oligopolistic structure. As shown by Giroud and Mueller (2010), good internal corporate governance is more likely to be value increasing in non-competitive industries.[29] *G* also has more potential to boost firm value in low transparency sectors in which agency issues can be severe if left unchecked. Consistent with this idea, Christensen, Hail and Leuz (2018) find that the oil and gas exploration and production industry has the worst ESG disclosure quality among all industries.

Overall, with the exception that *G* ratings may have some information about future returns in the energy sector, the relation between ESG ratings and future stock returns within each sector is mostly flat. On the bright side, this suggests

[29]The concentration in the energy sector is so pronounced that the six largest oil and gas firms in the world are nicknamed "Big Oil" or "supermajors". The energy sector is also notable for its waves of mergers and acquisitions (M&A) in the last decades. To illustrate, according to the data in the website of the Institute for Mergers, Acquisitions and Alliances (IMAA), the energy sector accounts for 14.8% of the total worldwide value of M&A between 2001 and 2019 - the highest share among all the sectors. The underlying data is sourced from Thomson Financials.

that sustainable investing does not require excluding particular sectors to avoid lowering returns, which could lead to under-diversification. On the dark side, however, the results provide little reason to be optimistic about the ability of sustainable investing to deliver positive abnormal returns.

3.3.4. Do negative screens hurt performance?

Next, we study the performance of negative ESG screens which are used by investors to exclude socially irresponsible stocks from their portfolios. These screens typically consist of excluding sin stocks (e.g., alcohol, tobacco, gaming) and stocks with low ESG ratings (e.g., Amel-Zadeh and Serafeim (2017)). These are also the most frequent types of ESG screening (e.g., Amel-Zadeh and Serafeim (2017)). Moreover, negative screens are often used by norm-constrained investors, such as pension funds, which play an important role in society (e.g., Hong and Kacperczyk (2009)). It is thus important to understand the relation between stock returns and negative screens. This analysis is also relevant because the relation between ESG ratings and future returns may be non-linear, a possibility we have underplayed thus far.

Several theoretical arguments in the literature suggest that negative screens are likely to hurt performance. A common argument is that if enough investors exclude unsustainable stocks from their portfolios they put downward pressure on prices and the remaining investors are forced to over-weight those stocks to the point in which they demand compensation for bearing diversifiable risks, such as litigation risks (e.g., Heinkel, Kraus and Zechner (2001), Hong and Kacperczyk (2009)). Related arguments include shunned stocks becoming less liquid or being neglected by analysts and media, thus commanding a risk premium à la Pastor and Stambaugh (2003) and Merton (1987b), respectively. Negatively screened stocks may also outperform because they carry higher systematic risk reflecting either hedging demands by investors with heterogeneous preferences (Zerbib (2020)) or a boycott risk premium that captures shifts in investors' propensity to boycott unsustainable stocks (Luo and Balvers (2017)). The systematic risk in these models can, for example, reflect economy-wide fluctuations in social norms and variation in the propensity to boycott stocks along the business cycle.

There are also counter-arguments, however. A common feature of these models is that the share of wealth held by the investors who apply negative screens must be relatively high, which casts doubt on the view that negative screens can affect stock returns in practice (e.g., Broccardo, Hart and Zingales (2020)). Consistent with this idea, Gantchev, Giannetti and Li (2017) find that divestment following ESG incidents does not lead to persistent pricing effects outside the three-day event window around the incident. Another piece of evidence comes from Bolton and Kacperczyk (2021) who find that negative screens based on carbon emissions do not affect stock returns. A further challenge has been raised by Davies and Wesp (2018). Under the assumption that firm managers are maximizing value in the absence of the threat of negative screens, the only way to avoid negative screens is to pursue projects which destroy value in the long-term. Managers and investors oriented towards the long-term are thus likely to ignore divestment threats.

We explore this question by zooming in on the performance of low ESG stocks. This is one of the most widely used types of negative screens by investors (e.g., Amel-Zadeh and Serafeim (2017)) and yet it has received relatively less attention in the literature compared to screening out sin stocks.[30]

[30] Several papers find that sin stocks outperform (e.g., Fabozzi, Ma and Oliphant (2008), Hong and Kacperczyk (2009), Statman and Glushkov (2009)). The annualized alphas vary widely, ranging from 1.43% to 19%. Blitz and Fabozzi (2017), however, find that sin stocks' alpha shrinks to zero once the quality factors of Fama and French (2015) are controlled for. In other words, sin stocks may tend to deliver high returns because they are profitable and pursue conservative investment strategies.

In particular, we regress monthly stock returns on a dummy variable which equals one for stock-months that belong to the bottom 10% of the distribution of ESG ratings or ESG momentum in that month. We also show the results obtained when redefining the dummy variable so that it identifies the worst ESG performers in a given GICS sector and month (worst-in-class). We include the full range of control variables and fixed effects used in the previous sections. Intuitively, we compare the performance of the most unsustainable stocks in our sample to the performance of the more sustainable stocks that a socially-minded investor is likely to over-weight if he shuns the worst ESG performers. We thus evaluate to what extent such a shift in portfolio weights hurts performance.

We present the results in Figure 3.8. The top (bottom) plots show the results using ESG ratings (ESG momentum). The left (right) plots show the results for the worst performers relative to the entire sample (relative to sector peers). Notably, across all four plots there is a tendency for the effect sizes to be negative, albeit imprecisely estimated in most cases.

It is interesting to note, however, that the few cases in which we find evidence for a significant relation between ratings and future returns occur when using *Composite*'s *E* rating and MSCI's *E*, *G*, and *ESG* ratings. The economic magnitudes are often relatively large. For example, the stocks with the lowest MSCI *E* ratings have on average 2.4% lower annualized returns than the remaining better rated firms do. Although we do not want to overinterpret isolated results, one reason for this might be that MSCI ratings predict future value-relevant news better than Sustainalytics and Refinitiv ratings do (e.g., Serafeim and Yoon (2021)). If so, our results might be explained by the finding in previous literature that low ESG ratings are noisy predictors of value-destroying ESG news (e.g., Serafeim and Yoon (2021)) and that investors react negatively to such news (e.g., Krüger (2015), Glossner (2018)).

Figure 3.8. Negative screens and stock returns

This figure summarizes the results from running panel regressions of monthly stock returns on a dummy variable that equals one for stock-months that belong to the bottom 10% of the distribution of lagged ESG ratings or ESG momentum in that month. All regressions include all control variables listed in Appendix Table B.1 as well as country-month and industry-month fixed effects. Following Bolton and Kacperczyk (2021), industries are defined based on six-digit Global Industry Classification Standard Codes (GICS) codes. The plots on top (bottom) present the results obtained using ESG ratings (ESG momentum). The plots on the left (right) present the results obtained when we define the dummy to be equal to one for stock-months belonging to the bottom 10% of ESG performers relative to the rest of the sample (relative to its sectors peers) in that month. Each bar represents the regression coefficient on the ESG variable used in a given regression. We use one of four types of ESG momentum as an independent variable: environmental (*E*), social (*S*), governance (*G*), and ESG (*ESG*). We use ratings from four raters: Refinitiv, MSCI IVA, Sustainalytics, and *Composite*. *Composite* combines the available ratings of the other three raters by averaging their ratings. We convert the ratings of each of the other three raters at each point in time to percentile ranks before averaging. Confidence intervals are depicted as whiskers around the point estimates. The longer (shorter) whiskers represent 95% (90%) confidence intervals. Standard errors are double clustered at the stock and month levels. The sample period is from January 2001 to December 2020.

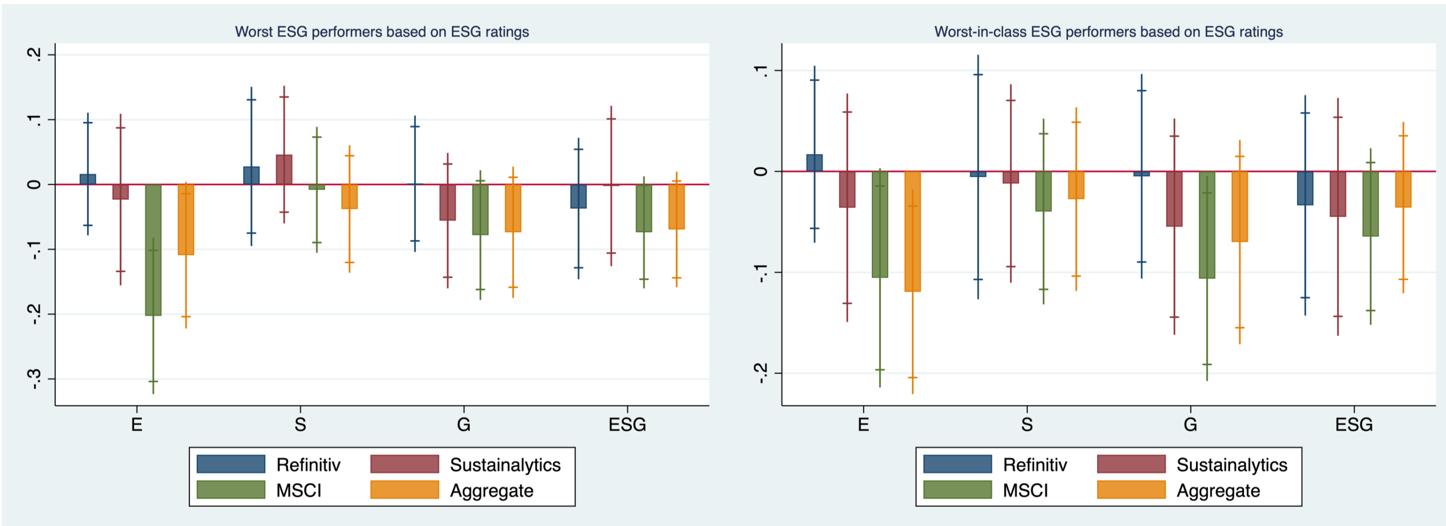

Figure 3.8. Negative screens and stock returns

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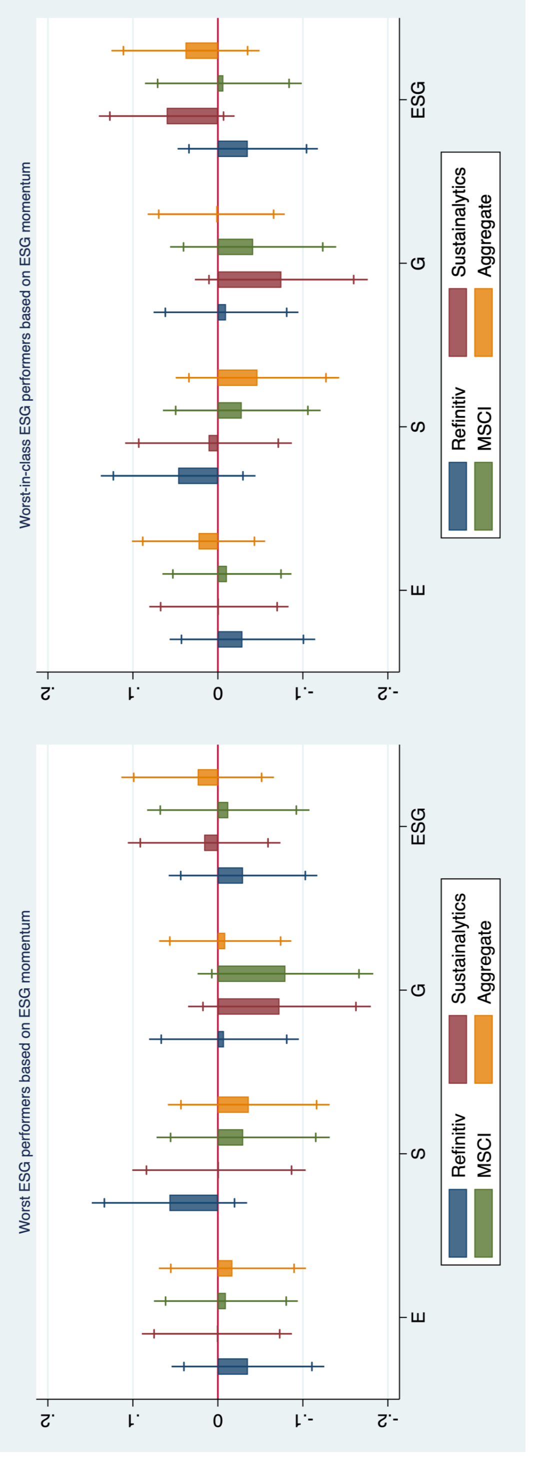

Overall we do not find strong evidence that the worst ESG stocks deliver particularly good or bad returns relative to more sustainable stocks, indicating that socially-minded investors may have been able to divest from the worst ESG performers without compromising on returns. This suggests that, so far, either low ESG stocks were not shunned by investors or that shunning, to the extent to which it occurred, did not have meaningful pricing effects. Our results thus provide some support for the skepticism raised in recent theoretical work about the ability of negative screens to affect firms' cost of equity (e.g., Davies and Wesp (2018), Broccardo, Hart and Zingales (2020)).

3.4. Conclusion

Is sustainable investing a royal road to higher risk-adjusted returns as many in the financial industry claim?[31] And is sustainable investing a promising market-based mechanism to solve social and environmental externalities (e.g., Fama (2021))?

Our results suggest there is little reason to be optimistic about either possibility, at least based on what has happened in the past. Using a comprehensive dataset spanning 9,253 stocks in 46 countries over the last two decades we find that it is seldom the case that higher ESG ratings are associated with higher future stock returns. This general finding holds for both negative and positive screens, for different dimensions of sustainability (*E*, *S*, *G*, *ESG*), whether we use ratings in levels or changes, and whether we use either one of the three most widely used ratings in the industry or combinations of these ratings. It also holds in our global sample under different specifications, within all major

[31] For example, according to a recent Financial Times article (Riding, 2020), the chief executive of a leading sustainability-oriented asset management company stated "ESG factors are not just 'nice to have' but drivers of outperformance.". Larry Fink, in his 2021 annual letter to shareholders, wrote the following: "(...) our deeply held investment conviction is that integrating sustainability can help investors (...) achieve better long-term, risk-adjusted returns."

world regions other than emerging countries, and within the various sectors of economic activity.

There are silver linings, however. First, our results suggest that it has been possible to "do good without doing poorly" in the last two decades. This indicates that the prediction (e.g., Pastor, Stambaugh and Taylor (2020)) that sustainable investing satisfies investors' non-pecuniary preferences at the expense of financial returns may not have yet materialized in an economically meaningful manner. Whether or not that will happen is uncertain. It will likely depend on factors such as whether or not enough investors are willing to substitute social for financial returns (e.g, Fama and French (2007)) and whether or not investors' pro-social preferences are elicited and incorporated in investment decisions by investment managers (e.g., Bauer, Ruof and Smeets (2021)). Second, our results cast doubt on the idea that there is widespread overvaluation in sustainable stocks. This is reassuring given the widespread concern that we are currently living through an ESG "bubble".[32]

We end with a word of caution about our results. We do not rule out that our results could be different if we would use different raters or more complex trading strategies, such as combining ESG ratings with other information sources or using ratings in fundamental analysis (ESG integration). Our results are also silent about the ability of ESG engagement strategies to make firms more sustainable and create financial value (e.g., Dimson, Karakaş and Li (2015), Barko, Cremers and Renneboog (2018)). Nonetheless, at the very least our paper provides comprehensive evidence that there may be good reason to be skeptical about claims that ESG investing outperforms or is a reliable alternative to non-market based solutions when it comes to solving the major climate and social problems of our time.

[32] See, for example, the Financial Times article (Temple-West, 2020) entitled "'Monstrous' run for responsible stocks stokes fears of a bubble".

Chapter 4

Social Networks and Corporate Social Responsibility

Are corporate social responsibility policies transmitted across firms through the social networks of their executives and directors? If so, which factors explain this phenomenon? Using social network data covering 83,604 top executives and directors of Russell 3000 firms from 2001 to 2016, I show that firms' CSR decisions are influenced by the CSR decisions of their social peers. My findings are consistent with the notion that some firms use social networks as a strategic information sharing tool with the aim of increasing firm value. Overall, this paper suggests that social learning through corporate social networks leads to a social multiplier in CSR investment decisions that not only amplifies the externalities of CSR on society but may also be consistent with profit maximization incentives.

In theory, social peer effects in CSR can arise for at least two reasons. On the one hand, firms may benefit from using the social networks of their executives and directors to obtain information about how to optimally design CSR projects and create value. The underlying reason is that the benefits of CSR are often intangible and are only realized over the long-term or in specific states of the world such as financial crises (e.g., Lins, Servaes and Tamayo (2017), Edmans (2020)). It is therefore challenging to precisely estimate the net present value (NPV) of alternative CSR projects and implement optimal CSR policies. Firms that are able to overcome these investment frictions by learning from their social peers may therefore be better equipped to design CSR policies and gain a competitive edge over their industry rivals.

On the other hand, executives and directors may mimic their social peers to maximize their private utilities. This can happen in at least two ways. First, executives and directors may internalize their peers' pro-CSR ideals and gain utility from acting according to those ideals (e.g., Akerlof and Kranton (2000)). Second, even if executives and directors do not internalize their peers' ideals, they may still decide to mimic their peers and abide by social norms of behavior to extract benefits such as peer esteem and board appointments (e.g., Bénabou and Tirole (2011a), Levit and Malenko (2016), Akerlof (2017)). For example, directors may find it in their best interest to be supportive of diversity policies whenever their peers also support such policies, even if they believe such policies are unlikely to create value and even if such policies do not stem from their own ideals.

Despite intuitive theoretical foundations, the identification of peer effects in CSR is challenging due to the difficulty of (i) separating peer effects from common unobserved shocks, and (ii) disentangling whether firms respond to the CSR decisions of their peers or to some other peer characteristic (Mansky (1993)). I deal with these empirical challenges by extending the identification strategy of Bramoullé, Djebbari and Fortin (2009), which explores the fact that not all the peers of a firm's social peers are socially connected with that firm. The CSR decisions of these indirect peers can thus be used as a valid instrumental variable for the CSR decisions of the firm's social peers. I build on this strategy by exploiting the fact that the CSR decisions of firms in the same industry are strategic complements (Cao, Liang and Zhan (2019)). I thus define the indirect peers of a firm *i* as the industry peers of firm *i*'s social peers that do not have social, industry or geographic ties with firm *i*. The identifying assumption is that, conditional on a high-dimensional set of control variables and industry-by-year, headquarters region-by-year and state-of-incorporation-by-year fixed effects, the CSR decisions of indirect peers only systematically affect the CSR decisions of firm *i* through its social peers. In addition, I focus the analysis on peer effects between social peers in different industries to fully disentangle industry and social peer effects.

I find that social peer effects are present for both the environmental and social dimensions of CSR. Firms with average levels of CSR increase their CSR by 16% in response to a one standard deviation increase in the average CSR of their social peers. Such a magnitude is comparable to the industry peer effects of CSR documented by Cao, Liang and Zhan (2019). This finding is robust to a wide range of exercises, including applying network community detection algorithms to form communities and control for time-varying endogenous network formation, simulating placebo networks to alleviate the concern that the results are driven by latent common factors, employing different combinations of fixed effects, first-differencing the regression equations, alternating between contemporaneous and lagged specifications, using alternative definitions of peers based on different industry and geographic boundaries, excluding board interlocks in which firms share the same directors, excluding firms with a very low or a very high number of social peers, using CSR scores from the Thomson Reuters ESG database instead of the MSCI ESG database, applying the partial identification method of Oster (2019) and controlling for a multitude of firm-level and peer-level variables. In addition, I show that the result holds when employing two quasi-natural experiments: (i) a difference-in-differences (diff-in-diff) design based on the deaths of directors and executives, and (ii) a regression discontinuity design based on shareholder-sponsored CSR proposals.

Having shown that social peer effects in CSR are a robust empirical regularity, I turn to the question of which individuals are responsible for the peer effects. I find that peer effects occur through the social networks of directors but not through those of executives. This is plausible for at least three reasons. First, directors are better positioned than executives to exchange information by virtue of sitting on the boards of several firms throughout their careers. Second, firms typically have more directors than they have top executives. This makes it easier for firms to acquire information through their board than through their top executives. To illustrate, in my sample the average firm can reach four times as many firms through the directors network compared to the executives network. Third, CSR encompasses several strategic considerations which are of interest to the board. The same strategic considerations are likely not to be directly related to the job descriptions of all top executives.

To understand why directors play such an important role, I look inside the boardroom. If directors drive peer effects either because they have a comparative advantage in information acquisition or because it is part of their job description to be informed, peer effects should on average be stronger for firms with CSR board committees in place. Directors sitting on these committees are responsible for monitoring and providing advice on CSR policy and are, therefore, more likely to actively engage in information acquisition on this issue. In line with this hypothesis, I find that firms with CSR committees mimic more than firms without such committees. Also, while firms with CSR committees mimic other firms with CSR committees the most, they also mimic firms without those committees. This is in contrast with firms without CSR committees which mostly mimic firms without CSR committees. Since firms without CSR committees tend to be smaller and have smaller social networks, my findings suggest these firms mimic less because they lack access to valuable information held by larger firms with specialized CSR committees and more CSR know-how.

Next, to better understand the nature of social peer effects in CSR, I turn to the question of which firms mimic. Two findings emerge. First, peer effects are concentrated in firms pursuing product differentiation strategies and firms operating in industries with high CSR intensity. Following previous literature (e.g., Servaes and Tamayo (2013), Albuquerque, Koskinen and Zhang (2019)), I measure product differentiation based on advertisement expenditures. Given the evidence in this literature that CSR is used as a value-enhancing product differentiation strategy, my results suggest that firms try to obtain information from their social peers in different industries to gain a competitive edge over industry rivals.

Second, peer effects are stronger for large firms that are strategically positioned in the corporate social network to acquire valuable information. I quantify the strategic positioning in the network with two variables that are designed to capture information social capital, that is, the ability of a node to acquire and spread valuable information in a network: decay centrality (Jackson (2008)) and diffusion centrality (Banerjee et al. (2013)). This result holds when controlling for the number of social peers and firm size, thus alleviating the concern that the results are spuriously driven by a size effect. This is, to the best of my knowledge, the first piece of direct evidence in favor of the conjecture of Dougal, Parsons and Titman (2015) that large firms have a comparative advantage in learning from their social peers due to their privileged position in the corporate social network.

I further address the question of why firms mimic by constructing more direct tests of whether peer effects are driven by social learning or by alternative channels such as social norms. To test for social learning, I would ideally measure whether or not firms mimic their social peers with the intention of learning and creating value. Since intentions are unobservable, I test instead for whether or not peer effects are stronger for firms in which the incentives of managers and shareholders are more aligned. The underlying assumption is that firms with more aligned incentives are more likely to mimic their social peers with the goal of learning and creating value. Consistent with a social learning explanation, I find peer effects are concentrated in firms with higher CEO pay-sensitivity to performance (delta), stronger board independence and, to a lesser extent, higher levels of industry competition. Moreover, I find peer effects are stronger for firms in which the convexity of CEO compensation (vega) is higher. Insofar as convexity in compensation contracts is often used to incentivize risk-averse managers not to pass up on risky positive NPV projects (Guay (1999)), this is in line with the idea that social learning alleviates a CSR underinvestment problem caused by investment frictions such as uncertainty and irreversibility (e.g., Guiso and Parigi (1999)).

I test for an alternative channel related to the evidence that CSR decisions are influenced by the social norms of executives and directors (e.g., Giuli and Kostovetsky (2014), Cronqvist and Yu (2017)). If social peer effects are caused by social norms, peer effects should be stronger for firms with more social capital along the dimensions of (i) civic engagement and pro-social preferences, and (ii) ability to enforce punishment threats that sustain cooperation and prosocial behavior.

I quantify these dimensions of social capital by exploiting two facts. First, there is large county-level variation in several measures of geographic social capital that have been widely used in the literature (e.g., Guiso, Sapienza and Zingales (2004), Lin and Pursiainen (2018)), namely: organ donation density, voter turnout, number of tax-exempt non-profit organizations per capita, and number of non-profit or recreational associations. Second, there is substantial evidence that firms absorb the social norms of the county where they are headquartered and that these norms influence corporate decision-making (e.g., Jha and Chen (2014), Hasan et al. (2017a), Hasan et al. (2017b)). Following the literature, I assign county-level measures of social capital to firms based on the location of firms' headquarters. At odds with this social norms channel, however, I do not find evidence that peer effects depend on geographic social capital. This holds when using each variable individually, when constructing variables based on peers' social capital and when constructing measures based on the principal component of the individual variables.

My paper contributes to the burgeoning literature documenting drivers of CSR (e.g., Flammer (2015b), Dimson, Karakaş and Li (2015), Dai, Liang and Ng (2020), Flammer and Kacperczyk (2019a), Dyck et al. (2019)) and to the broader literature on peer effects in corporate finance (e.g., Shue (2013), Leary and Roberts (2014), Kaustia and Rantala (2015), Fracassi (2017) and Grennan (2019)). The closest paper to mine is Cao, Liang and Zhan (2019), who document that industry peer effects in CSR arise because firms mimic each other to stay competitive. The key difference is that, in contrast to industry peer effects that arise out of competitive dynamics unrelated to social networks, the peer effects documented in this paper occur through cross-industry collaborative social interactions.

A long-standing question in CSR research is whether CSR is a tool to create long-term value (e.g., Edmans (2011) and Deng, Kang and Low (2013)) or a manifestation of agency problems (e.g., Giuli and Kostovetsky (2014) and Masulis and Reza (2014)). I contribute to this debate by showing that, contrary to the predictions of the agency view of CSR, firms with fewer agency problems put more effort to learn from their social peers and actively engage in CSR.

My paper also speaks to the literature on the role of the board of directors in corporate governance. Whereas previous studies have explored functions of the board such as monitoring, advising, CEO hiring and firing and setting CEO compensation (e.g., Adams, Hermalin and Weisbach (2010)), I show that the board also influences CSR policy. A contemporaneous study by Iliev and Roth (2020) documents that firms increase their CSR if their directors sit on the boards of foreign firms exposed to country-wide sustainability regulations. My paper complements theirs by identifying a distinct channel through which directors influence CSR.

Furthermore, I shed light on the extent to which different dimensions of firm social capital complement or substitute for one another. As pointed out by Servaes and Tamayo (2017), firm social capital, broadly understood as the level of trust and cooperation between the firm and its stakeholders, is multidimensional and we know little about how these different dimensions interact with each other. I fill the gap by providing evidence that more information social capital (i.e., better connected directors) induces firms to accumulate more social capital in the form of CSR. In addition, I show there is little evidence that geographic social capital mediates peer effects. This suggests that not all dimensions of firm social capital are complements.

4.1. Data and summary statistics

4.1.1. Social network construction

I construct social networks based on individual connections of top executives and directors for the largest 3000 publicly traded US companies (Russell 3000) with at least $10 million in assets. The data is sourced from the BoardEx database and covers the period 2001-2016.

Following Fracassi (2017), I define top executives as the top five executives based on compensation data from ExecuComp. Since the ExecuComp universe is the S&P 1500, I cannot apply this definition to all Russell 3000 sample firms. In those cases, I define the top executives to be the CEO, CFO and COO. The final sample comprises 83,604 individuals.

Building on Fracassi and Tate (2012), I consider four types of Boolean individual-level networks: current employment, past employment, other activities, and education networks. Current employment networks capture professional relationships that occur when two individuals sit on the same board or C-suite. Past employment networks are defined in the same way except for the fact that they capture past relationships that are no longer active in the current year. As for the other activities network, individuals are defined to be connected if they have active roles in the same clubs, charities or organizations. I assume that active memberships are those that are not simply described as *member* in BoardEx (e.g., President, trustee). As for the education network, individuals are said to be connected if they graduated from the same university with the same degree type within one year of one another.

I then aggregate the individual-level networks into firm-level networks. For each year and network type, I define two firms to be linked if at least two individuals working in those firms are connected in the underlying individual-level network. To capture board interlocks, two firms are also considered to be connected if they share a director. In addition, to reduce the possibility of capturing spurious social connections, I require the headquarters of firms that are socially connected through education, other activities and past employment networks to lie in the same Combined Statistical Area (CSA). CSAs, as defined by the United States Office of Management and Budget (OMB), are geographic polygons that combine areas with strong economic, social and commuting links. There are 172 combined statistical areas in the US, the largest of which is New York - Newark, with over 22 million inhabitants. As in Fracassi (2017), I sum across the four types of networks for each year separately and row-normalize the network matrices to obtain time-varying network weights. For a detailed explanation of the network construction approach, refer to Appendix C.2.

4.1.2. Corporate social responsibility variables

I use data from the MSCI ESG Stats Database to construct CSR scores. Following previous literature (e.g., Cao, Liang and Zhan (2019)), I focus on the following CSR categories: employee relations, community relations, environment, and workforce diversity. This approach reduces the likelihood that the CSR measure captures governance, product market competition and other industry-specific information. Binary scores in each category are available in the form of strengths and concerns in various subcategories. I follow Flammer and Kacperczyk (2019a, 2019b) and sum over all the strengths within each category. Since the maximum number of strengths within each category can vary over time, I scale by the total possible number of strengths for each firm-year. In a final step, I sum the scaled scores across all four categories to obtain an overall measure of CSR.[33] In robustness tests, I also use CSR scores from the Thom-

[33] Until recently, a typical approach in the literature was to subtract the concerns from the strengths. However, this procedure can be unreliable due to conceptual differences between

son Reuters ESG database. I compute these alternative scores by averaging the environmental and social scores constructed by Thomson Reuters.

4.1.3. Other variables

I use several proxies to measure the extent to which managers and shareholders' incentives are aligned. The first two proxies are CEO delta and CEO vega. Following Core and Guay (2002) and Coles, Daniel and Naveen (2006), delta is defined as the dollar-value sensitivity of the executive's stock and option portfolio to a one percentage point change in the stock price. Vega is the dollar-value sensitivity of the executive's portfolio to a 1/100 change in the annualized volatility of stock returns. I also use the Herfindahl-Hirschman index of Hoberg and Phillips (2016) as a measure of industry competition. The index is based on sales data and defined as the sum of the squared market shares of all firms in a given industry and year.

To quantify the extent to which firms are embedded in a social network rich in social capital, I use known proxies for county-level social capital (e.g., Rupasingha, Goetz and Freshwater (2006)): non-profit or recreational association density, registered tax-exempt non-profit organization density, voter turnout, and organ donation density. I obtain firm-specific measures of social capital by assigning county-level social capital to each firm based on headquarters location. Appendix C.3 provides a detailed description of these variables.

I collect voting data on shareholder-sponsored CSR proposals from the Institutional Shareholders Services (ISS) voting analytics database and from SharkRepellent. The ISS database covers S&P 1500 firms from 2003 to 2016 and SharkRepellent covers Russell 3000 firms from 2005 to 2016. The data on the deaths of directors is retrieved from BoardEx.

strengths and concerns (e.g., Mattingly and Berman (2006), Flammer (2018)). In untabulated results I do not find evidence for social peer effects in CSR concerns.

I employ the standard control variables in the literature related to firm size, leverage, profitability, liquidity, dividend payout, indebtedness and institutional ownership. Accounting variables are sourced from Compustat and institutional ownership data from Thomson Reuters. Detailed variable definitions are provided in the C.1.

4.1.4. Summary statistics

The plots in Figure 4.1 below show the cross-firm distribution of network degree centrality, that is, the number of social connections a given firm has. Degree centrality is measured as of 2009, the median sample year. Slightly over 50% of the firms have less than 50 connections, with fewer than 1% of firms having more than 250 connections. The average number of connections is 66 and the maximum is 365. Hence, there is a small number of firms with a large number of connections and a large number of firms with few connections.

This suggests that there is substantial cross-firm variation in access to information and that the benefits of information exchange may only accrue to a limited number of firms. Lins, Servaes and Tamayo (2017) conjecture that the high cost of CSR investments may be a reason for why not all firms engage in CSR. To the extent that smaller and more financially constrained firms also tend to have fewer network connections, lack of access to information via social networks might magnify the costs of engaging in CSR even further for these firms.

Table 4.1 provides summary statistics on the main variables for the full sample as well as for the lowest and highest terciles of the distribution of degree centrality. Firms with higher degree centrality tend to be larger, more profitable, distribute more dividends, spend more on advertising relative to their

Figure 4.1. Network degree distribution

The histogram on the left depicts the degree distribution of firm-level social connections in 2009 (the median year in the sample). The degree (or degree centrality) of a given firm is defined as the number of social connections that firm has. The heatmap on the right displays the firm-level social network in 2009 and the degree centrality of each firm. Warmer colors indicate higher degree centrality. To ease visualization of the heatmap, I exclude firms that have strictly less than two peers.

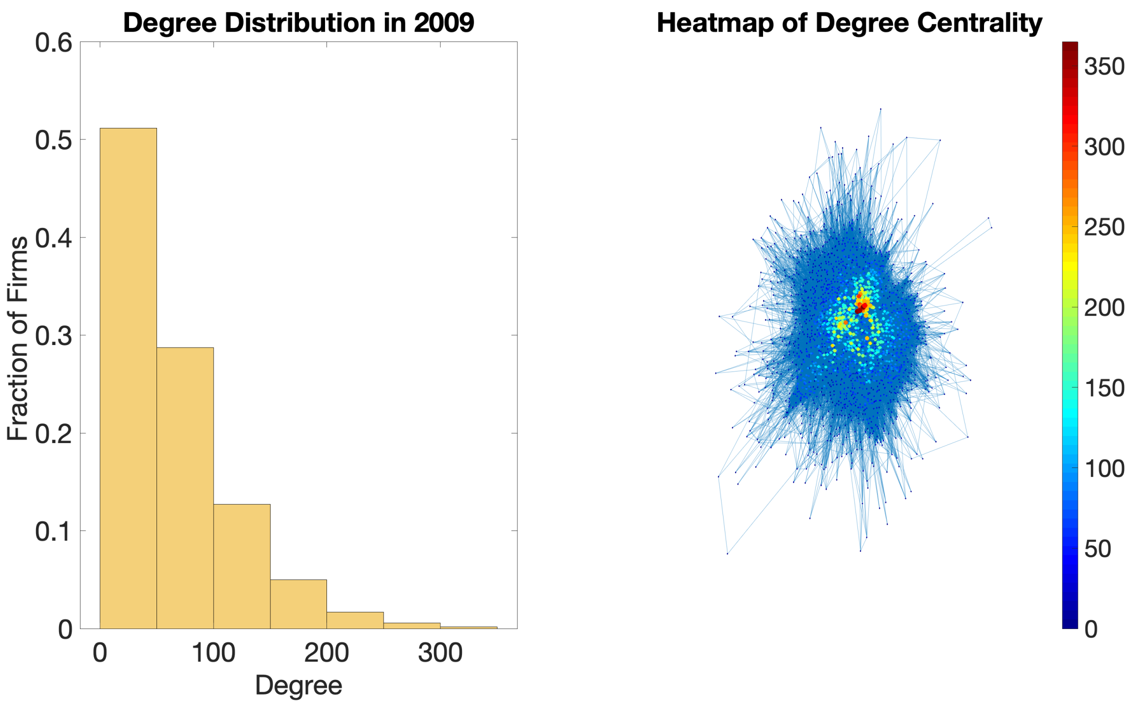

Table 4.1. Summary statistics

This table reports the summary statistics for the main variables used throughout the paper. See C.1 for variable definitions. The columns under the *Low Degree (L)* (*High Degree (H)*) label contain means and standard deviations for the firm-years that belong to the lowest (highest) tercile of the distribution of degree centrality. The degree centrality of a given firm is defined as the number of social connections the firm has. The column *H minus L* presents the difference in means between the high and low degree centrality subsamples. The last two columns present statistics for the full sample. All control variables are winsorized at the 1% and 99% levels. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

	Low Degree (L)		High Degree (H)		H Minus L	Full Sample	
	Mean	SD	Mean	SD	Diff. Means	Mean	SD
Firm Attributes							
Size	6.997	1.467	8.560	1.800	1.563***	7.644	1.730
MB Ratio	2.677	3.431	3.205	4.256	0.528***	2.984	3.893
Debt Ratio	0.218	0.219	0.252	0.194	0.034***	0.236	0.210
ROA	0.026	0.113	0.031	0.105	0.005***	0.024	0.116
Net Income	71.992	236.692	800.194	1,643.976	728.202***	344.856	1,065.201
Cash Ratio	0.164	0.196	0.158	0.179	-0.006**	0.169	0.196
Divid. Ratio	0.012	0.023	0.015	0.022	0.003***	0.013	0.022
Inst. Own.	0.615	0.288	0.595	0.289	-0.019***	0.607	0.286
Cust. Awa.	0.009	0.021	0.013	0.028	0.004***	0.010	0.024
R&D	12.606	36.694	186.755	486.038	174.149***	80.743	303.665
CSR score	1.113	0.248	1.476	0.625	0.363***	1.261	0.467
Sample							
No. Firms	2,821						
Sample Period	2001-2016						
No. Obs.	25,808						

sales, have more debt and invest more in both R&D and CSR. Further statistics outlining the features of the network are available in Appendix C.4.

4.2. Empirical model and identification strategy

4.2.1. The empirical model

In line with a growing literature in finance (e.g., Leary and Roberts (2014), Grennan (2019), Silva (2019)), I employ the standard linear-in-means model of peer effects:

$$y_{ijklt} = \alpha + \beta \bar{y}_{-ijklt} + \lambda' \bar{X}_{-ijklt} + \gamma' X_{ijklt} + \mu_{jt} + \delta_{kt} + \zeta_{lt} + \epsilon_{ijklt} \tag{4.1}$$

The dependent variable y_{ijklt} is the CSR score of firm *i* in year *t*, operating in industry *j*, headquartered in CSA region *k* and incorporated in state *l*. Firm *i*'s CSR is assumed to be a linear function of the mean outcome of its peer group ($\bar{y}_{-ijklt}$). The mean outcome for firm i is defined as the weighted average of the CSR scores across all its peers, excluding firm i itself. The weights are proportional to the strength of the social connection between each firm pair. As in Leary and Roberts (2014), I focus on a contemporaneous measure of peer effects. Nevertheless, I show that the results are robust to employing lagged specifications. The model further controls for the average characteristics of firm i's peer group ($\bar{X}_{-ijklt}$), its own characteristics (X_{ijklt}) and a set of three-digit SIC industry-by-year (μ_{jt}), headquarters CSA-by-year (δ_{kt}) and state-of-incorporation-by-year (ζ_{lt}) fixed effects.

4.2.2. The identification problem

As shown by Mansky (1993), the identification of peer effects is challenging for two reasons. First, it is necessary to separate peer effects that occur through social interactions (e.g., mimicking due to social learning) from correlated effects that arise due to latent common factors that induce changes in CSR in all firms within a peer group. Examples of correlated effects that have been documented in the literature are state-wide regulations that affect CSR, such as: state unemployment insurance benefits (Flammer and Luo (2015)), inevitable disclosure doctrines (Flammer and Kacperczyk (2019a)), and constituency statutes (Flammer and Kacperczyk (2019b)). Second, the behavior of a firm within a peer group simultaneously affects and is affected by other firms in the group, generating collinearity between the mean CSR decisions and the mean characteristics of the group. This so-called reflection problem makes it difficult to identify whether firms' CSR responds to the CSR decisions of its peers or to some other peer characteristic.

4.2.3. The identification strategy

My identification strategy builds on Bramoullé, Djebbari and Fortin (2009) and Giorgi, Pellizzari and Redaelli (2010), who formally show that the reflection and correlated effects problems can be solved in networks with partially overlapping peer groups. The firm-level social networks that are the focus of this paper satisfy this requirement. The underlying intuition is that social networks are rich in intransitive triads, meaning that firms are not connected with all the peers of their peers - thus generating indirect peers. Therefore, the actions of a firm's indirect peers affect that firm's actions through its peer group. This, in turn, generates within peer-group variation and breaks the reflection problem.

In practice, this strategy is operationalized by using the behavior of a firm's indirect peers as an instrumental variable for the CSR decision of a firm's peer group. I extend this idea by exploiting the fact that firms are part of geographic, social and industry networks, all of which are partially overlapping with respect to each other. This allows me to define the indirect peers of each firm i as the industry peers of the social peers of firm i and to use their CSR policies as an instrument for the CSR policies of firm i's social peers. In addition, I impose that (i) indirect peers are neither social peers nor industry peers of firm i, and (ii) indirect peers and firm i are headquartered in different geographic areas (CSAs).

The validity of this strategy hinges on the instrument being strong and satisfying the exclusion restriction. The expectation that the instrument is strong is supported by previous literature documenting economically significant industry peer effects in CSR decisions (Cao, Liang and Zhan (2019)). The exclusion restriction is that, conditional on CSA-by-year, state-of-incorporation-by-year and industry-by-year fixed effects (and remaining control variables), the average CSR decision of the industry peers of the social peers of a firm, which do not share any geographic, industry or social links with that firm, should only affect the CSR decision of that firm through its social peers. This seems reasonable for several reasons.

First, since I impose that there are no social, industry or geographic links between indirect peers and the firm being instrumented, there is no obvious channel based on industry competition, transfer of information over social networks or local events through which indirect peers would directly affect the firm. It is possible, however, that an indirect peer of a firm is involved in an attention grabbing event with news coverage and that the event leads both the indirect peer and the firm to change CSR in tandem. For example, a human rights scandal relating to an indirect peer outsourcing activities to a socially irresponsible firm in another country could produce this effect. Note, however, that such events are rare both across time and across firms, making it unlikely that such events systematically confound the results. Moreover, since the instrument is constructed as the average of CSR scores over a large number of indirect peers, the instrument is bound to have very little correlation with such rare firm-specific events. To alleviate this concern further, I show in robustness tests that the results do not change if I restrict the sample to firms that have many indirect peers.

Second, it is reasonable to assume that the instrument is orthogonal to the omitted variables causing endogenous sorting into social networks. Suppose there is such a variable (e.g., political views) that causes both social connections and CSR investment in firms. It would have to be the case that the average CSR decisions of indirect peers are systematically correlated with this variable. This is unlikely because I impose that the indirect peers are not social, geographic or industry peers of the firm being instrumented. Despite that, I reduce this concern in robustness tests by using community detection algorithms to identify network communities and controlling for network community-by-year fixed effects.

Third, I include high-dimensional fixed effects that capture a wide-range of common unobserved shocks. Industry-by-year peer effects control for time-varying industry-specific shocks as well as industry peer effects in CSR. In addition, CSA-by-year fixed effects separate social peer effects from geographic peer effects in CSR. These could arise due to, for example, exposure to common laws, geographic variation in social norms (e.g., Rupasingha, Goetz and Freshwater (2006)) and local agglomeration economies (e.g., Dougal, Parsons and Titman (2015)). In order to better control for the time varying-effect of laws, I also include state-of-incorporation-by-year fixed effects. Furthermore, I show the results are robust to defining indirect peers based on different geographic (state instead of CSA) and industry (one-digit SIC instead of three-digit

SIC) boundaries and applying first differences to equation (4.1), which eliminates time-invariant firm unobservables.

Fourth, I show that the results break down when using placebo networks. If correlated effects were driving the results, we should find peer effects in these placebo networks. Therefore, the lack of evidence for such spurious effects is evidence for the reliability of this identification strategy. Nevertheless, I acknowledge that no strategy based on a non-shock instrumental variable can completely rule out endogeneity concerns. For this reason, I complement this IV strategy with a quasi-experimental regression discontinuity design based on close-call CSR proposals and a diff-in-diff based on the deaths of executives and directors.

4.3. Do social peers mimic each other?

4.3.1. Baseline results

Table 4.2 presents the results from estimating model (4.1) via two-stage least squares (2SLS). The instrument is the average CSR score of indirect peers. All the coefficients are measured in standard deviation units to ease interpretation. t-statistics are reported in parentheses and standard errors are heteroskedasticity-robust and clustered at the firm-level. All regressions include firm-level controls for the same variables that are listed as peer-level controls. I present results using both the benchmark contemporaneous specification (columns (1) through (3)) and a lagged specification (columns (4) through (6)). The lagged specifications alleviate concerns about reverse causality bias.[34]

[34]Note that recent advances in the causal inference literature suggest lag identification seldom eliminates endogeneity bias and often makes the bias worse (e.g., Bellemare, Masaki and Pepinsky (2017)). Intuitively, lagging merely shifts the endogeneity problem by one year. I conduct the falsification tests of Bellemare, Masaki and Pepinsky (2017) to choose the appropriate model and a lagged model is rejected.

Table 4.2. Do social peers mimic each other?

This table reports the results of two-stage least squares (2SLS) regressions of firm CSR scores on social peers' CSR scores. Social peers are defined based on the social networks of executives and directors. The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) operate in different industries; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The additional controls are customer awareness and R&D investment. The coefficients are measured in standard deviation units. The Kleiberg-Paap F-stat is the cluster-robust Kleibergen and Paap (2006) F-statistic for weak instruments. t-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

	Contemporaneous			Lagged		
	(1)	(2)	(3)	(4)	(5)	(6)
Peers' CSR	0.437***	0.450***	0.455***	0.377**	0.391**	0.399**
	(2.623)	(2.712)	(2.827)	(1.979)	(2.060)	(2.200)
Peers' Size	-0.141*	-0.136*	-0.131*	-0.089	-0.089	-0.094
	(-1.652)	(-1.665)	(-1.873)	(-0.916)	(-0.966)	(-1.213)
Peers' MB Ratio	-0.001	-0.001	-0.002	0.005	0.006	0.005
	(-0.121)	(-0.116)	(-0.240)	(0.621)	(0.713)	(0.608)
Peers' Debt Ratio	0.023*	0.018	0.016	0.021	0.018	0.014
	(1.740)	(1.452)	(1.295)	(1.478)	(1.261)	(0.985)
Peers' ROA	0.006	0.004	0.005	0.017	0.015	0.013
	(0.501)	(0.378)	(0.431)	(1.177)	(1.211)	(1.112)
Peers' Net Income	-0.002	-0.016	-0.008	-0.014	-0.027	-0.029
	(-0.067)	(-0.454)	(-0.311)	(-0.351)	(-0.686)	(-1.042)
Peers' Cash Ratio	-0.065	-0.062	-0.045	-0.044	-0.049	-0.042
	(-1.394)	(-1.525)	(-1.499)	(-0.868)	(-1.090)	(-1.291)
Peers' Divid. Ratio	-0.026**	-0.023*	-0.025**	-0.022	-0.017	-0.021
	(-1.979)	(-1.668)	(-2.014)	(-1.519)	(-1.111)	(-1.518)
Peers' Inst. Own.	0.007	-0.002	-0.004	0.007	-0.002	-0.005
	(0.788)	(-0.195)	(-0.434)	(0.680)	(-0.252)	(-0.554)
Peers' Cust. Awa.			-0.020			-0.014
			(-1.341)			(-0.885)
Peers' R&D			-0.026			-0.005
			(-0.825)			(-0.134)
Kleiberg-Paap F-stat	78.543***	64.563***	65.471***	58.380***	47.681***	51.236***
First Stage Instrument	0.199***	0.202***	0.207***	0.186***	0.189***	0.197***
	(8.860)	(8.040)	(8.090)	(7.640)	(6.910)	(7.160)
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	No	No	Yes	No	No	Yes
Ex. Industry Peers	No	Yes	Yes	No	Yes	Yes
No. Obs.	25,808	25,808	25,808	22,958	22,958	22,958

The results in column (1) show that firms increase CSR by 0.437 standard deviations in response to a one standard deviation shock to social peers' CSR (t-statistic of 2.623), suggesting a pass-through effect of at least 40%. This corresponds to a 16% increase in CSR for a firm with an average level of CSR. Note that a one standard deviation shock to the average CSR of social peers is equivalent to a one standard deviation shock to the CSR of all peers (Giorgi, Frederiksen and Pistaferri (2020)). Since the average firm in the sample has 66 peers in the median year (2009), this is a very large shock. Hence, the large economic magnitude of peer effects should be interpreted in light of the large magnitude of the shock.

The results in column (4) show that lagging all right-hand side variables, including the instrument, has little impact on the statistical and economic significance of the results. In both lagged and contemporaneous regressions, the Kleiberg-Paap F-statistic is large and well above the standard cutoff value of 10, suggesting the instrument is sufficiently strong. The coefficient sign of the first stage instrument is positive, in line with the previous literature documenting positive industry peer effects of CSR (Cao, Liang and Zhan (2019)).

One concern is that I allow social peers to be industry peers. While industry-by-year fixed effects should absorb time-varying industry peer effects, it may be that there is within-industry heterogeneity in industry peer effects that are not captured by the fixed effects. To alleviate this concern I re-estimate the regressions excluding all social peers who are also industry peers. The results, reported in columns (2) and (5), suggest the fixed effects do a good job in capturing time-varying industry peer effects as the coefficients barely change.

In columns (3) and (6) I include two additional controls: customer awareness and R&D investment. Servaes and Tamayo (2013) find that the ability of CSR to create value is concentrated in firms with high customer awareness, as measured by the advertisement expenditure ratio. If firms with high customer awareness tend to have peers with high customer awareness, peer effects may spuriously reflect time-varying commonalities in that variable. As for R&D investment, Shen, Tang and Zhang (2019) show that innovative firms use CSR as a signal of long-run orientation to overcome information frictions related to risky transaction-specific investments between the firm and its stakeholders (e.g., suppliers). Since there are economically significant social peer effects in R&D (e.g., Fracassi (2017), Zacchia (2020)), peer effects in CSR may just be an artifact of R&D peer effects. The results show that the estimates are not confounded by the exclusion of these variables.

4.3.2. Robustness tests

In this section I tighten the identification strategy in several ways. First, I apply the partial identification method of Oster (2019) to gauge the extent to which the results are contaminated by biases stemming from time-varying unobservables. Intuitively, I estimate a range within which the true effect lies under the assumption that the degree of selection on unobservables is of the same magnitude as the degree of selection on observables. If the range includes zero one cannot reject the possibility that controlling for all relevant unobservables would render the effect of interest insignificant. Based on the baseline specification with all controls, I find that the range is [0.45,0.56] which suggests that the instrumental variable strategy is able to purge out the endogeneity bias effectively.

Second, I show that the results are robust to: (i) excluding firms with fewer than 10 peers; (ii) excluding firms with more than 250 peers; (iii) imposing that firms and their indirect peers cannot be in the same one-digit SIC industry (instead of three-digit SIC industry); (iv) controlling for the average CSR score of product market peers as defined by the 10-K text-based network industry classifications of Hoberg and Phillips (2010, 2016); (v) using only S&P 1500 firms; (vi) excluding direct board interlocks in which firms share the same directors; (vii) imposing that firms and their indirect peers cannot be headquartered in the same state (instead of CSA); (viii) using headquarters state-by-year fixed effects instead of CSA-by-year fixed effects; (ix) using firm and year fixed effects; (x) lagging the instrument and not lagging the variable being instrumented; (xi) lagging the instrument and the control variables and not lagging the variable being instrumented; (xii) lagging the instrument twice and all the right-hand side variables once; (xiii) not including any controls; (xiv) falsification tests based on placebo networks; (xv) applying community detection algorithms to form network communities and control for time-varying endogenous network formation; (xvi) using CSR scores from an alternative data provider (Thomson Reuters); (xvii) restricting the sample period to end in 2013 instead of 2016. This battery of robustness tests alleviates concerns related to overmeasurement and undermeasurement error of social connections, dependence of results on the definition of geographic and industry boundaries, firm-specific time-invariant unobservables, reverse causality, unobservable common shocks, endogenous network formation, lack of comparability of CSR scores across data providers, and methodological changes in CSR scores the data provider MSCI implemented after 2013. For brevity, all these robustness test results are reported in Appendix Sections C.5 through C.9. Sections C.5 and C.6 of the Appendix also explain the methodologies underlying the placebo network simulations and the community detection algorithms.

Third, I provide evidence that the results are robust to employing two different quasi-natural experiments. This reduces the concern that the results are driven by a hard to detect failure of the IV to eliminate all sources of omitted variable bias. First, I follow Fracassi (2017) and use a diff-in-diff approach based on the deaths of executives and directors as an exogenous shock that breaks social connections. Second, I employ a regression discontinuity design based on CSR proposals in the spirit of Cao, Liang and Zhan (2019) and Dai, Liang and Ng (2020). Results and detailed explanations on the design of the quasi-natural experiments can be found in Sections C.7 and C.8 of the Appendix.

4.4. Which individuals mimic?

The analysis thus far suggests social peers mimic each others' CSR policies. A natural follow-up question is which types of individuals are responsible for the mimicking. As a starting point, I investigate whether social peer effects are driven by the board of directors or the top management team. To address this question, I define social networks that only include either social connections between directors or social connections between executives. I then re-estimate model (4.1) for both networks separately. In both cases, I impose that indirect peers cannot be socially connected with the firm in question through either network. I also decompose CSR into its social and environmental components to allow for the possibility that different types of individuals mimic different types of CSR.

I present the results in Panel A of Table 4.3. In columns (1) and (2) I report the estimates of peer effects over the directors networks for environmental and social CSR, respectively. The peer effects associated with the social component of CSR are substantially larger than those associated with the environmental component. In particular, a one standard deviation shock to social peers' environmental CSR leads to a 4.5% increase in environmental CSR for the average firm. An equivalent shock to the social component of peers' CSR

Table 4.3. Which individuals mimic? The role of directors versus executives

This table reports the results of two-stage least squares (2SLS) regressions of firm CSR scores on social peers' CSR scores. Results are presented by CSR dimension (social versus environmental) and by network type (executives versus directors). The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) operate in different industries; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The additional controls are customer awareness and R&D investment. The coefficients are measured in standard deviation units. The Kleiberg-Paap F-stat is the cluster-robust Kleibergen and Paap (2006) F-statistic for weak instruments. t-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

	Panel A: Levels					
	Directors Network		Executives Network		Aggregate Network	
	Env. (1)	Social (2)	Env. (3)	Social (4)	Env. (5)	Social (6)
Peers' CSR	0.308***	1.059***	0.184**	-0.179	0.378***	1.049***
	(3.028)	(3.429)	(2.323)	(-1.362)	(3.716)	(2.874)
Kleiberg-Paap F-stat	169.453***	34.347***	158.057***	65.460***	175.982***	24.729***
First Stage Instrument	0.288***	0.311***	0.238***	0.282***	0.297***	0.282***
	(13.020)	(5.860)	(12.570)	(8.090)	(13.270)	(4.970)
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs	24,738	24,738	21,990	21,990	24,875	24,875

	Panel B: First Differences					
	Directors Network		Executives Network		Aggregate Network	
	Env. (1)	Social (2)	Env. (3)	Social (4)	Env. (5)	Social (6)
Δ Peers' CSR	0.407**	0.786***	-0.018	-0.074	0.495***	0.760***
	(2.404)	(3.654)	(-0.162)	(-0.481)	(3.381)	(3.031)
Kleiberg-Paap F-stat	73.894***	46.300***	79.016***	37.758***	75.739***	39.815***
First Stage Instrument	0.192***	0.386***	0.162***	0.254***	0.207***	0.760***
	(8.600)	(6.800)	(8.890)	(6.140)	(8.700)	(3.030)
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	21,997	21,997	19,154	19,154	22,116	22,116

leads to a 20.8% increase. Columns (5) and (6) show this finding also holds when using the aggregate network of executives and directors instead of the directors network.

One possible explanation for this asymmetry is that firms can obtain more useful information about the social component of CSR from social peers in different industries compared to what they can learn about the environmental component. This is plausible because the social component of CSR includes dimensions such as employee relations and workforce diversity that are often material to firms across different industries. In contrast, the environmental component of CSR tends to be more material for specific industries such as nonrenewable resources and transportation sectors (e.g., Khan, Serafeim and Yoon (2016)), thus making it harder to learn from social peers in different industries.

In columns (3) and (4) I present the results using the executives network. The peer effects estimates are smaller compared to those obtained using the directors network and are only statistically significant for the environmental component of CSR. The concentration of peer effects in the directors network may be due to directors being better positioned than executives to acquire valuable information on behalf of firms. Not only are directors typically better connected than executives by virtue of sitting in several boards throughout their careers, but firms also tend to have more directors than top-level executives.[35] In addition, directors are more likely than executives to sit on boards of firms in different industries. To illustrate, in my sample the average firm is socially connected to four times as many firms through the directors network as it is through the executives network.

In Panel B I show the results are robust to first-differencing the model,

[35]US firms have on average 9 directors (e.g., Adams (2017)). In contrast, I consider at most the top five executives by compensation when building the executives network.

thus alleviating the concern that the results are driven by firm-specific time-invariant unobservables. There is however one exception: there is no evidence in Panel B for peer effects in either of the two CSR components when using the executives network. Given the evidence that executives networks do not play a role, I focus the analysis on peer effects over directors networks in the remainder of the paper.

Next, I look inside the boardroom to better understand why directors play such an important role. If peer effects are driven by socially connected directors exchanging information on CSR, peer effects should on average be stronger for firms with specialized CSR committees whose job description revolves around CSR. To test this hypothesis, I extend the baseline model by interacting the average CSR scores of social peers with dummy variables capturing whether or not a firm has a specialized CSR committee in a given year.[36]

I report the results in Table 4.4. I present results using specifications in levels (columns (1) through (3)) and in first differences (columns (4) through (6)). All regressions control for the percentage of social peers that have CSR committees in place. This mitigates potential selection biases not purged out by the IV. I also construct social networks that only capture social connections with firms that have CSR committees and social networks that only capture social connections with firms that do not have such committees. The allows to disentangle whether firms mimic peers without committees (columns (2) and (5)), peers with committees (columns (3) and (6)) or both (columns (1) and (4)).

[36]BoardEx provides the names of board committees. I identity the CSR committees based on the following keywords: *environment*, *social responsibility*, *corporate responsibility*, *civic responsibility*, *community*, *ethics*, *sustainability*, *social*, *diversity*, *CSR*, *culture*, *integrity*, *public policy*, and *public responsibility*. Since the SEC only requires firms to disclose details about compensation, audit and nominating committees, there is a possibility that the classification identifies too few firms as having specialized CSR committees. Nevertheless, this is unlikely to generate substantial measurement error because firms lack incentives to hide information about the existence of particular committees (e.g., Adams (2005)).

Table 4.4. Which individuals mimic? The role of CSR board committees

This table reports the results of two-stage least squares (2SLS) regressions of firm CSR scores on social peers' CSR scores. Social peers are defined based on the social networks of directors. In columns (2) and (5) ((3) and (6)) the social peer group only includes social peers without (with) CSR board committees in place. In columns (1) and (4) the social peer group includes all social peers irrespective of whether or not they have a CSR board committee. The magnitude of peer effects is allowed to vary as a function of whether or not firms have a CSR board committee in a given year. $D_{Committee}$ is equal to one if the firm has a CSR committee in a given year. D_{Not} is equal to one for the remaining observations. The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) operate in different industries; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The additional controls are customer awareness and R&D investment. The coefficients are measured in standard deviation units. $P(C = N)$ is the p-value obtained from testing the hypothesis that peer effects are equal across firms with and without CSR board committees. The Sanderson-Windmeijer F-stat refers to the Sanderson and Windmeijer (2016) weak instrument F-test for models with multiple endogenous variables. t-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

Table 4.4. Which individuals mimic? The role of CSR board committees

(continued)

	Levels			First Differences		
	Includes All Social Peers (1)	Peers Without CSR Board Committees (2)	Peers With CSR Board Committees (3)	Includes All Social Peers (4)	Peers Without CSR Board Committees (5)	Peers With CSR Board Committees (6)
Peers' CSR × D_{Not}	0.523***	0.537***	-0.071	0.207**	0.304***	0.001
	(3.221)	(3.362)	(-0.520)	(2.025)	(2.674)	(0.009)
Peers' CSR × $D_{Committee}$	0.991***	1.015***	0.415***	0.751***	0.864***	0.600***
	(6.335)	(6.673)	(2.765)	(7.128)	(7.600)	(4.864)
Sanderson-Windmeijer F-Stat						
Ind. Peer's CSR × D_{Not}	69.400***	71.700***	78.830***	94.970***	74.520***	64.280***
Ind. Peer's CSR × $D_{Committee}$	91.950***	97.270***	112.010***	119.680***	95.720***	79.990***
First Stage Instrument						
Ind. Peer's CSR × D_{Not}	0.234***	0.233***	0.136***	0.268***	0.247***	0.137***
	(9.040)	(9.400)	(10.360)	(11.190)	(9.780)	(9.960)
Ind. Peer's CSR × $D_{Committee}$	0.740***	0.709***	0.634***	0.768***	0.736***	0.669***
	(31.240)	(29.230)	(19.880)	(39.200)	(38.160)	(18.710)
P(C = N)	0.000	0.000	0.000	0.000	0.000	0.000
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	25,664	25,553	19,300	22,833	22,730	17,174

In line with the hypothesis, the results in columns (1) and (4) indicate that firms with CSR committees mimic two to three times as much as firms without committees. I further document in columns (2) and (5) that firms with and without CSR committees both mimic social peers without committees. This suggests the result is not merely driven by firms with CSR committees tending to invest in a similar manner because they have similar committees. In contrast, social peers without CSR committees do not mimic firms without CSR committees (columns (3) and (6)).

Overall, this is consistent with the conjecture of Dougal, Parsons and Titman (2015) that small firms have relatively few opportunities to learn from their social peers because their executives and directors are not as well-connected as those of larger firms. In my sample firms with CSR committees have on average 106 peers while firms without such committees have on average 59 peers. Firms with CSR committees are also, on average, almost six times as large as firms without committees in terms of total assets. Therefore, smaller firms without CSR committees may optimally choose to mimic less because they do not have access to valuable information that larger well-connected firms with such committees do. Cross-firm differences in access to valuable information may thus partially explain the large cross-firm variation in CSR investment observed in practice. I test for this hypothesis in the next section.

4.5. Which firms mimic?

Next, I try to characterize the types of firms that mimic the most. Recent findings in the literature suggest that CSR is a form of value-enhancing product differentiation strategy that builds customer loyalty and reduces firm systematic risk (e.g., Servaes and Tamayo (2013), Flammer (2015b), Albuquerque, Koskinen and Zhang (2019)). If social peer effects arise due to firms exchanging information on CSR with the goal of creating firm value, peer effects should be stronger for firms with higher product differentiation. Following Albuquerque, Koskinen and Zhang (2019), I use the advertisement expenditures ratio as a proxy for product differentiation. As an alternative proxy, I also consider industry CSR intensity as a revealed preference measure of competition on CSR-based product differentiation. Although not without drawbacks, this measure alleviates the concern that some firms pursue a product differentiation strategy while spending little on advertising (e.g., Tesla).

To test this hypothesis I interact social peers' CSR scores with indicator variables that classify each firm-year as having either below median or above median values of product differentiation in a given year. The results from specifications in levels (first differences) are shown in columns (1) and (2) (columns (4) and (5)) of Table 4.5. Consistent with the hypothesis, the results show that social peer effects are concentrated in firms pursuing product differentiation strategies and firms operating in industries with high CSR intensity. The results thus suggest that industry and social peer effects reinforce each other. The more competition on CSR there is an industry, the more firms rely on information from social peers in other industries to obtain a competitive edge.

I further test whether or not larger firms mimic more than smaller firms. The motivation is twofold. First, boards of larger firms tend to put more weight on stakeholder interests (e.g., Adams (2005)). If the results are due to boards paying attention to stakeholder interests, social peer effects should be stronger for larger firms. Note that the fact that larger firms are more attentive to stakeholders interests does not mean that these firms are not maximizing firm-value. Large investments in CSR may simply be more value-enhancing for large firms than for small firms (e.g., Magill, Quinzii and Rochet (2015)). For example, small firms may mimic less because they lack customer awareness and financial

Table 4.5. Which firms mimic? The role of product differentiation and size

This table reports the results of two-stage least squares (2SLS) regressions of firm CSR scores on social peers' CSR scores. Social peers are defined based on the social networks of directors. The magnitude of peer effects is allowed to vary as a function of either industry CSR intensity, product differentiation or firm size. D_{High} is a binary indicator equal to one if one of these variables is larger than or equal to the median of the within-year distribution of that variable. D_{Low} is equal to one for the remaining observations. The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) operate in different industries; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The additional controls are customer awareness and R&D investment. The coefficients are measured in standard deviation units. *P(H = L)* is the p-value obtained from testing the hypothesis that peer effects are equal across firms in the *Low* and *High* groups. The Sanderson-Windmeijer F-stat refers to the Sanderson and Windmeijer (2016) weak instrument F-test for models with multiple endogenous variables. t-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

	Levels			First Differences		
	Ind. CSR Intensity	Product Different.	Firm Size	Ind. CSR Intensity	Product Different.	Firm Size
	(1)	(2)	(3)	(4)	(5)	(6)
Peers' CSR × D_{Low}	0.129	0.488***	0.296*	0.068	0.155	-0.013
	(0.802)	(3.141)	(1.953)	(0.736)	(1.473)	(-0.131)
Peers' CSR × D_{High}	0.769***	0.479***	0.581***	0.408**	0.223**	0.366***
	(3.125)	(3.284)	(4.186)	(2.241)	(2.355)	(4.145)
Sanderson-Windmeijer F-Stat						
Ind. Peer's CSR × D_{Low}	18.120***	76.940***	70.340***	86.870***	89.430***	86.530***
Ind. Peer's CSR × D_{High}	27.800***	80.860***	78.610***	35.340***	98.170***	93.820***
First Stage Instrument						
Ind. Peer's CSR × D_{Low}	0.189***	0.373***	0.349***	0.304***	0.383***	0.395***
	(4.590)	(14.420)	(11.820)	(9.060)	(15.880)	(16.170)
Ind. Peer's CSR × D_{High}	0.200***	0.519***	0.577***	0.206***	0.545***	0.561***
	(5.370)	(21.800)	(25.150)	(5.900)	(26.610)	(29.830)
P(H = L)	0.043	0.746	0.000	0.063	0.029	0.000
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	25,664	25,664	25,664	22,833	22,833	22,833

resources that justify an expensive CSR-based product differentiation strategy. Second, large firms are more likely to be better positioned in the corporate social network to obtain valuable information (e.g., Dougal, Parsons and Titman (2015)). I test this hypothesis by interacting social peers' CSR scores with indicator variables that define small (large) firms as those that have below (above) median total assets in a given year. The results in columns (5) and (6) support the expectation that social peer effects are indeed concentrated in larger firms.

The results thus far indicate that larger firms and firms with CSR committees mimic the most. It is unclear, however, whether this is the case simply because smaller firms have less to benefit from investing in CSR or also because smaller firms are not well-positioned in the network to acquire valuable information. To disentangle these two effects I construct a direct test of whether or not peer effects depend on information social capital, that is, the ability of firms to access valuable information through their social networks. I measure this concept with the decay centrality measure of Jackson (2008).

Decay centrality is designed to capture the ability of a node in a network to acquire and spread information in the network. Intuitively, the measure counts the number of firms that each firm can reach in the network within a given number of steps, weighting each count by a factor that decreases with the number of steps and increases with a parameter capturing information usefulness. This parameter captures the intuition that information becomes less useful as it is passed along the network because some noise is added to the information at each step. I report results using a parameter value of 0.5, that is, I assume information usefulness halves with each step. The results are almost identical if I assume that information usefulness does not deteriorate as it travels along the network. If social peer effects in CSR are driven by firms' ability to obtain valuable information, the peer effects estimates should be stronger for firms with high decay centrality, even after controlling for variables related to firm size.

I re-estimate model (4.1) allowing the peer effects coefficient to vary as a function of whether firms belong to the bottom, middle or top tercile of the distribution of decay centrality in a given year. In addition to the full set of controls included throughout the paper, I also control for the number of social peers to ensure decay centrality is not just capturing the fact that some firms have more social connections than others. I further control for the local clustering coefficient of Watts and Strogatz (1998) which measures how close-knit the local network around each firm is.[37] Firms in close-knit groups are likely able to effectively exchange information with other firms in the group because it is feasible to sustain within-group cooperation with threats of within-group punishment such as word-of-mouth sanctions (e.g., Lippert and Spagnolo (2011)). There is, however, a crucial distinction. While firms in close-knit groups are likely to share information, they are likely unable to obtain the most valuable information that circulates in the network as a whole. This happens because close-knit communities tend to be isolated from the rest of the network, limiting access to new and innovative ideas (e.g., Burt (2000), Burt (2004)). By controlling for the clustering coefficient, I reduce the likelihood that decay centrality captures access to non-valuable information.

I report the results in columns (1) and (4) of Table 4.6 using specifications in levels and first differences, respectively. I find that the effect sizes increase with decay centrality, suggesting that a firm's position in the social network is a strong determinant of whether a firm mimics or not. In columns (2) and

[37]The Watts and Strogatz (1998) local clustering coefficient varies between zero and one. A score of one (zero) occurs when all (none) of a firms' connections are connected to each other. In my sample 25% of the sample firms exhibit high clustering coefficients above 0.5. Refer to Section C.4 of the Appendix for an in-depth discussion and a histogram depicting the sample distribution of the local clustering coefficient.

Table 4.6. Which firms mimic? The role of information social capital

This table reports the results of two-stage least squares (2SLS) regressions of firm CSR scores on social peers' CSR scores. Social peers are defined based on the social networks of directors. The magnitude of peer effects is allowed to vary as a function of either decay centrality, diffusion centrality or clustering coefficient. D_{High}, D_{Med} and D_{Low} are binary indicators equal to one if the associated variable in a given firm-year belongs to the third, second and first tercile of the within-year distribution of that variable, respectively. The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) operate in different industries; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The additional controls are customer awareness and R&D investment. The coefficients are measured in standard deviation units. *P(H = L)* is the p-value obtained from testing the hypothesis that peer effects are equal across firms in the highest and lowest terciles. The Sanderson-Windmeijer F-stat refers to the Sanderson and Windmeijer (2016) weak instrument F-test for models with multiple endogenous variables. t-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

	Levels			First Differences		
	Decay Central. (2)	Diffusion Central. (1)	Cluster. Coeff. (3)	Decay Central. (5)	Diffusion Central. (4)	Cluster. Coeff. (6)
Peers' CSR × D_{Low}	0.279*	0.282*	0.616***	0.046	0.044	0.405***
	(1.823)	(1.860)	(4.861)	(0.452)	(0.435)	(4.626)
Peers' CSR × D_{Med}	0.327**	0.341***	0.399***	0.132	0.176**	0.154
	(2.456)	(2.647)	(2.876)	(1.511)	(2.003)	(1.579)
Peers' CSR × D_{High}	0.691***	0.718***	0.207	0.504***	0.535***	-0.082
	(5.624)	(6.227)	(1.421)	(6.375)	(6.919)	(-0.755)
Sanderson-Windmeijer F-Stat						
Ind. Peer's CSR × D_{Low}	61.910***	67.090***	86.160***	78.220***	77.490***	100.890***
Ind. Peer's CSR × D_{Med}	75.510***	83.020***	80.560***	103.210***	101.120***	93.610***
Ind. Peer's CSR × D_{High}	78.360***	83.020***	72.280***	103.510***	112.800***	85.010***
First Stage Instrument						
Ind. Peer's CSR × D_{Low}	0.315***	0.330***	0.596***	0.348***	0.342***	0.627***
	(11.190)	(12.210)	(24.280)	(12.960)	(12.970)	(33.430)
Ind. Peer's CSR × D_{Med}	0.610***	0.600***	0.575***	0.584***	0.584***	0.546***
	(36.910)	(36.720)	(28.840)	(41.320)	(42.570)	(33.870)
Ind. Peer's CSR × D_{High}	0.748***	0.728***	0.431***	0.755***	0.754***	0.434***
	(54.220)	(49.150)	(15.580)	(69.010)	(61.150)	(19.210)
P(H = L)	0.000	0.000	0.000	0.000	0.000	0.000
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	25,664	25,664	25,664	22,833	22,833	22,833

(5) I show that this result is robust to using the diffusion centrality measure of Banerjee et al. (2013). This measure generalizes the notion of decay centrality by taking into account that it is more likely that information travels between two nodes in the network if there are multiple paths connecting those nodes.

Next, I investigate whether or not firms with higher clustering coefficients also mimic more. Such a finding would open the possibility that some firms mimic based on non-valuable information. The peer effects estimates in columns (3) and (6) suggest this is not the case: only firms in the bottom tercile of the distribution of clustering coefficient mimic their peers.

Overall, the findings in this section suggest that firms use the social networks of their directors to acquire valuable information on CSR with the goal of obtaining a competitive edge over their industry rivals. Moreover, this effect is concentrated in large firms with high levels of information social capital, suggesting that the ability of firms to use social networks to obtain valuable information is very uneven across firms.

4.6. Why do firms mimic?

In this section I delve deeper into the question of why firms mimic the CSR policies of their social peers. I briefly explain the theoretical motivation underlying two economic channels related to social learning and social norms that may drive social peer effects and proceed by testing these channels.

4.6.1. Social learning channel

In theory, peer effects can arise if socially connected firms share information and learn from one another (e.g., Ellison and Fudenberg (1993, 1995), Banerjee and Fudenberg (2004), Acemoglu et al. (2011)). Firms have incentives to share information because it is typically challenging to determine whether or not a given CSR investment is worth pursuing. This is the case for several reasons. First, some of the benefits of CSR are intangible (e.g., reputation), are only realized over the long term or are state-dependent. For example, Amiraslani et al. (2017) show that CSR paid off in the form of improved ability to raise debt capital during the 2008-09 financial crisis but not during normal periods. Second, only recently did CSR become a widely used strategic tool to which substantial research and business efforts are dedicated, creating limits to learning how to optimally design CSR from academic literature and from co-workers' past experiences. Third, CSR encompasses a wide range of topics requiring different types of expertise that may not be readily available to all firms.

These factors create an environment of uncertainty for firms trying to estimate the NPV of alternative CSR projects. In addition, CSR investments are often irreversible and costly. To illustrate, for the average firm in their sample in 2006, Lins, Servaes and Tamayo (2017) estimate the cost of increasing CSR investment from the 1st to the 4th quartile of the cross-firm distribution of CSR to be $203.5 million. This may matter because in the presence of uncertainty about the net benefits of irreversible costly investments, firms often choose to postpone investments (e.g., Guiso and Parigi (1999)). Postponing investments, however, can lead to a situation of costly underinvestment if firms fall behind their industry rivals.

The exchange of information among social peers may allow firms to tackle this challenge in two ways. First, insofar as firms have the option of mimicking successful projects and avoiding value-destroying projects, information sharing spreads the downside risk of of choosing value-destroying sequences of trial-and-error CSR investments over time. This, in turn, incentivizes trial-and-error experimentation and can lead to faster learning across peers. Second, even if firms do not mimic specific projects, they may still learn from peers through the exchange of ideas about the intangible benefits of broad types of CSR (e.g., diversity versus health) and the optimal timing of these investments. If this exchange of ideas leads to similar opinions about the optimal design of CSR policies, peer effects will arise due to learning even if firms do not mimic specific projects.[38]

Thus, the social learning channel posits that social networks mitigate uncertainty and limits to learning by allowing firms to exchange information on how to optimally design value-creating CSR policies. Hence, under the social learning channel, the incentives for information sharing and learning stem solely from the desire to maximize firm value. If this is the case, peer effects should be stronger for firms with stronger ex-ante incentives to maximize firm value, that is, firms with better incentive alignment between shareholders and managers. If instead social peer effects are a manifestation of non-profit maximization motives (e.g., irrational herding or reputation concerns), we would expect either (i) peer effects to be strongest when incentive alignment is weakest, or (ii) no difference in peer effects across firms with different incentives.

I test the social learning channel by interacting the average CSR scores of the social peers with dummy variables indicating whether or not a given firm-year belongs to the bottom, middle or top tercile of the within-year distribution of a given incentive alignment proxy. I capture alignment of incentives with (i) CEO pay-related managerial incentives, (ii) the quality of board monitoring, (iii) institutional ownership, and (iv) industry competition.

[38] It is also possible that a firm learns from its peers about which types of CSR projects are not worth pursuing. This does not preclude peer effects. Firms with failed projects can exchange information about those projects for information about successful projects. The firms with failed projects benefit by learning how to create value with CSR investments and firms with successful projects learn how to avoid destroying value. For example, a firm *i* with a failed project can obtain information about a successful project S_j from social peer *j*, give that information to another peer *k* who is not itself socially connected to *j* in exchange for information about another successful project S_k and share the information on project S_k with firm *j*. This allows firms *i* and *j* (*i* and *k)* to invest in project S_k (S_j), resulting in contemporaneous peer effects between firm *i* and its peers.

First, I measure CEO pay-related managerial incentives with CEO delta and CEO vega. CEO delta is a measure of CEO pay-sensitivity to performance. Hence, if firms try to learn from their social peers with the goal of maximizing value, their incentives to do so are stronger the higher CEO delta is.[39] CEO vega is a measure of the convexity of managerial compensation. It captures incentives given to managers, via stock options, to avoid costly underinvestment by incentivizing risk-averse managers to invest in risky positive NPV projects (Guay (1999)). Since uncertainty and limits to learning can theoretically lead to costly underinvestment in CSR, social learning could be a valuable tool for high vega firms interested in allocating resources to risky CSR projects. Second, I measure the quality of board monitoring by the fraction of independent directors on the board because independent directors can, in principle, be a good governance force aligning the incentives of managers and shareholders (e.g., Adams, Hermalin and Weisbach (2010)). Third, I use institutional ownership as a measure of incentive alignment because sophisticated institutional investors are independent stakeholders with a comparative advantage over retail investors in monitoring (e.g., Ferreira and Matos (2008), Borochin and Yang (2017)). Fourth, I measure industry competition with the Herfindahl-Hirschman index. Firms in more competitive industries tend to have more aligned incentives because they face more pressure to reduce agency problems (e.g., Giroud and Mueller (2010), Kim and Lu (2011)).

The results are reported in Table 4.7. I report the results in first differences to eliminate all time-invariant omitted variables. This rules out concerns that, for example, firms with higher CEO delta are fundamentally different from

[39] It is conceivable that higher CEO delta is not always consistent with profit-maximization incentives. I alleviate this problem to some extent by testing whether or not firms in the top tercile of the distribution of delta mimic more than firms in the bottom tercile. The test should be valid as long as incentives are on average better aligned for firms in the top tercile of the distribution of delta relative to firms in the bottom tercile. It is thus not necessary to assume that incentive alignment increases monotonically with delta.

Table 4.7. Why do firms mimic? The social learning channel

This table reports the output of two-stage least squares (2SLS) regressions of changes in firm CSR scores on changes in social peers' CSR scores. Social peers are defined based on the social networks of directors. The magnitude of peer effects is allowed to vary as a function of either firm-level CEO delta, CEO vega, fraction of independent directors, institutional ownership or industry competition. D_{High}, D_{Med} and D_{Low} are binary indicators equal to one if the associated variable in a given firm-year belongs to the third, second and first tercile of the within-year distribution of that variable, respectively. The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) operate in different industries; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The additional controls are customer awareness, R&D investment and the following local network measures of social capital: organ donation density, voter turnout, registered organization density and association density. The coefficients are measured in standard deviation units. *P(H = L)* is the p-value obtained from testing the hypothesis that peer effects are equal across firms in the highest and lowest terciles. The Sanderson-Windmeijer F-stat refers to the Sanderson and Windmeijer (2016) weak instrument F-test for models with multiple endogenous variables. t-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

	CEO Delta	CEO Vega	Fraction Independent Directors	Institutional Ownership	Industry Competition
	(1)	(2)	(3)	(4)	(5)
Δ Peer's CSR × D_{Low}	0.060	0.076	0.136	0.188*	0.185*
	(0.494)	(0.641)	(0.725)	(1.897)	(1.815)
Δ Peer's CSR × D_{Med}	0.193	0.153	0.262	0.180*	0.240**
	(1.639)	(1.313)	(1.413)	(1.827)	(2.367)
Δ Peer's CSR × D_{High}	0.345***	0.445***	0.452***	0.196*	0.215**
	(3.031)	(4.124)	(2.600)	(1.932)	(2.065)
P(H = L)	0.000	0.000	0.000	0.810	0.472
Sanderson-Windmeijer F-Stat					
Δ Ind. Peer's CSR × D_{Low}	65.410***	64.000***	43.510***	98.970***	101.730***
Δ Ind. Peer's CSR × D_{Med}	67.490***	69.580***	47.470***	102.540***	97.210***
Δ Ind. Peer's CSR × D_{High}	67.120***	71.500***	45.340***	97.330***	97.060***
First Stage Instrument					
Δ Ind. Peer's CSR × D_{Low}	0.549***	0.575***	0.559***	0.569***	0.549***
	(21.280)	(22.810)	(18.280)	(28.480)	(27.250)
Δ Ind. Peer's CSR × D_{Med}	0.557***	0.527***	0.502***	0.518***	0.552***
	(25.500)	(20.680)	(20.710)	(27.840)	(26.660)
Δ Ind. Peer's CSR × D_{High}	0.603***	0.619***	0.584***	0.537***	0.482***
	(24.610)	(27.580)	(24.110)	(27.870)	(22.350)
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes
No. Obs.	15,151	15,151	12,185	22,798	22,448

firms with low CEO delta along unobservable dimensions. I further control for all four measures of network specific geographic social capital described in Section 4.1.3. This alleviates the concern that the identification of the social learning channel is confounded by the social norms channel discussed in the next subsection. I also control for the incentive alignment variables at the peer level. This makes it less likely that the results are driven by a network effect in corporate governance practices across firms (e.g., Bouwman (2011)). For example, it could be the case that better incentive alignment leads firms to invest more in CSR and that firms with well-aligned incentives tend to be socially connected with firms that also have well-aligned incentives. It is also worth noting that there is a reduced number of observations in the regressions involving delta, vega and fraction of independent directors. This happens because these variables are sourced from ExecuComp and ISS which only cover S&P 1500 firms.

The results show that (i) the economic significance of peer effects increases monotonically with firm value maximization incentives in all cases with the exception of the specifications in which incentive alignment is proxied for by institutional ownership or industry concentration, (ii) the bulk of the peer effects is concentrated in firms in the top terciles of delta, vega, fraction of independent directors and, to a lesser extent, industry competition, and (iii) the magnitude of peer effects is high and precisely estimated for firms with high vega, consistent with social learning alleviating underinvestment problems by reducing investment frictions such as uncertainty and limits to learning.[40]

[40]Another explanation is that CEO vega is capturing excessive risk-taking and short-term incentives. This is very unlikely. First, if this was the case, we would not expect peer effects to be stronger when board monitoring and CEO delta are higher. More monitoring should curb excessive risk-taking and higher delta, by itself, incentivizes underinvestment in risky projects because CEO wealth is not diversified (e.g Coles, Daniel and Naveen (2006)). Second, excessive risk-taking in CSR strongly increases the probability of CEO dismissal when performance is poor (Hubbard, Christensen and Graffin (2017)). Hence, from an ex-ante perspective, there is a large downside risk to gambling. Third, contracts with high vega tend to be structured, albeit imperfectly, in a way that curbs excessive risk-taking incentives (e.g., Kubick, Robinson and Starks (2018)). Fourth, it

Overall, these results provide evidence that firms mimic their social peers only when managers have strong incentives to maximize firm value. This is consistent with the idea that social networks are a value-creating resource that firms can strategically use to overcome uncertainty and costly underinvestment. As a consequence, these findings also cast doubt on explanations based on non-profit maximization motives such as irrational herding.

Furthermore, these results provide a mechanism that can at least partially explain the negative association between CSR investment and agency frictions documented by Ferrell, Liang and Renneboog (2016). In detail, my results suggest that when agency frictions are low, firms are able to use their social networks to obtain information that decreases uncertainty about CSR investments. This, in turn, decreases the real option value of waiting for more information before investing and leads to more CSR investment.

It is also interesting to note that the finding that social peer effects are not stronger when institutional ownership is higher is consistent with the idea that institutional investors can influence CSR directly (e.g., Dyck et al. (2019)), thus reducing the need for board intervention. Iliev and Roth (2020) also provide evidence for this substitution effect by showing that sustainability regulations in foreign countries are less likely to spill over to US firms via international board interlocks when institutional ownership concentration is higher. In Appendix Table C.12 I show that social peer effects in CSR are also stronger when ownership concentration is lower.

4.6.2. Social norms channel

Given the evidence that social norms and values affect CSR decisions (e.g., Giuli and Kostovetsky (2014), Cronqvist and Yu (2017)), it is also possible that

is more likely that short-term oriented CEOs prefer to cut on CSR spending to meet performance targets rather than investing more in CSR.

the social transmission of CSR occurs via an identity economics channel (e.g., Akerlof and Kranton (2000), Bénabou and Tirole (2011a)).

In the identity economics framework, a director jointly maximizes a standard utility function related to how much CSR increases profit as well as an identity utility function related to his ideals about whether or not firms have a societal role beyond firm value maximization. Social peers' identities can influence CSR choices in two ways. First, directors may internalize their social peers' pro-CSR ideals through mechanisms of persuasion (DeMarzo, Vayanos and Zwiebel, 2003) and peer esteem (Akerlof (2016, 2017)). If so, identity utility is highest if CSR choices match the ideal norms of behavior that are associated with the peers' identities and, therefore, their CSR choices. Second, failure to conform with social peers' behavior may lead to punishment. For example, by shunning employee diversity policies or taking a soft stand on the importance of protecting the environment, a director may negatively affect how pro-CSR peers perceive her and risks punishment in the form of foregone social benefits, such as peer esteem and future board appointments (e.g., Levit and Malenko (2016)). If the punishment is strong enough, the desire to conform will lead to peer effects in CSR. This is especially so because social networks are known to be important sources of job opportunities and financial returns to executives and directors (e.g., Hwang and Kim (2009), Fracassi and Tate (2012), Engelberg, Gao and Parsons (2013), Ishii and Xuan (2014)).

If social peer effects in CSR are driven by identity and social norms, peer effects should be stronger for firms whose executives and directors have social networks that are rich in the following two dimensions of social capital: (i) social capital as a measure of civic engagement and pro-social preferences, capturing the likelihood that peers believe they have a role beyond value maximization; (ii) social capital as a measure of the extent to which networks can enforce punishment threats that sustain cooperation and pro-social behavior.

To quantify social capital at the network level, I exploit the fact there is ample cross-sectional variation in geographic social capital at the county-level in the US and that there is evidence that firms absorb the social norms of the county where the firm is headquartered.[41] Geographic social capital is likely to capture the desired dimensions of firm-level social capital because high social capital counties are, by definition, communities in which trust, reciprocity and pro-social behavior are sustained through internalized community values and networks of relationships. In such communities, individuals act with the well-being of the community in mind and expect others to do the same. This expectation is self-fulfilling because shared norms of behavior are rewarded by the community and deviant behavior is punished.[42]

Following an extensive literature (e.g., Putnam (2000), Guiso, Sapienza and Zingales (2004), Rupasingha, Goetz and Freshwater (2006), Lin and Pursiainen (2018)), I measure geographic social capital with county-level data on organ donation per capita, association density per capita, registered organization density per capita, voter turnout and the first principal component of the last three variables. I then assign county-level data to each firm based on the location of firm headquarters. I do not account for headquarters relocations because Compustat only provides data on the most recent headquarters. This is, how-

[41] For instance, Hasan et al. (2017a) find that firms engage in less tax evasion in US counties with more social capital. Hasan et al. (2017b) find that firms headquartered in low social capital counties have access to cheaper debt. Jha and Chen (2014) find evidence that audit firms infer trustworthiness of clients based on whether firms are headquartered in a low or high social capital county.

[42] Furthermore, insofar as individuals internalize community values and norms and derive satisfaction (e.g., self-esteem) from behaving according to those values and norms, individuals can punish themselves for deviating from the norm. Such punishments can include feeling guilty, shame, lack of self-esteem and discomfort arising from cognitive dissonance. Ultimately, as pointed out by Bénabou and Tirole (2011b), the standards of communities regarding the enforcement of punishments and rewards for pro-social behavior will affect not only dynamics of stigma and esteem but also moral sentiments of shame and pride.

ever, unlikely to affect the results because relocations are very rare (e.g., Parsons, Sulaeman and Titman (2018)). To create a measure of the geographic social capital embedded in the local network of each firm, I average the social capital variables across each firm's social peers, including the firm itself.

Table 4.8 reports the findings of whether or not peer effects are stronger for firms whose local social networks are richer in social capital. I split firms in terciles based on the distribution of social capital in each year. Each column (1) through (5) shows the results using one of the social capital variables. Across most specifications, the peer effects in the lowest and highest tercile are similar and, in all five cases, peer effects are the strongest for firms in the middle tercile. This indicates that peer effects do not increase monotonically with social capital. I confirm this by formally testing for equality of peer effects in the highest and lowest tercile of social capital. With the exception of the regression using registered organization density as a proxy for social capital (p-value of 0.051), I always fail to reject the null of equality at the 10% level.

One possible caveat of these results is that the local network measure of geographic social capital is not the relevant measure. It could be the case that a firm's own social capital, as opposed to local network social capital, fully determines the extent to which a firm mimics its peers. This could arise if own social capital leads some firms to always want to invest in a level of CSR that is deemed adequate by its social peers, irrespective of peers' social capital. As shown in Appendix Table C.13, this turns out not to be the case. All in all, there is no evidence that social peer effects are driven by social norms.

4.7. Conclusion

This study provides evidence that CSR policies are transmitted across firms through their directors' social networks. Based on rich social network data

Table 4.8. Alternative explanations: the social norms channel

This table reports the output of two-stage least squares (2SLS) regressions of changes in firm CSR scores on changes in social peers' CSR scores. Social peers are defined based on the social networks of directors. The magnitude of peer effects is allowed to vary as a function of each firm's local network geographic social capital. Local network geographic social capital is measured as the peer group average, including the firm itself, of one of the following variables: organ donation density, voter turnout, registered organization density, association density or the first principal component of the previous three variables. D_{High}, D_{Med} and D_{Low} are binary indicators equal to one if the associated variable in a given firm-year belongs to the third, second and first tercile of the within-year distribution of that variable, respectively. The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) operate in different industries; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The additional controls are customer awareness and R&D investment. The coefficients are measured in standard deviation units. *P(H = L)* is the p-value obtained from testing the hypothesis that peer effects are equal across firms in the highest and lowest terciles. The Sanderson-Windmeijer F-stat refers to the Sanderson and Windmeijer (2016) weak instrument F-test for models with multiple endogenous variables. t-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

	Local Network Geographic Social Capital				
	Organ Donation (1)	Voter Turnout (2)	Registered Org. Density (3)	Association Density (4)	Principal Component (5)
Δ Peer's CSR × D_{Low}	0.084	0.180*	0.113	0.121	0.112
	(0.818)	(1.668)	(1.049)	(1.132)	(1.012)
Δ Peer's CSR × D_{Med}	0.316***	0.300***	0.295***	0.302***	0.311***
	(3.266)	(3.155)	(3.212)	(3.112)	(3.244)
Δ Peer's CSR × D_{High}	0.191*	0.120	0.243***	0.160	0.123
	(1.682)	(1.107)	(2.667)	(1.422)	(1.131)
P(H = L)	0.165	0.423	0.051	0.613	0.882
Sanderson-Windmeijer F-Stat					
Δ Ind. Peer's CSR × D_{Low}	101.990***	113.260***	91.520***	95.970***	93.140***
Δ Ind. Peer's CSR × D_{Med}	114.100***	109.310***	127.680***	111.260***	111.230***
Δ Ind. Peer's CSR × D_{High}	96.170***	96.550***	129.820***	112.170***	107.680***
First Stage Instrument					
Δ Ind. Peer's CSR × D_{Low}	0.471***	0.473***	0.432***	0.431***	0.418***
	(18.800)	(18.540)	(17.120)	(15.470)	(14.820)
Δ Ind. Peer's CSR × D_{Med}	0.558***	0.579***	0.553***	0.561***	0.545***
	(29.420)	(26.800)	(31.050)	(23.960)	(25.940)
Δ Ind. Peer's CSR × D_{High}	0.426***	0.447***	0.554***	0.432***	0.465***
	(14.750)	(17.220)	(22.510)	(16.710)	(18.800)
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes
No. Obs.	22,653	22,653	22,653	22,653	22,653

for 83,604 top executives and directors of Russell 3000 firms, my estimates indicate that firms with average levels of CSR increase their CSR by 16% in response to a one standard deviation increase in the average CSR scores of their social peers. Overall, the economic magnitude of social peer effects is comparable to the industry peer effects of CSR documented by Cao, Liang and Zhan (2019).

Social network spillovers seem to be driven by firms exchanging information with their social peers in different industries with the goal of creating firm value and obtaining a competitive edge over their industry rivals. The effect is uneven across firms, with most of the mimicking being done by (i) firms pursuing product differentiation strategies for which CSR is more likely to add value, (ii) firms that are strategically positioned in the social network to obtain valuable information, and (iii) firms in which the profit maximization incentives of managers and shareholders are better aligned.

Overall, the results reveal a bright side of corporate social networks for both firms and society at large. For firms, the results suggest that social networks may allow for the design of better CSR policies in a manner that is consistent with aligned profit maximization incentives. For society, the existence of peer effects implies the existence of a social multiplier in CSR investing, thus amplifying the positive externalities of CSR on society.[43]

There may also be a dark side, however. The fact that social peer effects seem to be driven by social learning is consistent with the existence of frictions in CSR investment, such as uncertainty and limits to learning. Otherwise, social learning would not be necessary in the first place. If so, many firms may be underinvesting in CSR due to their inability to overcome investment frictions.

[43]The existence of a social multiplier is implied by the existence of peer effects. See Glaeser, Sacerdote and Scheinkman (2003) for a proof. Intuitively, any shock that increases the CSR of a firm i will lead to an increase in CSR of social peers via peer effects. The increase in CSR of social peers will, in turn, lead to increases in CSR investment by their own social peers.

Therefore, the large cross-firm variation in CSR investment that exists nowadays may partly be an outcome of cross-firm differences in exposure to investment frictions and differences in ability to mitigate those frictions.

Chapter 5

Summary and concluding remarks

The first essay shows that commodity futures returns aggregate dispersed information about future macroeconomic fundamentals and country-level stock returns in many countries around the world. Remarkably, we also find that countries' dependence on commodity trade is not the primary explanation for this phenomenon. This finding holds even if we account for indirect exposures to commodity trade dependence that arise due to financial and trade integration across countries.

This suggests that the information discovery role played by commodity markets is phenomenally complex and truly global in nature. We find further support for this interpretation in our finding that the information that commodity markets have about future stock returns around the world is fairly split both across commodity sectors and across countries. In other words, the information flows from commodity to stock markets are not restricted to the energy sector and a narrow set of countries with special characteristics.

Our results are thus consistent with one of the key ingredients of the influential model of Sockin and Xiong (2015): the idea that commodity futures prices have information about the state of the global economy. As such, our findings give some plausibility to the theoretical possibility studied by Sockin and Xiong (2015) that commodity futures trading may generate (potentially distorted) price signals that affect the production decisions of firms around the globe as well as commodity spot prices.

Our hope is that this essay motivates further research on the interaction between commodity futures trading and the real economy. Only then can we truly understand whether we should celebrate or try to curb the speculation surge in commodity futures markets that we have experienced since the early 2000s (e.g., Cheng and Xiong (2014)).

The second essay provides evidence that sustainable investing based on ESG ratings is unlikely to have systematically improved or hurt investment performance during the last two decades. This finding is strikingly robust - it holds across most world regions, in different time periods, in different sectors of economic activity, when using different ESG ratings from three major raters, when using combinations of these ratings, and whether we use ESG ratings in levels or changes (ESG momentum).

While we acknowledge that the performance of sustainable investing might be different going forward, we believe that our findings provide interesting insights. On the bright side, our results indicate that sustainable stocks are unlikely to be very overvalued at this stage. This alleviates the pervasive concern raised by several policymakers and investors that we are living through an ESG "bubble". Relatedly, our findings also indicate that it might be possible to engage in sustainable investing without forgoing returns. This is a most positive finding as it suggests that it might be possible to satisfy the non-pecuniary utility of investors without sacrificing their pensions and material well-being.

These results, however, also have a darker interpretation. Exactly because the relation between ESG ratings and stock returns is flat, the results allow for the possibility that sustainable investing cannot be trusted to always decrease the cost of equity of sustainable firms. As such, our results provide some empirical support to the view that sustainable investing is not a reliable solution to create a more sustainable world.

We recognize that theoretical models (e.g., Pastor, Stambaugh and Taylor (2020)) predict that sustainable firms may even outperform their unsustainable counterparts over short periods of time due to unexpected news that increase the realized returns of sustainable firms - a fact that need not imply that sustainable firms benefit from a lower cost of equity. Our findings neither discredit these models nor imply that sustainable investing will not be an important driver of positive change in the future.

Rather, our results highlight that there is a possibility that many sustainable firms may not be compensated with lower costs of equity for extended periods of time - as long as two decades. This is worrying because many would argue that two decades is hardly short-term in the fight against societal problems such as climate change. For example, climate experts often give 2030 and 2050 as deadlines for courageous reductions in carbon emissions. It is therefore uncertain whether or not sustainable investing will provide strong enough incentives for firms to reach desirable sustainability targets in a timely manner. Hence, our results suggest that it might be risky to bet on sustainable investing to substitute for government policy in bringing about a more sustainable word.

In the third essay I identify a new channel through which boards of directors shape CSR practices in firms: social learning through social networks. In particular, I provide evidence that the various educational, leisure, and employment links that are shared by firms' directors create a social network that functions as a market for information exchange about CSR.

Notably, these social network effects in CSR are concentrated in firms that can benefit from learning about CSR policy, firms that have a good position in the social network that enables them to access valuable information, and firms in which the incentives of managers and shareholders are aligned. My results thus suggest that the traditional concept of good governance that underpins shareholder capitalism is, at least to some extent, consistent with CSR.

A compelling feature of this essay is that it identifies a narrow channel through which directors shape CSR policies in the corporate world. This sheds some light on how a specific corporate governance mechanism - the board of directors - influences CSR. In doing so, it also contributes to unraveling the blackbox of how CSR decisions are made in practice. These two issues are key to fully understanding the relation between corporate governance and CSR. As such, the findings in this essay may be useful inputs in the debate about what the role of firms in society should be.

Indeed, these questions have been the target of great public debate. A particularly polemic case is the *Sustainable corporate governance* initiative by the European Commission, which is motivated by a study by Ernst & Young entitled “Study on directors’ duties and sustainable corporate governance”. One of the aims of this initiative is to make directors pay more attention to stakeholders’ interests. The problem, according to the study, is that directors ignore the interests of stakeholders because their incentive are overly aligned with the short-term profit maximization incentives of shareholders.

My results suggest that there may be more nuance to this idea than meets the eye. At least when it comes to social network effects in CSR, the firms with better aligned incentives are precisely the ones that make efforts to learn about

CSR. It is thus not the case that incentive alignment and traditional corporate governance necessarily lead directors to ignore the interests of stakeholders.[44]

What does this imply for governance reforms such as the European Commission's initiative? My results do not imply that the levels of social responsibility that firms voluntarily choose are optimal from the perspective of society as a whole. My results also do not suggest that there is no room for government policy to address the negative environmental and societal externalities that firms create. In fact, my results suggest that spreading information about the benefits of CSR might lead some firms to invest more in CSR in a way that is consistent with good governance. The CSR Programme of the United Nations Industrial Development Organization (UNIDO) works with governments and firms around the world to do exactly this. My results, however, provide evidence that good corporate governance can, to some extent, coexist with CSR. This, by itself, is arguably desirable. As such, proposals to change the corporate governance status quo may benefit from carefully addressing the possibility that there may be an economically relevant opportunity cost of doing such changes.

[44] I acknowledge that the external validity of my results is limited to the scope of social network effects in CSR. The study of Ferrell, Liang and Renneboog (2016), however, also finds that corporate governance metrics are positively associated with CSR in a more general setting. My study complements theirs by identifying a narrow channel through which the positive association between corporate governance and CSR arises. This is interesting for two reasons. First, the study of this narrow channel allows me to use a variety of tests and identification strategies that provide further credibility to the idea that CSR is, to some extent, consistent with good corporate governance. Second, there are very good reasons for why social network effects in CSR could be a manifestation of agency problems. To the extent that this is the case, my results provide particularly strong evidence for the good governance view of CSR.

Appendices

Appendix A

The Information Content of Commodity Futures Markets

In this Appendix we present a detailed description of the procedure we used to match our commodities with the UNCTADstat data on export and import dependence, as well as the results of a variety of robustness checks.

A.1. Matching commodities to UNCTADstat data

We identify all the SICT codes in UNCTADstat that match the individual commodities included in each of our six sectors. We match each of the 28 individual commodities to SICT codes up to the fifth digit based on commodity names. For each matched five-digit code, we retrieve the associated three-digit code to conduct a further matching round; this is the highest level of granularity in the data that is available for download. There are cases in which a perfect match at the five-digit level coexists with a poor match at the three-digit level. For instance, we were able to perfectly match the commodity *wheat* to three-digit codes *041 - wheat, including spelt, and meslin , unmilled* - and *046 - meal and* *flour of wheat and flour of meslin* - as all the subcategories of *041* and *046* explicitly refer to the word *wheat*. However, while we were also able to textually match *wheat* to category *081.26 - bran, sharp and other residues, whether or not in the form of pellets, derived from sifting, milling or other working of cereals or of leguminous plants of wheat* -, we were not able to match it to any other of the many five-digit categories within three-digit category *081*. Therefore, *wheat* was not matched to *081* SICT code but it was matched to *041 and 046*. It may be the case that while most of the five-digit subcategories within *081* cannot be matched to one specific commodity, it might still be possible to match most of them to one of our sectors as a whole (Agriculture I or Agriculture II, in this case). In this case, we still assign the corresponding three-digit category to a sector. For instance, we could not match *289.19 - ores and concentrates of other precious metals* - to any specific commodity, but we could match it broadly to the Precious Metals sectors. Since it turns out that we were able to match every subcategory within *289 - ores and concentrates of precious metals; waste, scrap and sweepings of precious metals (other than gold)* - to Precious Metals, we matched *289* to the sector Precious Metals. As a general rule aimed at minimizing noise, we require that a matching at the three-digit level is only appropriate if the ratio of the number of matches to all possible matches is at least 1:2 at the five-digit level. In practice, the ratio is either much higher or much lower than 1:2 and the decision to match or not match is unambiguous.

A.2. Derivation of the infinite-horizon adjustment

Without loss of generality assume T is an arbitrarily large integer and $|\gamma_i| < 1$. Then, we can re-write (2.6) as:

$$
\begin{aligned}
r_{i,T:T+19} &= \beta_{i,0} + \sum_{s}(\beta_{i,s,1} r^{c}_{s,T-21:T-2} + \beta_{i,s,2} r^{c}_{s,T-41:T-22}) + \phi_i r_{i,T-20:T-1} + \epsilon_{i,T:T+19} \\
&= \sum_{k=0}^{T-41} \phi_i^{T-41-k}(\beta_{i,0} + \sum_{s}(\beta_{i,s,1} r^{c}_{s,k+20:k+39} + \beta_{i,s,2} r^{c}_{s,k:k+19}) \\
&+ \epsilon_{i,k+41:k+60}) + \phi_i^{T-40} r_{i,21:40}
\end{aligned}
$$

The commodity sector-specific infinite-horizon marginal effect is defined as the sum of the absolute values of marginal effects over time, discounted by an appropriate power of ϕ_i. Mathematically, the infinite-horizon marginal effect for the commodity sector *s* is computed as:

$$
\begin{aligned}
&\lim_{T\to\infty}\left(\sum_{k=0}^{T-41}\left|\frac{\partial r_{i,T+19:T}}{\partial r^{c}_{s,k+20:k+39}}\right| + \left|\frac{\partial r_{i,T+19:T}}{\partial r^{c}_{s,k:k+19}}\right|\right) \\
&= \lim_{T\to\infty}\sum_{k=0}^{T-41} |\phi_i^{T-41-k}(\beta_{i,s,1} + \beta_{i,s,2})| \\
&= \sum_{h=1}^{2} \theta_i |\beta_{i,s,h}|
\end{aligned}
$$

where $\theta_i = \frac{1}{1-|\phi_i|}$

A.3. Appendix tables

Table A.1. Individual commodities summary information

This table summarizes the list of commodities, the sector to which each commodity belongs, the exchange on which the commodities are traded, the delivery months, and starting dates for each commodity's time series of commodity futures returns.

Commodity	Symbol	Exchange	Delivery Months	First Observation
Energy				
Heating oil	HO	NYMEX	All	1979/11/01
Crude oil	CL	NYMEX	All	1983/04/04
Gasoline	HU/RB	NYMEX	All	2005/11/01
Natural gas	NG	NYMEX	All	1990/05/01
Gas-Oil-Petroleum	LF	ICE	All	2000/12/29
Propane	PN	NYMEX	All	2000/12/29
Industrials				
Copper	HG	NYMEX	1,3,5,7,9,12	1979/11/01
Agriculture I				
Corn	C-	CBOT	3,5,7,9,12	1979/11/01
Oats	O-	CBOT	3,5,7,9,12	1979/11/01
Wheat	W-	CBOT	3,5,7,9,12	1979/11/01
Soybean Oil	BO	CBOT	1,3,5,7,8,9,10,12	1979/11/01
Soybean Meal	SM	CBOT	1,3,5,7,8,9,10,12	1979/11/01
Soybeans	S-	CBOT	1,3,5,7,8,9,11	1979/11/01
Rough Rice	RR	CBOT	1,3,5,7,9,11	1986/08/21
Agriculture II				
Cotton	CT	ICE	3,5,7,10,12	1979/11/01
Lumber	LB	CME	1,3,5,7,9,11	1979/11/01
Coffee	KC	ICE	3,5,7,9,12	1979/11/01
Sugar	SB	ICE	3,5,7,10	1979/11/01
Cocoa	CC	ICE	3,5,7,9,12	2000/12/29
Milk	DE	CME	2,4,6,8,9,12	2000/12/29
Livestock				
Feeder Cattle	FC	CME	1,3,4,5,8,9,10,11	1979/11/01
Live Cattle	LC	CME	2,4,6,8,10,12	1979/11/01
Lean Hogs	LH	CME	2,4,6,7,8,10,12	1979/11/01
Pork Bellies	PB	CME	2,3,5,7,8	1979/11/01
Precious Metals				
Gold	GC	NYMEX	2,4,6,8,10,12	1979/11/01
Silver	SI	NYMEX	3,5,7,9,12	1979/11/01
Palladium	PA	NYMEX	3,6,9,12	1979/11/01
Platinum	PL	NYMEX	1,4,7,10	1979/11/01

Table A.2. Proxies for the risk-free rate

This table presents a summary of the different proxies used to approximate the risk-free rate in each country in our sample. Developed countries are listed in bold. *GFD* stands for Global Financial Data, which is where all the data on risk-free rates was sourced.

	Total Return Indices - Bills	
Austria	Korea	**Singapore**
Bulgaria	Lithuania	Slovenia
Chile	Malaysia	**Spain**
China	Mexico	Turkey
Finland	**Norway**	UAE
Hong Kong	**Portugal**	**USA**
Ireland	Romania	Vietnam
Jordan*	Russia	
	Treasury Bill Yields	
Australia	Greece	Nigeria
Bahrain	Hungary	Pakistan
Bangladesh	India	Philippines
Belgium	**Israel**	Poland
Brazil	**Italy**	Serbia
Canada	**Japan**	South Africa
Croatia	Kazakhstan	Sri Lanka
Czech Republic	Kenya	**Sweden**
Denmark	Lebanon	**Switzerland**
Egypt	Mauritius	Taiwan
France	**Netherlands**	Tunisia
Germany	**New Zealand**	**UK**
	GFD Indices - Bonds	
Argentina	Colombia	Indonesia
	Deposit Rates	
Estonia	Peru	
	Interbank Interest Rates	
Kuwait	Ukraine	Qatar
	Overnight Interest Rates	
Jordan*	Oman	
	Central Bank Interest Rates	
Morocco		

* For Jordan we use the overnight interest rate from November 1996

Table A.3. Starting dates for country-specific variables

This table shows the starting dates for all country-specific variables: gross returns, risk-free rates, inflation rates and real industrial production growth. Developed countries are listed in bold. A blank entry indicates that we did not observe data for that country and variable in our sample. Inflation is not available for three countries and industrial production for eight countries.

Country Name	Gross Returns	Risk Free Rates	Inflation Rates	Industrial Production	Country Name	Gross Returns	Risk Free Rates	Inflation Rates	Industrial Production
	Emerging economies					*Developed economies*			
Brazil	199501	199501	199501	200002	**USA**	197911	197911	197911	199102
India	199302	199302	197911	199102	**Japan**	197911	197911	197911	199102
China	199301	199301	198601	199702	**UK**	197911	197911	197911	199102
Russia	199501	199501	199202	199202	**Germany**	197911	197911	197911	199102
Korea	198801	198801	197911	199102	**France**	197911	197911	197911	199102
Mexico	198801	198801	197911	199102	**Australia**	197911	197911		
Chile	198801	198801	197911	199102	**Italy**	197911	197911	197911	199102
Indonesia	198801	198801	197911	199102	**Canada**	197911	197911	197911	199102
Malaysia	198801	198801	197911	199102	**Spain**	197911	197911	197911	199102
Poland	199301	199301	198301	199102	**Switzerland**	198002	198002	197911	199102
Taiwan	198801	198801	197911	199102	**Hong Kong**	197911	197911	197911	199102
South Africa	199301	199301	197911	199102	**Norway**	197911	197911	197911	199102
Philippines	198801	198801	197911	199102	**Singapore**	197911	197911	197911	199102
Thailand	198801	198801	197911	199102	**Sweden**	197911	197911	197911	199702
Turkey	198801	198801	197911	199102	**Austria**	197911	197911	197911	199102
Argentina	198801	198801	197911	199310	**Denmark**	197911	197911	197911	199702
Colombia	199301	199301	197911	199102	**Finland**	198612	198612	198701	199102
Chech Republic	199501	199501	197911	199302	**Netherlands**	197911	197911	197911	199102
Egypt	199501	199501	197911	200403	**Belgium**	197911	197911	197911	199102
Qatar	200506	200506	200301	200301	**Ireland**	198801	198801	199701	199702
Hungary	199501	199501	197911	199102	**New Zealand**	198201	198201		
UAE	200506	200506	200702	200702	**Portugal**	198801	198801	197911	199102
Morocco	199501	199501	197911	199102	**Israel**	199301	199301	197911	199102
Pakistan	199301	199301	197911	199102					
Srilanka	199301	199301	197911	201002					
Greece	198801	198801	197911	199302					
Bulgaria	200506	200506							
Estonia	200206	200206	199008	199802					
Croatia	200206	200206	197911						
Lebanon	200206	200206	200801						
Romania	200512	200512	199011	199102					
Slovenia	200206	200206	197911	199202					
Vietnam	200612	200612	199502	199502					
Ukraine	200606	200606	199202	200202					
Bahrain	200506	200506	197911						
Bangladesh	200912	200912	197911	199102					
Jordan	198801	198801	197911	199102					
Kazakhstan	200512	200512	199202	199902					
Kenya	200206	200206	197911						
Kuwait	200506	200506	197911	199108					
Lithuania	200806	200806	199206	199601					
Mauritius	200206	200206	197911						
Nigeria	200206	200206	197911	199102					
Oman	200506	200506	199605	199606					
Peru	199301	199301	197911	199102					
Serbia	200806	200806	197911	199102					
Tunisia	200406	200406	197911	199102					

Table A.4. Robustness tests for the predictive regressions

This table summarizes the results of the following regression with overlapping observations for each country *i*:

$$r_{i,t:t+19} = \beta_{i,0} + \sum_s (\beta_{i,s,1} r^c_{s,t-21:t-2} + \beta_{i,s,2} r^c_{s,t-41:t-22}) + \phi_i r_{i,t-20:t-1} + \varepsilon_{i,t:t+19}$$

It shows the number of countries for which monthly stock market excess returns ($r_{i,t:t+19}$) are predicted by sector-*s* one-month ($r^c_{s,t-21:t-2}$) and two-month ($r^c_{s,t-41:t-22}$) lagged commodity futures returns at the 10% significance level (based on Newey-West standard errors). T and *%* stand for total number and percentage of countries (out of 70). In Panel A, we run the predictive regressions using monthly non-overlapping stock market excess returns. In Panel B, we use gross returns instead of excess returns. In Panel C, we estimate the baseline regression using post-1995 data only. The table also shows descriptive statistics for the regression R^2 and counts of significant coefficients for at least one sector and for at least one lag. *p(Wald) < 10%* denotes the number of countries for which we reject the null hypothesis that all commodity sectors fail to predict stock market excess returns. The full sample refers to the period from November 1979 to March 2016.

		Energy	Industrial Metals	Agriculture I	Agriculture II	Livestock & Meats	Precious Metals	EWI	GSI	At least one sector
Panel A: Full sample, non-overlapping returns										
$r^c_{s,t-21:t-2}$	T	18	10	13	3	6	9	7	10	41
	%	26%	14%	19%	4%	9%	13%	10%	14%	59%
$r^c_{s,t-41:t-22}$	T	4	17	5	23	7	19	15	5	49
	%	6%	24%	7%	33%	10%	27%	21%	7%	70%
	T	21	24	18	26	12	25	18	13	59
At least one horizon	%	30%	34%	26%	37%	17%	36%	26%	19%	84%
R^2	mean	6.07%	median	3.87%	max	37.52%	min	0.0076		
p(Wald) < 10%	T	41	59%							

Table A.4. Robustness tests for the predictive regressions

(continued)

		Energy	Industrial Metals	Agriculture I	Agriculture II	Livestock & Meats	Precious Metals	EWI	GSI	At least one sector
Panel B: Full sample, gross returns										
$r^c_{s,t-21:t-2}$	T	24	13	5	12	17	8	6	14	50
	%	34%	19%	7%	17%	24%	11%	9%	20%	71%
$r^c_{s,t-41:t-22}$	T	9	16	12	11	9	9	13	17	44
	%	13%	23%	17%	16%	13%	13%	19%	24%	63%
	T	31	24	17	20	24	15	16	25	61
At least one horizon	%	44%	34%	24%	29%	34%	21%	23%	36%	87%
R^2	mean	5.64%	median	3.76%	max	22.59%	min	1.26%		
p(Wald) < 10%	T	35	50%							
Panel C: Starting sample in January 1995										
$r^c_{s,t-21:t-2}$	T	11	8	5	5	14	2	7	8	37
	%	16%	11%	7%	7%	20%	3%	10%	11%	53%
$r^c_{s,t-41:t-22}$	T	12	36	9	7	6	3	12	21	53
	%	17%	51%	13%	10%	9%	4%	17%	30%	76%
	T	22	41	14	11	18	5	16	23	60
At least one horizon	%	31%	59%	20%	16%	26%	7%	23%	33%	86%
R^2	mean	5.90%	median	4.39%	max	22.76%	min	1.90%		
p(Wald) < 10%	T	33	47%							

Table A.5. Contemporaneous and predictive regressions

This table summarizes the results of the following regression with overlapping observations for each country *i*:

$$r_{i,t:t+19} = \alpha_i + \sum_s (\beta_{i,s,0} r^c_{s,t:t+19} + \beta_{i,s,1} r^c_{s,t-21:t-2} + \beta_{i,s,2} r^c_{s,t-41:t-22} + \beta_{i,s,3} r^c_{s,t-62:t-42}) + \phi_i r_{i,t-20:t-1} + \varepsilon_{i,t:t+19}$$

It shows the number of countries for which monthly stock market excess returns ($r_{i,t:t+19}$) are predicted by sector-*s* contemporaneous ($r^c_{s,t:t+19}$) and one-month ($r^c_{s,t-21:t-2}$), two-month ($r^c_{s,t-41:t-22}$) and three-month ($r^c_{s,t-61:t-42}$) lagged commodity futures returns at the 10% significance level (based on Newey-West standard errors). *T*, *%*, *P* and *N* stand for total number, percentage, number of positive and number of negative countries (out of 70), respectively. The table also shows descriptive statistics for the regression R^2 and counts of significant coefficients at all lags, for at least one sector and for at least one lag. *p(Wald) < 10%* denotes the number of countries for which we reject the null hypothesis that all commodity sector returns fail to predict stock market excess returns. The sample period is from November 1979 to March 2016.

Table A.5. Contemporaneous and predictive regressions

(continued)

		Energy	Industrial Metals	Agriculture I	Agriculture II	Livestock & Meats	Precious Metals	EWI	GSI	At least one sector
Separate Regression per country, controlling for lagged stock return										
$r^c_{s,t:t+19}$	T	24	50	15	48	6	29	68	58	69
	%	34%	71%	21%	69%	9%	41%	97%	83%	99%
	P	21	48	15	46	4	29	68	58	
	N	3	2	0	2	2	0	0	0	
$r^c_{s,t-21:t-2}$	T	25	23	6	14	16	9	8	16	53
	%	36%	33%	9%	20%	23%	13%	11%	23%	76%
	P	3	9	3	13	13	4	5	6	
	N	22	14	3	1	3	5	3	10	
$r^c_{s,t-41:t-22}$	T	8	8	19	12	10	14	9	11	45
	%	11%	11%	27%	17%	14%	20%	13%	16%	64%
	P	2	7	3	6	7	13	3	11	
	N	6	1	16	6	3	1	6	0	
$r^c_{s,t-61:t-42}$	T	13	9	8	6	20	12	7	9	45
	%	19%	13%	11%	9%	29%	17%	10%	13%	64%
	P	3	6	5	2	20	10	6	9	
	N	10	3	3	4	0	2	1	0	
All Lags	T	0	2	0	4	0	1	3	3	
	%	18.57%	12.86%	11.43%	8.57%	28.57%	17.14%	10.00%	12.86%	
At least one lag	T	46	58	36	52	26	42	68	61	70
	%	66%	83%	51%	74%	37%	60%	97%	87%	100%
	P	23	53	20	48	20	37	68	59	
	N	28	16	19	7	8	6	8	10	
R^2	mean	15.77%	median	13.84%	max	42.89%	min	4.98%		
p(Wald) <10%	T	70	100%							

Appendix B

Drawing Up the Bill: Does Sustainable Investing Affect Stock Returns Around the World?

B.1. Appendix tables: variable definitions, data sources, and further descriptive statistics

Table B.1. Variable definitions and data sources

This table provides the definitions and data sources of the variables used throughout the paper.

ESG Variables	
ESG Ratings	ESG ratings are sourced from Refinitiv, MSCI IVA, and Sustainalytics. We use the separate environment (*E*), social (*S*), and governance (*G*) ratings provided by each rater. We further construct *ESG* ratings by averaging over the three dimensions. In the case of MSCI IVA and Sustainalytics we weigh each dimension by the weights provided by the rater. These weights reflect the importance of each dimension for a given firm. In the case of Refinitiv we use equal weights because the rater does not provide alternative weights. We also construct *Composite* ESG ratings for each dimension of ESG (*E*, *S*, *G*, and *ESG*) by averaging over the ratings of the three main raters along the relevant dimension. We convert the individual ratings to percentile ranks in each time period before averaging. When constructing *Composite* ESG ratings we only require that at least one rating is available. All the ratings range from zero to 100. In the case of MSCI IVA the original ratings range from zero to 10 and we multiply the ratings by 10 following Christensen, Serafeim and Sikochi (2021) and Serafeim and Yoon (2021).

Table B.1. Variable definitions and data sources

(continued)

ESG Variables	
ESG Momentum	Year-on-year change of the ESG ratings defined above.
Stock Returns & Characteristics	
Returns	We follow the approach of Bessembinder et al. (2019). For US stocks we use the monthly stock returns in the CRSP field *RET*. For non-US stocks we compute monthly stock returns in US dollars using Compustat daily data and the formula $\frac{PRCCD_t \times FX_t \times QUNIT_{t-1} \times AJEXDI_{t-1} \times TRFD_t}{PRCCD_{t-1} \times FX_{t-1} \times QUNIT_t \times AJEXDI_t \times TRFD_{t-1}}$. $PRCCD_t$ denotes the closing stock price at time *t*. $TRFD_t$, $AJEXDI_t$ and $QUNIT_t$ adjust for dividends, stock-splits, and quotation units. FX_t is the exchange rate from local currency to US dollars. We use the last day of each month with a non-zero closing price to compute monthly returns. We limit the sample to stock-months with more than four days of non-zero closing prices. Returns are adjusted for delisting as in Bessembinder et al. (2019).
Size	Natural logarithm of market capitalization in millions of dollars. For US stocks we define market capitalization as the absolute value of the product of end-of-year stock price (CRSP field *ALTPRC*) and the number of shares outstanding (CRSP field *SHROUT*). We use CRSP field *ALTPRC* instead of *PRC* following Bali, Engle and Murray (2016). We adjust market capitalization for stock-splits by multiplying by the adjustment factor $\frac{CFACSHR}{CFACPR}$ as recommended in the CRSP documentation. For non-US stocks market capitalization is computed using Compustat data and the formula $PRCCD \times FX \times QUNIT^{-1} \times CSHOC$ following Bessembinder et al. (2019). These data fields are defined in the variable definition of *Returns*. We convert market capitalization to millions of dollars.
B/M	Natural logarithm of book market of equity divided by market capitalization. The book market of equity is computed as the sum of the following Compustat items: book value of stockholder's equity (*SEQ*), deferred taxes (*TXBD*), investment tax credit (*ITCB*). We subtract the book value of preferred stock which is defined as the redemption value (*PRTKRV*), liquidating value (*PSTKL*), or par value (*PSTK*) as available. If none of these measures is available we assume the book value of preferred stock is zero. Market capitalization is computed as described in the definition of the variable *Size*. Negative values of book market of equity are set to zero.

Table B.1. Variable definitions and data sources

(continued)

Stock Returns & Characteristics	
B/M Dummy	Dummy equal to one if *B/M* takes a negative value and zero otherwise. Refer to Fama and French (1992) and Hou, Kho and Karolyi (2011) for more examples of this approach of dealing with negative accounting values.
Momentum	Cumulative return (in US dollars) over the previous 12 months (excluding the last month). Following Bali, Engle and Murray (2016) we set *Momentum* to missing if there are fewer than nine months of available data during the 11-month period used to compute *Momentum*.
Total Volatility	Standard deviation of monthly stock returns (in US dollars) over the previous 12 months.
Inverse Price Ratio	Inverse of year-end share price (in US dollars). Share prices are measured as described in the definition of the variable *Returns*.
Leverage	Ratio of total debt (Compustat items *DLTT+DLC*) to total assets (Compustat item *AT*).
Investment	Percentage change in total assets (Compustat item *AT*).
Gross Profitability	Revenues (Compustat item *REVT*) minus costs of goods sold (Compustat item *COGS*) divided by total assets (Compustat item *AT*).
R&D	R&D spending (Compustat item *XRD*) divided by total assets (Compustat item *AT*). When R&D is missing, it is assumed zero.
Tangibility	Property, plant, and equipment (Compustat item *PPENT*) divided by total assets (Compustat item *AT*).

Table B.2. Sample coverage by country

This table summarizes the cross-sectional and time-series coverage of the sample within various geographic units: countries, regions, and worldwide. The seven regions are Asia-Pacific, Emerging Asia, Emerging Europe, Middle-East and Africa (EEMA), Europe, Japan, Latin America, and North America. Following Fama and French (2017), Japan and Asia-Pacific are treated as different regions. For each geographic unit the table reports the starting date and the number of unique firms/stocks and industries covered. These statistics are provided for the full sample and for subsamples. The subsamples restrict the sample to those stock-months in year *t+1* for which ESG ratings from a specific rater are available in year *t-1*. This results in a subsample for each rater: the Refinitiv subsample, the Sustainalytics subsample, and the MSCI IVA subsample. The full sample is obtained by constructing a fictitious rater (*Composite*) which combines the available ratings of the other three raters. This is done by averaging the ratings across the three raters. We convert the ratings of each of these three raters at each point in time to percentile ranks before averaging.

	Refinitiv Sample			Sustainalytics Sample			MSCI IVA Sample			*Composite* Sample		
Country	Start Date	No. Stocks	No. Industries	Start Date	No. Stocks	No. Industries	Start Date	No. Stocks	No. Industries	Start Date	No. Stocks	No. Industries
Panel A: Asia Pacific												
Australia	2004-Jan	378	61	2011-Jan	220	52	2003-Jul	306	58	2003-Jul	403	61
Hong Kong	2004-Jan	200	58	2011-Jan	213	57	2001-Jan	296	62	2001-Jan	331	63
New Zealand	2006-Jan	46	27	2011-Jan	20	17	2005-Jan	48	26	2005-Jan	62	31
Singapore	2006-Jan	39	28	2011-Jan	40	24	2005-Jan	61	38	2005-Jan	71	39
Panel B: Emerging Asia												
China	2010-Jan	186	44	2013-Jan	60	28	2009-Jan	394	53	2009-Jan	430	54
India	2009-Jan	93	34	2012-Jan	124	35	2004-Jul	232	49	2004-Jul	254	50
Indonesia	2010-Jan	29	18	2013-Jan	44	19	2005-Jan	59	30	2005-Jan	69	31
Malaysia	2010-Jan	48	27	2011-Jan	67	25	2008-Jan	98	35	2008-Jan	127	37
Pakistan							2009-Jan	13	7	2009-Jan	13	7
Philippines	2010-Jan	17	9	2013-Jan	22	11	2007-Jul	32	17	2007-Jul	34	17
South Korea	2004-Jan	124	49	2011-Jan	117	44	2002-Jan	149	50	2002-Jan	160	51
Taiwan	2004-Jan	129	34	2011-Jan	132	36	2005-Jan	137	35	2004-Jan	158	38
Thailand	2009-Jan	29	18	2013-Jan	41	19	2003-Jan	60	27	2003-Jan	72	27

Table B.2. Sample coverage by country

(continued)

	Refinitiv Sample			Sustainalytics Sample			MSCI IVA Sample			*Composite* Sample		
Country	Start Date	No. Stocks	No. Industries	Start Date	No. Stocks	No. Industries	Start Date	No. Stocks	No. Industries	Start Date	No. Stocks	No. Industries
Panel C: EMMA												
Israel	2004-Jan	10	8	2011-Jan	24	15	2008-Jan	22	14	2004-Jan	29	17
Morocco							2009-Jan	11	9	2009-Jan	11	9
Nigeria							2015-Jan	13	6	2015-Jan	13	6
Poland	2009-Jan	33	18	2012-Jan	22	14	2005-Jan	28	16	2005-Jan	38	20
Russia	2009-Jan	32	13	2011-Jan	31	14	2006-Jan	39	16	2006-Jan	44	17
Saudi Arabia	2009-Jan	16	9							2009-Jan	16	9
South Africa	2010-Jan	99	36	2013-Jan	59	29	2008-Jan	95	33	2008-Jan	108	37
Turkey	2010-Jan	16	12				2006-Jan	10	7	2006-Jan	19	13
Panel D: Europe												
Austria	2004-Jan	23	16	2011-Jan	22	19	2005-Jan	27	22	2004-Jan	33	24
Belgium	2004-Jan	37	28	2011-Jan	18	16	2001-Jan	40	30	2001-Jan	42	30
Denmark	2004-Jan	40	25	2011-Jan	22	19	2002-Jan	44	26	2002-Jan	46	27
Finland	2004-Jan	34	25	2011-Jan	32	22	2001-Jan	42	27	2001-Jan	45	27
France	2004-Jan	151	56	2011-Jan	128	54	2001-Jan	180	58	2001-Jan	192	59
Germany	2004-Jan	146	52	2011-Jan	139	50	2001-Jan	188	60	2001-Jan	211	60
Greece	2004-Jan	24	19				2005-Jan	16	13	2004-Jan	27	21
Ireland	2004-Jan	17	16	2011-Jan	13	11	2003-Jan	16	13	2003-Jan	20	17
Italy	2004-Jan	78	36	2011-Jan	41	25	2002-Jan	105	41	2002-Jan	106	41
Netherlands	2004-Jan	56	34	2011-Jan	60	33	2001-Jan	60	34	2001-Jan	77	39
Norway	2004-Jan	53	27	2011-Jan	61	30	2002-Jan	64	27	2002-Jan	87	34
Portugal	2004-Jan	17	15	2011-Jan	10	10	2003-Jan	17	16	2003-Jan	18	16
Spain	2004-Jan	63	33	2011-Jan	43	23	2002-Jan	74	35	2002-Jan	78	36
Sweden	2004-Jan	109	44	2011-Jan	88	42	2001-Jan	189	51	2001-Jan	194	52
Switzerland	2004-Jan	103	37	2011-Jan	86	36	2001-Jan	113	41	2001-Jan	128	43
UK	2004-Jan	424	68	2011-Jan	307	61	2001-Jan	571	66	2001-Jan	608	68

Table B.2. Sample coverage by country

(continued)

	Refinitiv Sample			Sustainalytics Sample			MSCI IVA Sample			*Composite* Sample		
Country	Start Date	No. Stocks	No. Industries	Start Date	No. Stocks	No. Industries	Start Date	No. Stocks	No. Industries	Start Date	No. Stocks	No. Industries
Panel E: Latin America												
Argentina	2017-Jan	45	20				2005-Jan	15	9	2005-Jan	49	21
Brazil	2009-Jan	54	29	2011-Jan	48	29	2004-Jan	82	35	2004-Jan	88	36
Chile	2009-Jan	31	19	2013-Jan	21	13	2008-Jan	28	16	2008-Jan	38	21
Colombia	2010-Jan	16	10	2013-Jan	12	8	2012-Jan	14	7	2010-Jan	18	10
Mexico	2004-Jan	36	21	2013-Jan	33	20	2003-Jan	39	21	2003-Jan	47	25
Peru	2010-Jan	22	11				2010-Jan	16	8	2010-Jan	29	14
Panel F: North America												
Canada	2004-Jan	293	51	2011-Jan	257	53	2001-Jan	362	59	2001-Jan	403	61
US	2004-Jan	2,800	72	2011-Jan	1,238	71	2001-Jan	3,184	73	2001-Jan	3,487	73
Panel G: World Regions												
Asia-Pacific	2004-Jan	663	67	2011-Jan	493	64	2001-Jan	711	67	2001-Jan	867	68
Emerging Asia	2004-Jan	655	64	2011-Jan	607	62	2002-Jan	1,174	68	2002-Jan	1,317	68
EMMA	2004-Jan	206	49	2011-Jan	136	42	2005-Jan	218	51	2004-Jan	278	54
Europe	2004-Jan	1,375	69	2011-Jan	1,070	68	2001-Jan	1,746	71	2001-Jan	1,912	71
Japan	2004-Jan	399	64	2011-Jan	457	62	2001-Jan	704	67	2001-Jan	723	68
Latin America	2004-Jan	204	46	2011-Jan	114	40	2003-Jan	194	44	2003-Jan	269	48
North America	2004-Jan	3,093	72	2011-Jan	1,495	71	2001-Jan	3,546	73	2001-Jan	3,890	73
Total	2004-Jan	6,593	72	2011-Jan	4,371	71	2001-Jan	8,291	73	2001-Jan	9,253	73

Table B.3. Average *ESG* ratings by country

This table shows the average *ESG* ratings of each rater per country and per region during the period from 1999 until 2018. The table also shows the *ESG* ratings in 2018. The seven regions are Asia-Pacific, Emerging Asia, Emerging Europe, Middle-East and Africa (EEMA), Europe, Japan, Latin America, and North America. Following Fama and French (2017), Japan and Asia-Pacific are treated as different regions. The four raters are Refinitiv, Sustainalytics, MSCI IVA, and *Composite*. *Composite* is a fictitious rater obtained by averaging over the ratings of the other three raters. We convert the ratings of each of these three raters at each point in time to percentile ranks before averaging.

	All Sample Years (1999-2018)				Restricted to Year 2018			
Country	Refinitiv	Sustainalytics	MSCI IVA	Composite	Refinitiv	Sustainalytics	MSCI IVA	Composite
Panel A: Asia Pacific								
Australia	40.29	49.59	55.25	42.22	41.53	48.38	66.05	43.23
Hong Kong	41.72	25.04	28.23	31.60	59.79	34.07	39.00	44.55
New Zealand	42.87	51.20	65.23	48.29	44.28	49.50	80.49	51.65
Singapore	40.12	26.54	46.92	37.09	58.19	39.01	52.34	50.56
Panel B: Emerging Asia								
China	29.09	21.16	21.45	22.93	32.09	28.29	20.70	23.28
India	58.52	45.86	41.21	47.37	62.85	50.98	41.64	46.64
Indonesia	51.43	35.81	45.79	44.60	54.88	37.75	45.02	47.41
Malaysia	46.96	31.57	40.89	38.54	60.98	39.58	43.89	44.06
Philippines	42.95	25.88	26.48	30.85	51.09	29.78	22.33	28.13
South Korea	52.95	46.33	43.83	46.96	49.85	39.52	32.27	38.28
Taiwan	44.97	44.28	36.66	41.40	58.68	58.18	44.96	53.47
Thailand	61.21	48.22	50.37	48.36	72.60	59.11	53.38	55.57

Table B.3. Average *ESG* ratings by country

(continued)

	All Sample Years (1999-2018)				Restricted to Year 2018			
Country	Refinitiv	Sustainalytics	MSCI IVA	Composite	Refinitiv	Sustainalytics	MSCI IVA	Composite
Panel C: EMMA								
Israel	43.49	35.28	36.54	36.75	41.04	53.73	40.73	40.20
Morocco			49.47	50.85			46.33	46.46
Nigeria			59.26	58.74			63.66	63.66
Pakistan			39.74	39.92			58.75	58.17
Poland	41.01	35.89	44.07	39.89	49.77	38.14	36.84	40.87
Russia	48.24	39.81	24.83	37.03	57.31	49.68	19.09	36.19
Saudi Arabia	29.04			28.12	36.38			33.70
South Africa	61.12	71.63	61.95	60.67	64.05	72.63	63.92	62.98
Turkey	44.57		37.65	40.00	48.04		40.91	43.88
Panel D: Europe								
Austria	60.66	59.90	70.98	60.58	70.16	70.42	75.37	68.87
Belgium	53.89	64.02	57.02	51.97	62.55	70.04	61.40	60.57
Denmark	60.95	75.26	69.91	63.78	65.95	74.94	71.32	66.84
Finland	67.57	83.37	73.74	70.10	75.02	87.68	83.89	80.23
France	70.23	67.93	66.31	64.18	69.60	71.37	66.07	66.53
Germany	65.23	57.32	63.46	57.44	63.00	60.93	60.45	58.98
Greece	46.13		42.75	41.88	53.94		60.10	53.99
Ireland	45.95	44.26	46.27	41.73	63.80	57.42	67.29	59.36
Italy	64.39	67.16	51.23	55.29	72.39	70.38	55.15	61.77
Netherlands	71.22	68.55	70.21	65.87	69.03	75.17	73.84	69.92
Norway	58.46	62.85	62.69	56.82	56.67	55.29	67.83	55.11
Portugal	70.15	75.90	62.14	63.47	72.05	91.02	67.24	67.48
Spain	72.65	74.70	64.77	66.10	75.07	79.43	63.06	66.93
Sweden	66.56	73.28	72.14	66.90	61.76	63.64	70.77	64.01
Switzerland	56.03	54.98	61.92	53.64	52.44	52.26	68.14	55.45
United Kingdom	58.82	62.21	61.04	56.80	60.48	58.45	72.91	62.65

Table B.3. Average *ESG* ratings by country

(continued)

	All Sample Years (1999-2018)				Restricted to Year 2018			
Country	Refinitiv	Sustainalytics	MSCI IVA	Composite	Refinitiv	Sustainalytics	MSCI IVA	Composite
Panel E: Latin America								
Argentina	38.37		58.59	43.94	43.00		44.82	39.84
Brazil	56.03	59.17	43.34	50.12	59.21	61.31	35.02	40.83
Chile	44.97	57.59	49.13	45.86	51.34	59.54	61.82	51.28
Colombia	64.16	74.82	48.67	63.63	68.01	70.51	50.36	62.45
Mexico	52.99	50.58	36.43	44.96	56.82	57.10	37.59	46.84
Peru	37.61		53.16	43.65	38.10		53.84	46.18
Panel F: North America								
Canada	45.09	50.95	52.99	47.20	50.48	48.05	58.94	51.49
United States of America	44.19	45.51	43.32	42.36	40.46	41.99	45.44	39.85
Panel G: World Regions								
Asia-Pacific	40.90	36.47	45.21	38.11	49.34	41.53	51.18	44.98
Emerging Asia	48.20	40.88	38.52	40.57	50.20	46.64	33.84	37.94
EMMA	52.45	53.00	49.75	48.72	55.79	59.74	47.63	49.35
Europe	62.44	64.58	63.23	59.13	63.80	64.04	68.13	62.92
Japan	53.07	49.98	56.38	51.73	59.56	46.20	53.98	51.70
Latin America	51.02	57.92	44.59	48.09	52.03	60.64	42.81	45.27
North America	44.31	46.46	44.30	42.93	41.34	43.03	46.83	41.03

B.2. Additional analyses and robustness tests

Figure B.1. ESG ratings and stock returns: world regions, 2001-2020

This figure summarizes the results from running panel regressions of monthly stock returns on the lagged ESG ratings of stocks traded in one of the following regions: Asia-Pacific, Japan, Europe, North America, and Emerging Countries. All regressions include all control variables listed in Appendix Table B.1 as well as country-month and industry-month fixed effects. Following Bolton and Kacperczyk (2021), industries are defined based on six-digit Global Industry Classification Standard Codes (GICS) codes. Each plot in the figure shows the results of running regressions using one of the following four types of ESG ratings: environmental (*E*), social (*S*), governance (*G*), and ESG (*ESG*). We use ratings from four raters: Refinitiv, MSCI IVA, Sustainalytics, and *Composite*. *Composite* combines the available ratings of the other three raters by averaging their ratings. We convert the ratings of each of the other three raters at each point in time to percentile ranks before averaging. Each bar represents the regression coefficient on the ESG rating variable used in a given regression. Confidence intervals are depicted as whiskers around the point estimates. The longer (shorter) whiskers represent 95% (90%) confidence intervals. Standard errors are double clustered at the stock and month levels. The sample period is from January 2001 to December 2020.

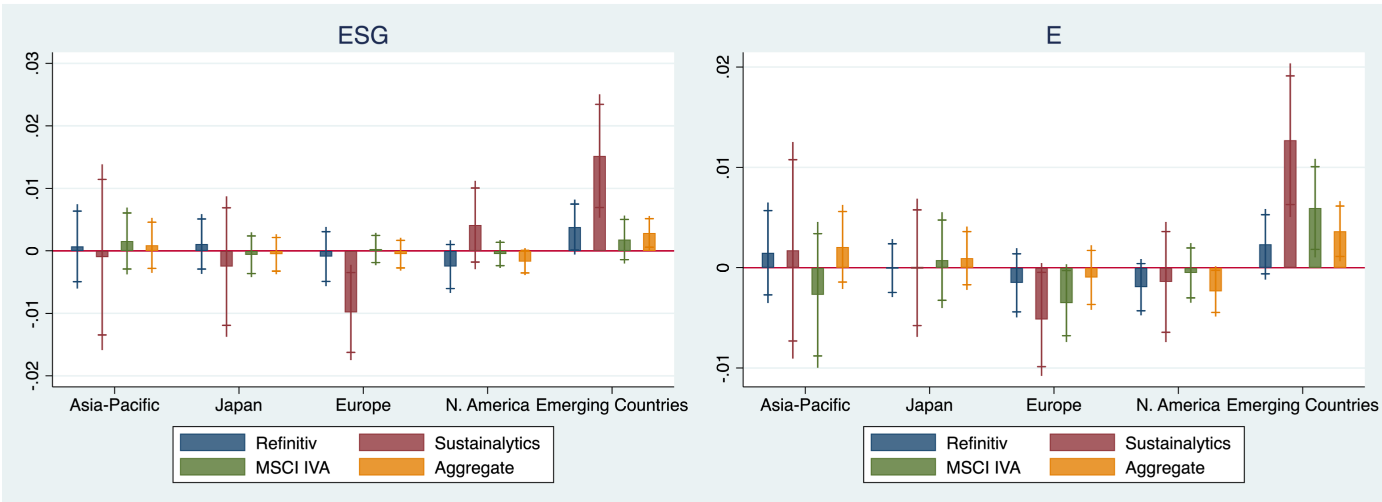

Figure B.1. ESG ratings and stock returns: world regions, 2001-2020

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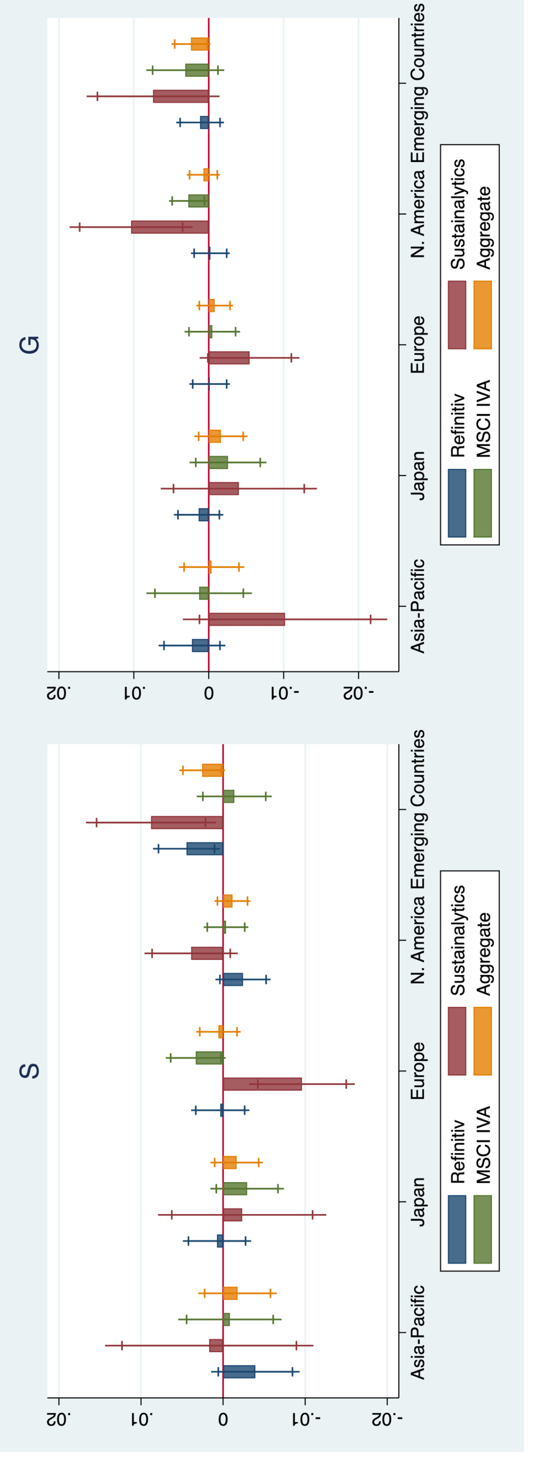

Figure B.2. ESG momentum and stock returns: world regions, 2001-2020

This figure summarizes the results from running panel regressions of monthly stock returns on the lagged ESG momentum of stocks traded in one of the following regions: Asia-Pacific, Japan, Europe, North America, and Emerging Countries. ESG momentum is defined as the year-on-year change in ESG ratings. All regressions include all control variables listed in Appendix Table B.1 as well as country-month and industry-month fixed effects. Following Bolton and Kacperczyk (2021), industries are defined based on six-digit Global Industry Classification Standard Codes (GICS) codes. Each plot in the figure shows the results of running regressions using one of the following four types of ESG momentum: environmental (*E*), social (*S*), governance (*G*), and ESG (*ESG*). We use ratings from four raters: Refinitiv, MSCI IVA, Sustainalytics, and *Composite*. *Composite* combines the available ratings of the other three raters by averaging their ratings. We convert the ratings of each of the other three raters at each point in time to percentile ranks before averaging. Each bar represents the regression coefficient on the ESG momentum variable used in a given regression. Confidence intervals are depicted as whiskers around the point estimates. The longer (shorter) whiskers represent 95% (90%) confidence intervals. Standard errors are double clustered at the stock and month levels. The sample period is from January 2001 to December 2020.

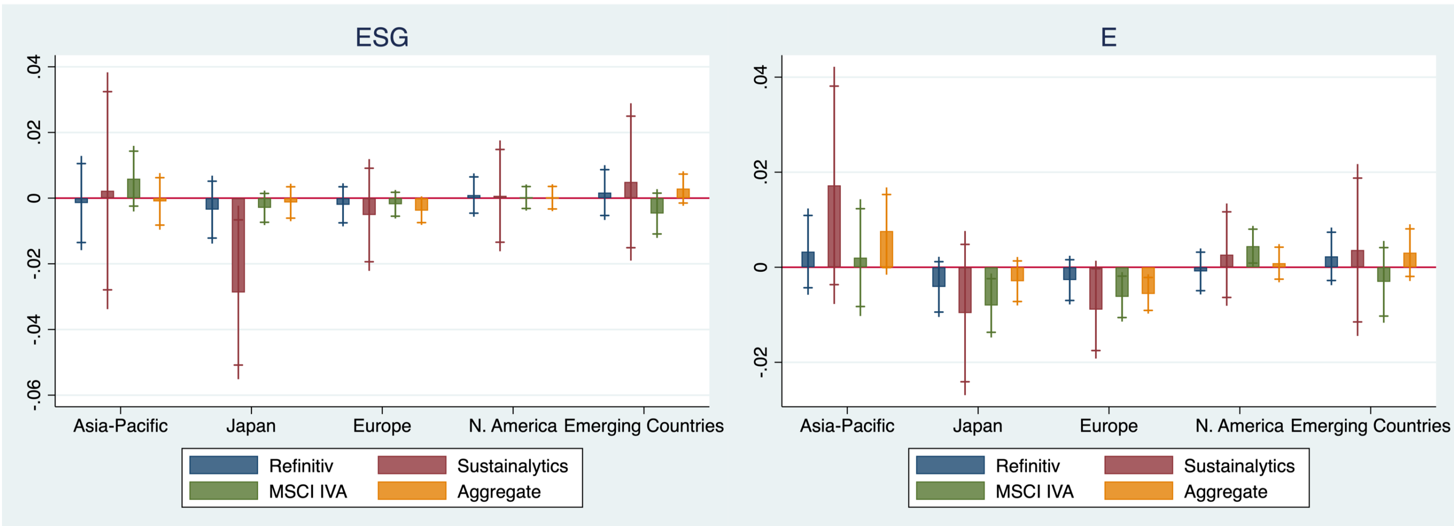

Figure B.2. ESG momentum and stock returns: world regions, 2001-2020

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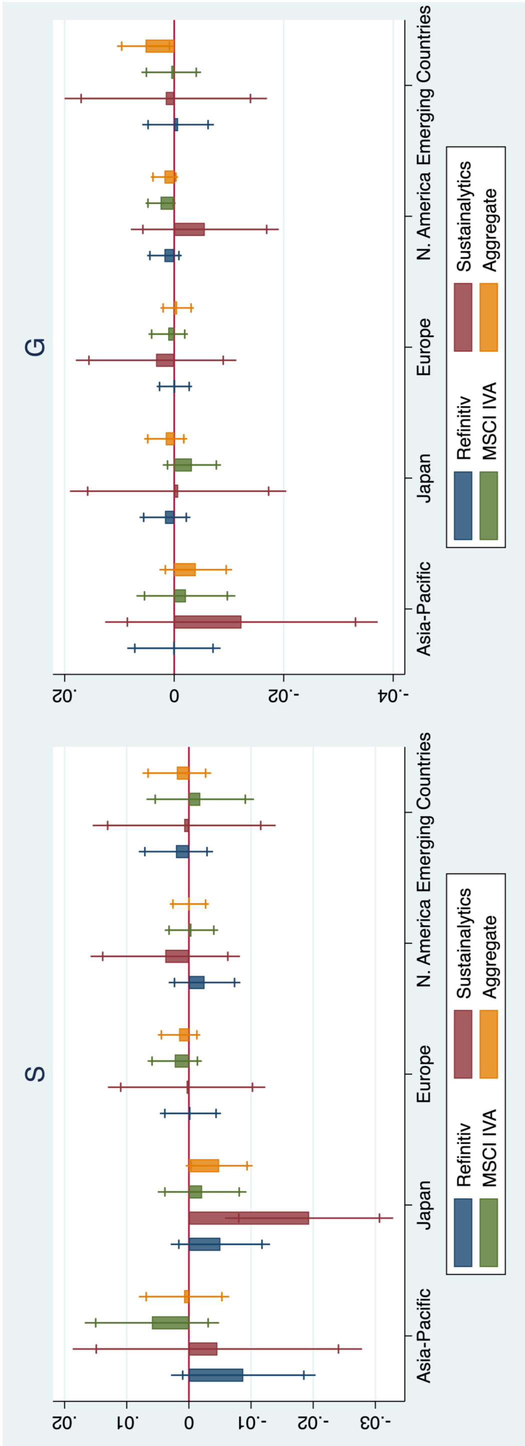

Figure B.3. ESG momentum and stock returns: world regions, 2001-2012 and 2013-2020

This figure summarizes the results from running panel regressions of monthly stock returns on the lagged ESG momentum of stocks traded in one of the following regions: Asia-Pacific, Japan, Europe, North America, and Emerging Countries. ESG momentum is defined as the year-on-year change in ESG ratings. All regressions include all control variables listed in Appendix Table B.1 as well as country-month and industry-month fixed effects. Following Bolton and Kacperczyk (2021), industries are defined based on six-digit Global Industry Classification Standard Codes (GICS) codes. The plots on the left (right) show the results of running these regressions over the period January 2001 to December 2012 (January 2013 to December 2020). Each plot in the figure shows the results of running regressions using one of the following four types of ESG momentum: environmental (*E*), social (*S*), governance (*G*), and ESG (*ESG*). We use ratings from four raters: Refinitiv, MSCI IVA, Sustainalytics, and *Composite*. *Composite* combines the available ratings of the other three raters by averaging their ratings. We convert the ratings of each of the other three raters at each point in time to percentile ranks before averaging. Each bar represents the regression coefficient on the ESG momentum variable used in a given regression. Confidence intervals are depicted as whiskers around the point estimates. The longer (shorter) whiskers represent 95% (90%) confidence intervals. Standard errors are double clustered at the stock and month levels.

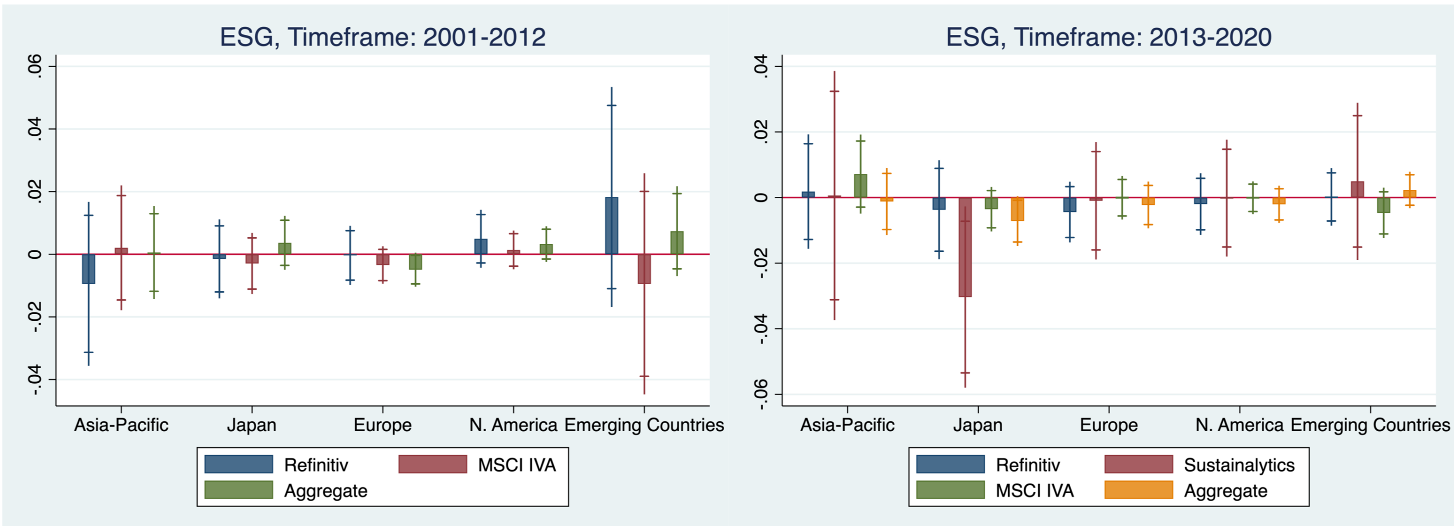

Figure B.3. ESG momentum and stock returns: world regions, 2001-2012 and 2013-2020

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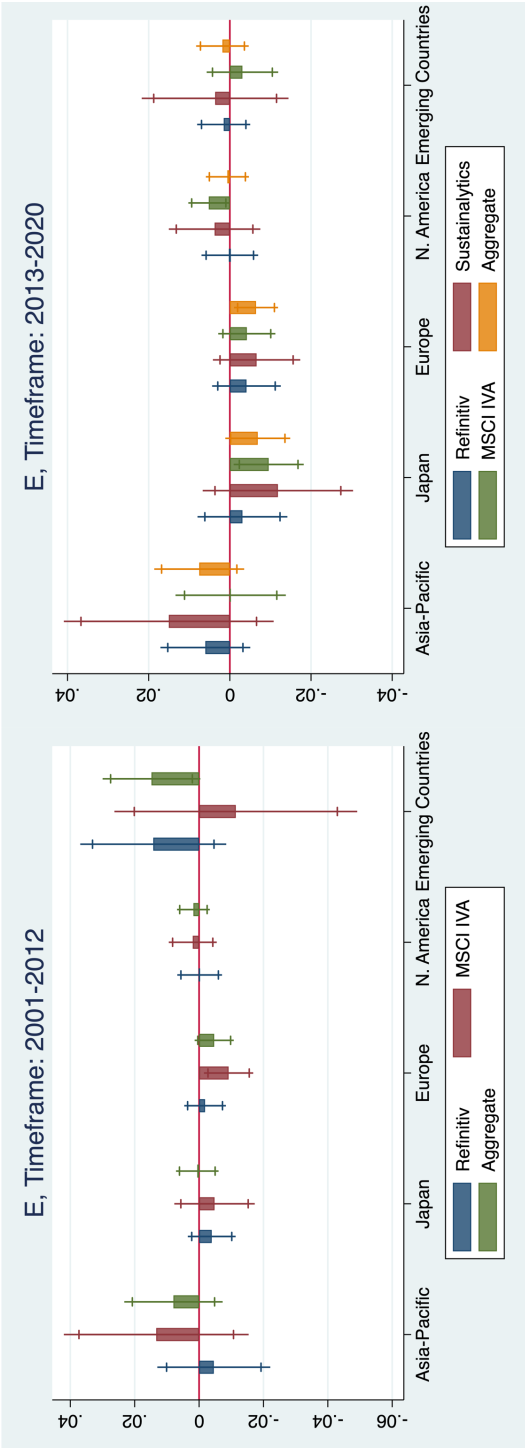

Figure B.3. ESG momentum and stock returns: world regions, 2001-2012 and 2013-2020

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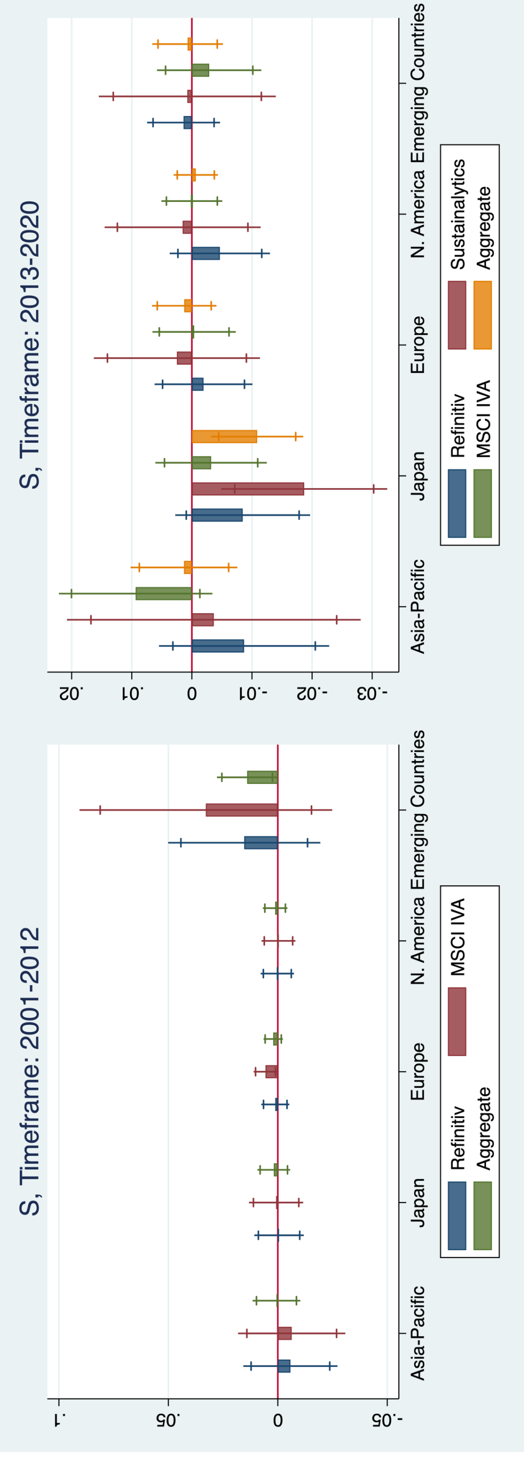

Figure B.3. ESG momentum and stock returns: world regions, 2001-2012 and 2013-2020

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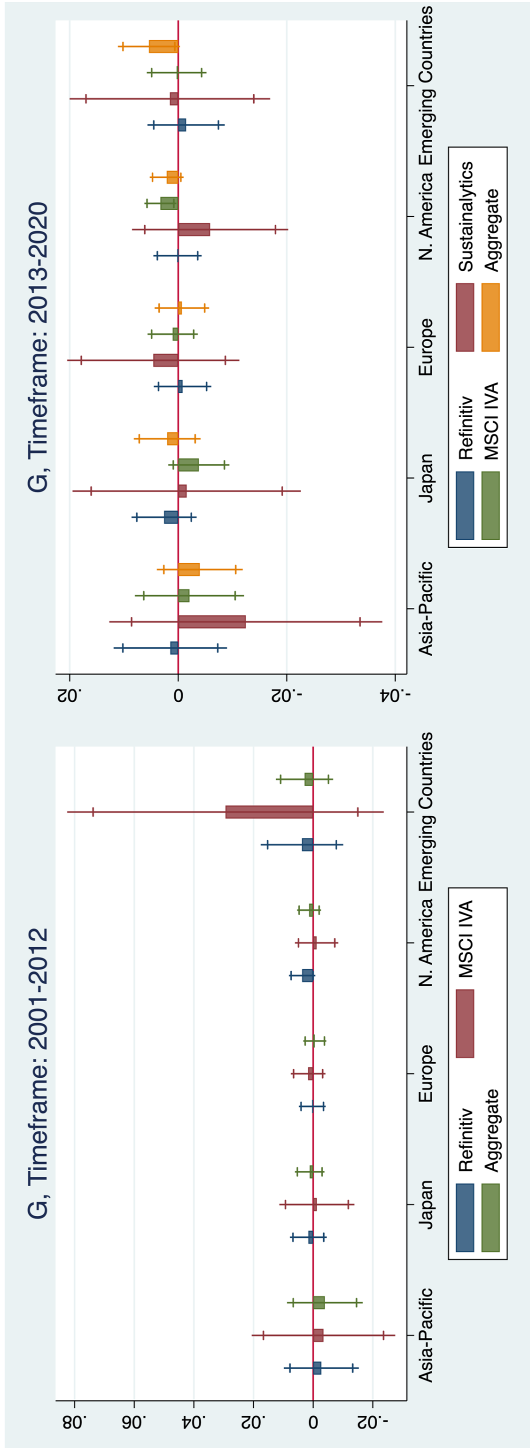

Figure B.4. ESG ratings and stock returns: sectors of economic activity, additional results

This figure summarizes the results from running panel regressions of monthly stock returns on the lagged ESG ratings of stocks traded in one of the following sectors of economic activity: consumer staples, communication services, financials, healthcare, industrials, information technology, and utilites. These sectors are defined based on Global Industry Classification Standard Codes (GICS) codes. All regressions include all control variables listed in Appendix Table B.1 as well as country-month and industry-month fixed effects. Following Bolton and Kacperczyk (2021), industries are defined based on six-digit GICS codes. We consider four types of ESG ratings: environmental (*E*), social (*S*), governance (*G*), and ESG (*ESG*). We use ratings from four raters: Refinitiv, MSCI IVA, Sustainalytics, and *Composite*. *Composite* combines the available ratings of the other three raters by averaging their ratings. We convert the ratings of each of the other three raters at each point in time to percentile ranks before averaging. Each plot in the figure shows the results of running regressions for a given sector. Each bar represents the regression coefficient on the ESG rating variable used in a given regression. Confidence intervals are depicted as whiskers around the point estimates. The longer (shorter) whiskers represent 95% (90%) confidence intervals. Standard errors are double clustered at the stock and month levels. The sample period is from January 2001 to December 2020.

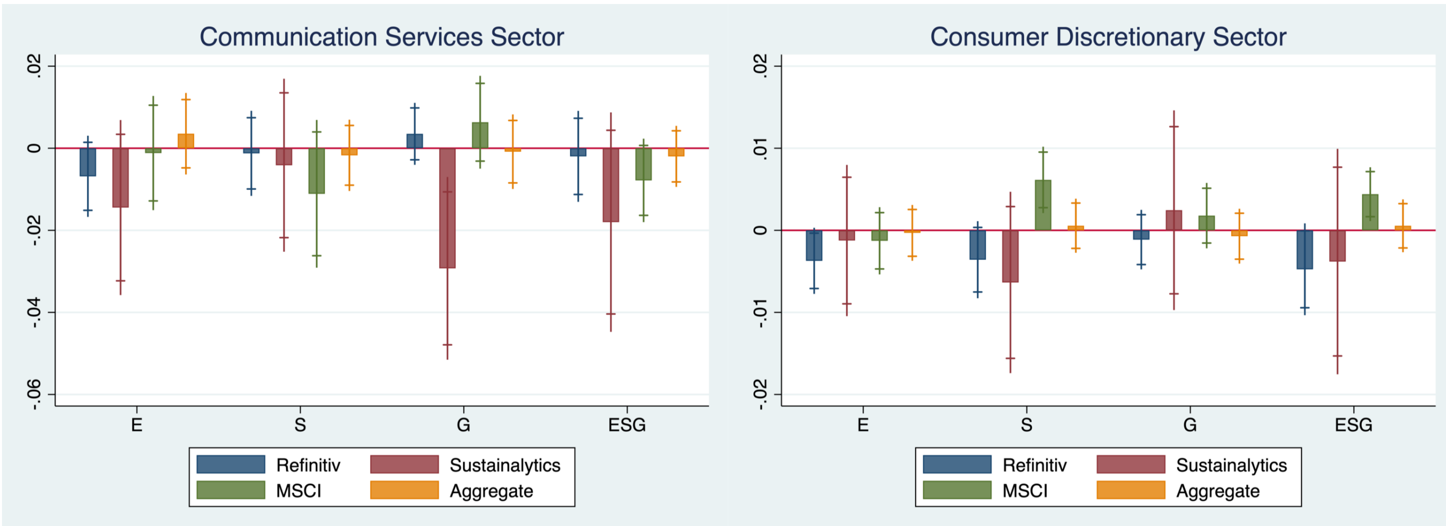

Figure B.4. ESG ratings and stock returns: sectors of economic activity, additional results

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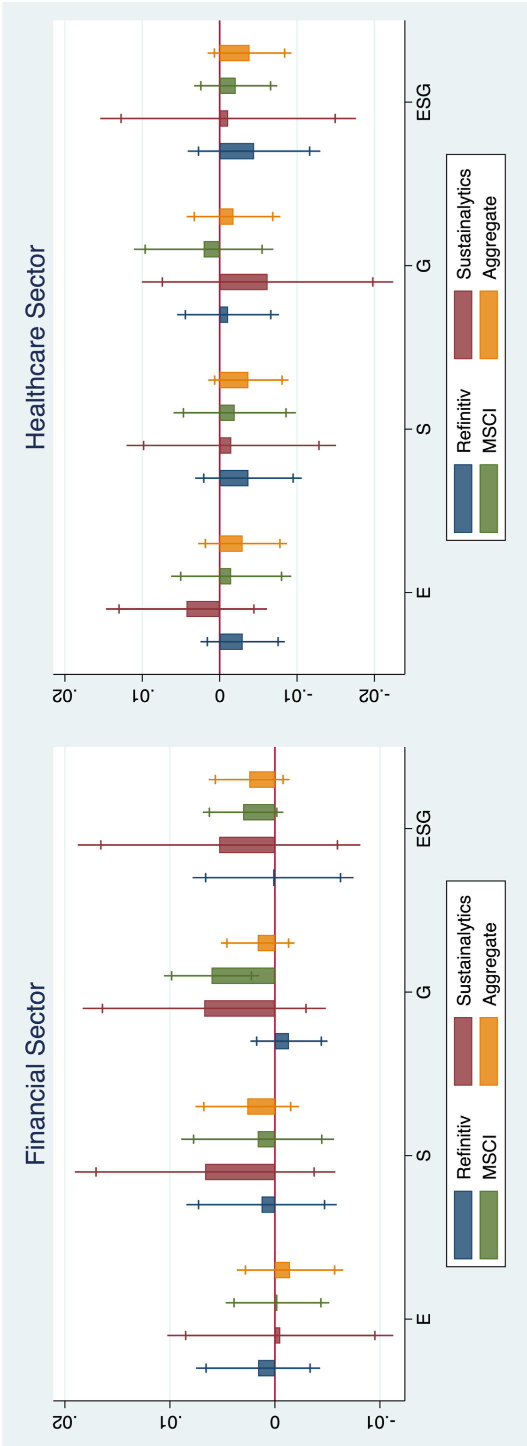

Figure B.4. ESG ratings and stock returns: sectors of economic activity, additional results

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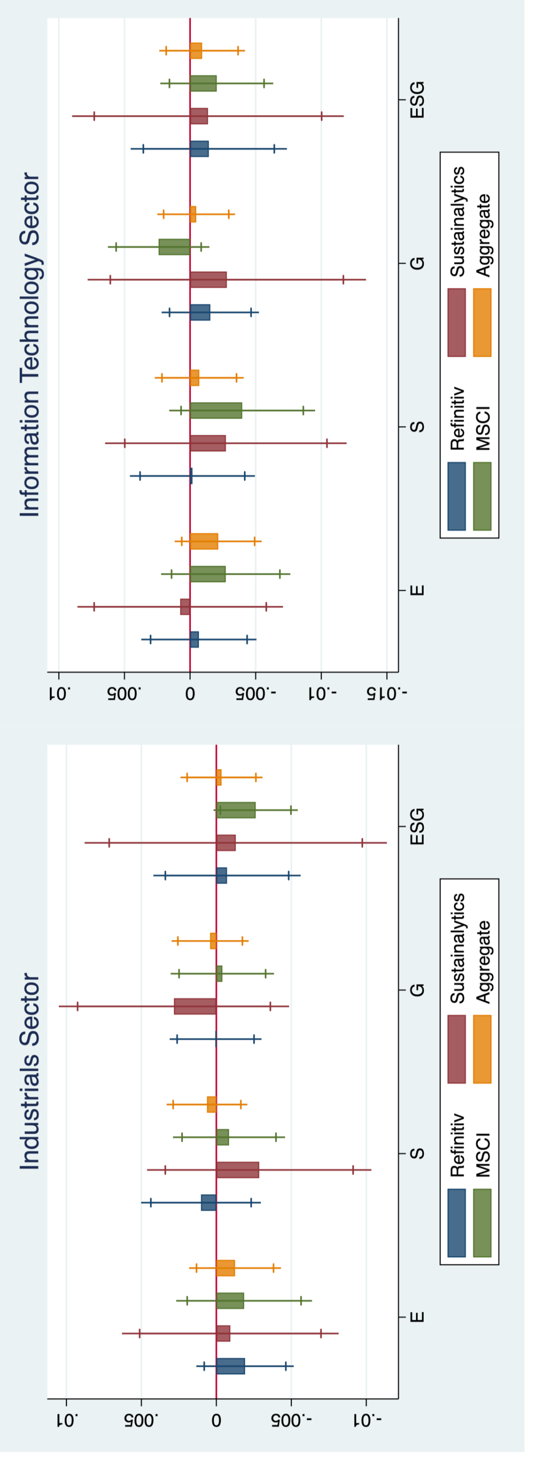

Figure B.4. ESG ratings and stock returns: sectors of economic activity, additional results

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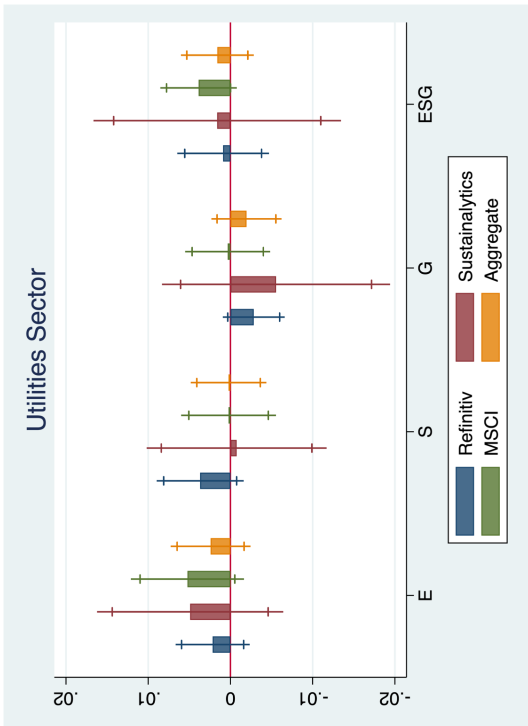

Figure B.5. ESG momentum and stock returns: sectors of economic activity

This figure summarizes the results from running panel regressions of monthly stock returns on the lagged ESG momentum of stocks traded in one of the following sectors of economic activity: communication services, consumer discretionary, consumer staples, energy, financials, healthcare, industrials, information technology, materials, and utilites. These sectors are defined based on Global Industry Classification Standard Codes (GICS) codes. ESG momentum is defined as the year-on-year change in ESG ratings. All regressions include all control variables listed in Appendix Table B.1 as well as country-month and industry-month fixed effects. Following Bolton and Kacperczyk (2021), industries are defined based on six-digit GICS codes. We consider four types of ESG ratings: environmental (*E*), social (*S*), governance (*G*), and ESG (*ESG*). We use ratings from four raters: Refinitiv, MSCI IVA, Sustainalytics, and *Composite*. *Composite* combines the available ratings of the other three raters by averaging their ratings. We convert the ratings of each of the other three raters at each point in time to percentile ranks before averaging. Each plot in the figure shows the results of running regressions for a given sector. Each bar represents the regression coefficient on the ESG momentum variable used in a given regression. Confidence intervals are depicted as whiskers around the point estimates. The longer (shorter) whiskers represent 95% (90%) confidence intervals. Standard errors are double clustered at the stock and month levels. The sample period is from January 2001 to December 2020.

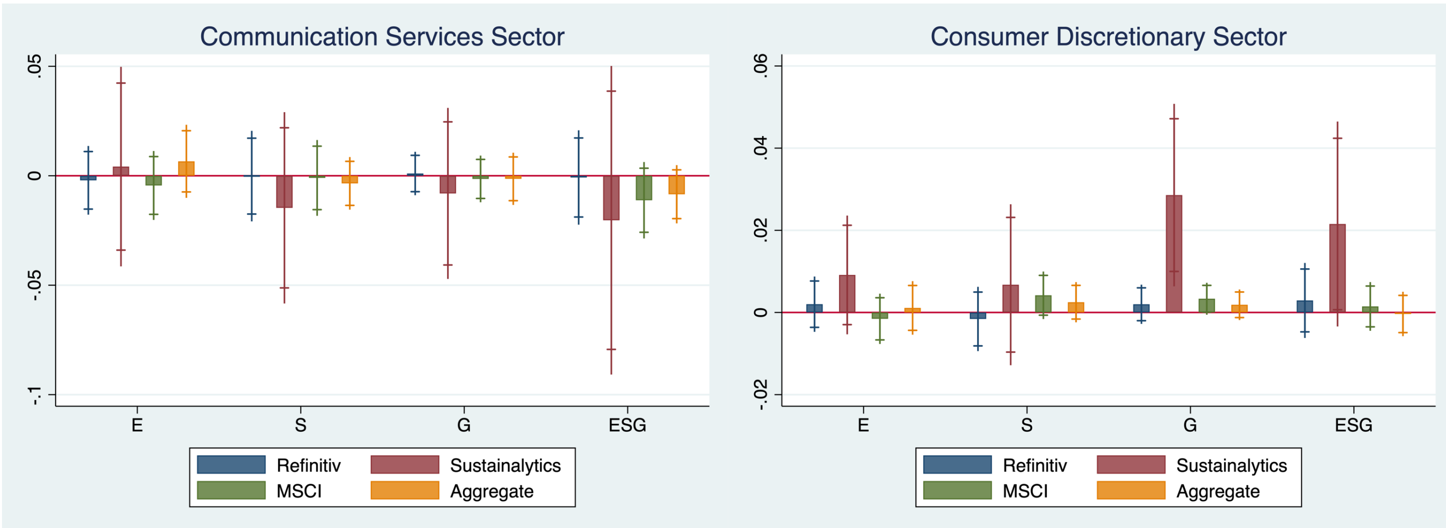

Figure B.5. ESG momentum and stock returns: sectors of economic activity

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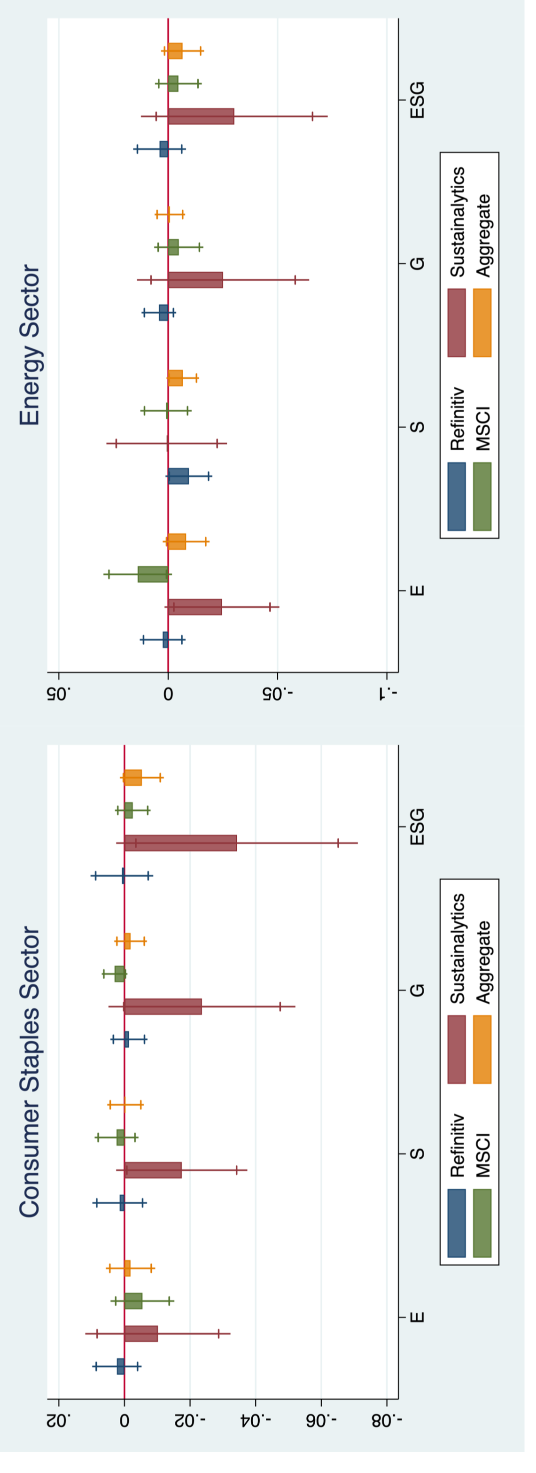

Figure B.5. ESG momentum and stock returns: sectors of economic activity

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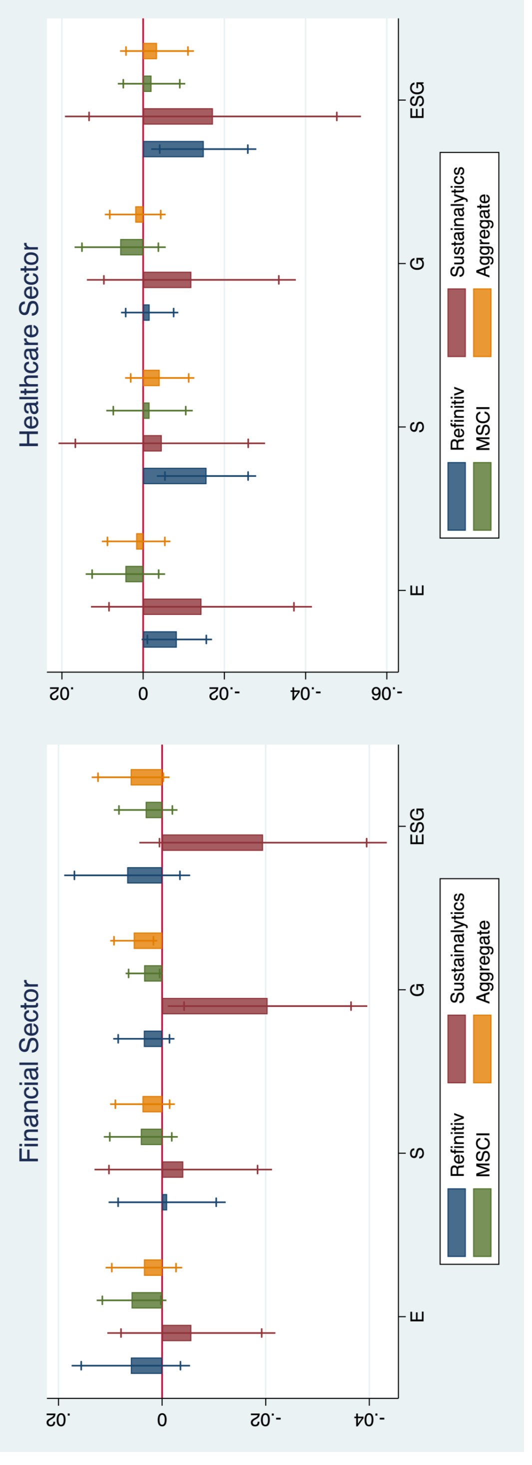

Figure B.5. ESG momentum and stock returns: sectors of economic activity

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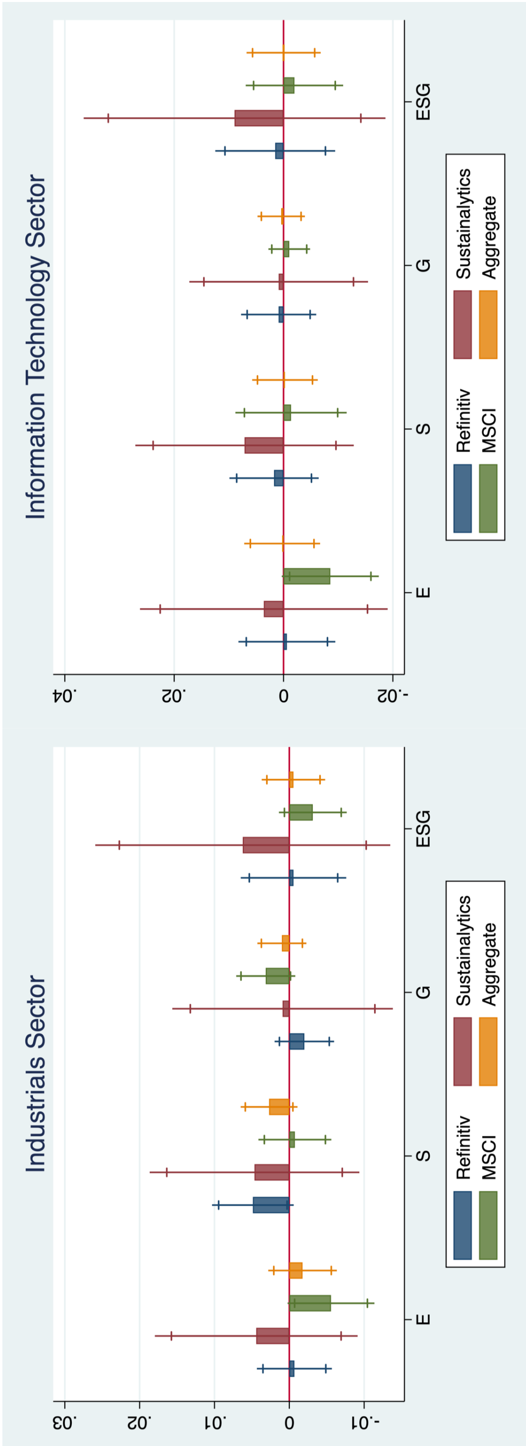
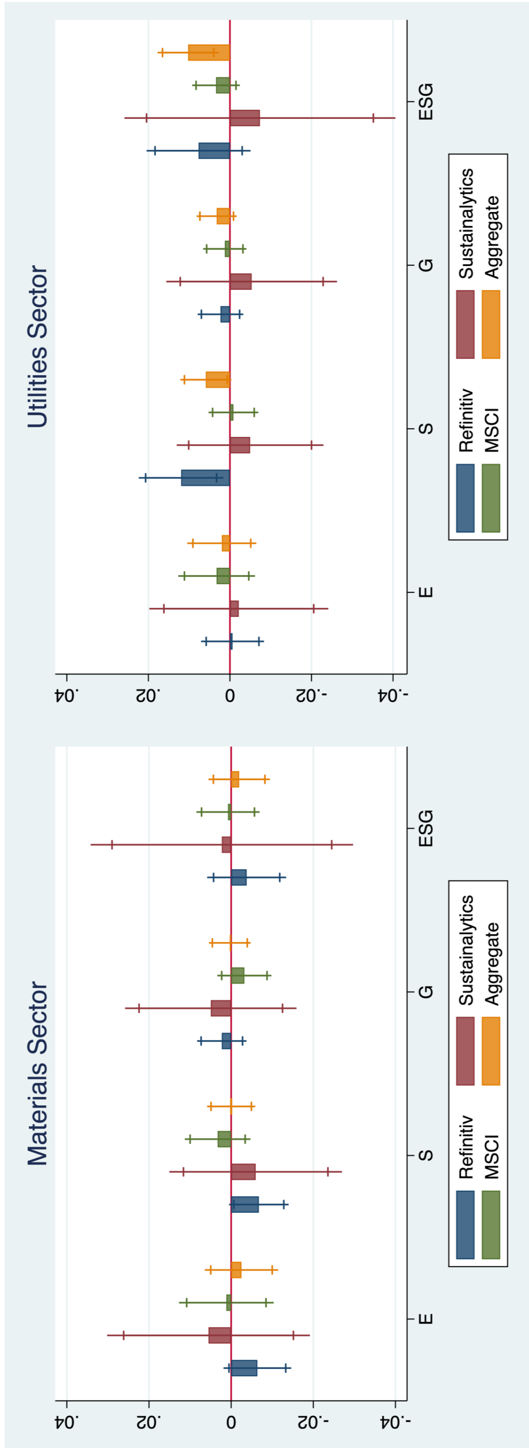


Figure B.5. ESG momentum and stock returns: sectors of economic activity (continued)



Appendix C

Social Networks and Corporate Social Responsibility

C.1. Variable definitions and data sources

Table C.1. Variable definitions and data sources

This table provides the definitions and data sources of the variables used throughout the paper.

Corporate Social Responsibility Variables	
CSR scores	Sum of KLD strengths over the following categories: employee relations, community relations, environment and workforce diversity. The score in each category is normalized by the possible number of strengths for each firm-year. Sourced from the MSCI ESG Stats Database (formerly known as Kinder, Lydenberg and Domini & Co. (KLD)).
Alternative CSR scores	Average of the environmental and social sustainability scores of Thomson Reuters. These scores are based on the sustainability performance of each firm relative to its industry rivals in a given year. Sourced from the Thomson Reuters ESG database.
Firm-Level Control Variables	
Size	Natural logarithm of total assets in millions of dollars (Compustat item AT).
MB Ratio	Market value of equity (Compustat item PRCC_F) divided by the book value of equity (Compustat item BKVLPS).

Table C.1. Variable definitions and data sources

(continued)

Firm-Level Control Variables	
Debt Ratio	Total long-term debt (Compustat item DLTT plus DLC) divided by total assets (Compustat item AT).
ROA	Income before extraordinary items (Compustat item IB) divided by total assets (Compustat item AT).
Net Income	Net income before extraordinary items and discontinued operations in millions of dollars (Compustat item NI).
Cash Ratio	Cash balances (Compustat item CHE) divided by total assets (Compustat item AT).
Dividend Ratio	Cash dividends (Compustat items DVC plus DVP) divided by total assets (Compustat item AT).
Customer Awareness/ Product Differentiation	Cost of advertising media (radio, television, newspapers, periodicals) and promotional expenses (Compustat item XAD) divided by total sales (Compustat item SALE).
R&D	Stock of research and development expenses (Compustat item XRD) computed by capitalizing R&D expenses following the perpetual inventory method of the Bureau of Economic Analysis (Sliker (2007)). Data on the consumer price index is sourced from Global Financial Data.
Institutional Ownership	Fraction of firm stock owned by institutional investors. Sourced from Thomson Reuters.
Peer-Level Control Variables	For each firm-level control variable, there is a corresponding peer-level control variable constructed as a weighted average of the values of the firm-level control variable across all of a firm's social peers, excluding the firm itself. The weights are the normalized strengths of social connections between firm-pairs in a given year.

Table C.1. Variable definitions and data sources

(continued)

Economic Channels Variables	
CEO Delta	Change in the dollar value of the CEO's stock and option portfolio for a one percentage point change in stock price. Sourced from ExecuComp.
CEO Vega	Change in the dollar value of the CEO's stock and option portfolio for a 0.01 change in the annualized standard deviation of stock returns. Sourced from ExecuComp.
Fraction of Independent Directors	Fraction of independent directors on the board. Sourced from Institutional Shareholder Services.
Industry Competition	Herfindahl-Hirschman index computed as the sum of the squared market shares of all firms in a given industry. A firm's market share is computed as the ratio of its sales to total industry sales. The industries are defined based on the TNIC-3 text-based network industry classification of Hoberg and Phillips (2016). Sourced from the Hoberg-Philips data library.
Organ Donation Density	Number of organ donations per capita in each state. Sourced from the Organ Procurement and Transplantation Network (OPTN).
Registered Organization Density	Number of tax-exempt non-profit organizations per capita in each county. Sourced from the National Center for Charitable Statistics (NCCS).
Voter Turnout	Ratio of the number of votes cast in the closest presidential election to the population eligible to vote. Data on votes cast and population eligible to vote are obtained from the American Community Survey and the MIT Election Lab, respectively.

Table C.1. Variable definitions and data sources

(continued)

Economic Channels Variables	
Association Density	Number of non-profit and/or recreational associations per capita in each US county. The following association types are included: civic and social organizations, bowling centers, gold course and country clubs, fitness and recreational centers, sports teams and clubs, religious organizations, political organizations, labor unions and similar labor organizations, business associations, and professional organizations. Sourced from the County Business Patterns (CBP) compiled by the Census Bureau.
Principal Component Index	Principal component of registered organization density, association density and voter turnout. Following Lin and Pursiainen (2018), the principal component is computed for each year separately after standardizing and winsorizing each variable each year at 1% and 99% levels.
Institutional Ownership Concentration Variables	
Inst. Own. HHI Index	Herfindahl-Hirschman index computed as the sum of the squared ownership stakes of institutional investors. Sourced from Thomson Reuters.
Inst. Own. by Largest Five	Fraction of firm stock owned by the largest five institutional investors. Sourced from Thomson Reuters.
Inst. Own. by Blockholders	Fraction of firm stock owned by institutional investors with ownership stakes of at least 5%. Sourced from Thomson Reuters.

Table C.1. Variable definitions and data sources

(continued)

Network Topology Variables	
Degree Centrality	Number of firms a given firm is socially connected with through the social networks of its directors. Social network data is sourced from BoardEx.
Decay Centrality	Network topology variable that measures the extent to which a firm is strategically positioned in the social network to acquire valuable information from its social peers. Refer to Jackson (2008) for technical details. Social network data is sourced from BoardEx.
Diffusion Centrality	Network topology variable that measures the extent to which a firm is strategically positioned in the social network to acquire valuable information from its social peers. Refer to Banerjee et al. (2013) for technical details. Social network data is sourced from BoardEx.
Local Clustering Coefficient	Fraction of pairs of a firm's social peers that are connected to each other. Refer to Watts and Strogatz (1998) for technical details. Social network data is sourced from BoardEx.

C.2. Details on social network construction

To ensure the analysis is free of survivorship bias, I follow a network construction approach similar to that of Engelberg, Gao and Parsons (2013). First, since BoardEx coverage is very limited before 2000, I constrain the sample period to run from 2001 until 2016. Second, BoardEx does not provide CUSIP or ticker symbol information for inactive firms. For these cases, I use the Levenshtein (1966) textual algorithm to match the names of inactive firms to firm names in the Compustat Funda tables. The algorithm is applied several times until no further matches are identified. All matches are manually checked to avoid errors.

Following Fracassi (2017), I define top executives as the top five executives based on compensation data from ExecuComp. Since there is no common individual-level identifier between BoardEx and ExecuComp, I employ the Levenshtein (1966) algorithm to textually match executives names in each firm-year. All the matches are manually checked. One difficulty is that the names in the two databases often come in different formats, in which case the textual algorithm may fail to produce a correct match. For instance, one database might use the nickname Chuck Smith to refer to Charles Smith or Doctor Smith in the other database. It is also frequent that surnames of female executives change or that one of the databases does not clearly distinguish between members of the same family (e.g., James Smith can refer to James Smith Jr. or James Smith Sr.). I resolve these ambiguities by manually searching for name and professional history information on LinkedIn, company websites, SEC reports, Bloomberg executives profiles and news articles.

Since the ExecuComp universe is restricted to the S&P 1500, I cannot identify the top five executives by compensation for all Russell 3000 sample firms. In those cases, I define the top executives to be the CEO, CFO and COO of the firm. My final sample consists of 83,604 individuals based on which I construct 83,604-by-83,604 time-varying networks.

In constructing the education network I follow the general strategy of Engelberg, Gao and Parsons (2013), but with a few differences worth noting. As a first step, I assign each of the several thousands of degree descriptions into 7 categories: (i) undergraduate, (ii) masters and non-research post-graduate degrees, (iii) MBA, (iv) PhD and post-doc, (v) non-research law degrees, (vi) medical degrees, (vii) and other qualifications. Unlike Engelberg, Gao and Parsons (2013), I create a specific category for medical degrees. The motivation is that the facilities where medical students are trained are often geographically separate from those of non-medical students, thus diminishing the chance of meaningful social interaction. I also exclude online programs and short-term certifications that only require a few days or weeks of contact hours. Since many short-term courses are repeated several times within a year for different cohorts, it is often impossible to assign names to cohorts and infer social connections.

In the second step, I map each institution into a unique identifier to correct for the fact that BoardEx assigns different names and abbreviations to the same institution. For instance, I assign KU Leuven to the same identifier as the Catholic University of Leuven. I further account for name changes by checking the history of each institution and I conduct international translations whenever necessary. For example, Arthur D. Little School of Management was renamed Hult Business School in 2003 and Rensselaer's Education for Working Professionals was known as Hartford Graduate Center. The Academie du Droit Internationale de la Haye is assigned the same identifier as Hague Academy of International Law. I also assign university research centers to the respective university campus. For example, the Carolina Center for Genome Sciences is assigned to the University of North Carolina at Chapel Hill, one of the 17 campuses of the University of North Carolina system.

In the third step, I refine the matching by excluding ambiguous cases. For instance, I exclude the Indian Institute of Technology since there are 23 campuses in 23 states across India. However, when information is available elsewhere (e.g., Bloomberg Executives Profile or LinkedIn), I use that information to pin down the specific campus or college where individuals studied. I exclude cases in which the link between an individual and an institution takes the form of a fellowship instead of a degree. I also ignore academic links to professional organizations such as the American Academy of Forensic Sciences. Such institutions do not grant degrees and often have members spread over many states and even countries. In addition, BoardEx does not provide information about whether or not individuals are active in these organizations, making it impossible to define meaningful social connections.

Given the extensive amount of manual matching involved in the construction of the education network, the matching is done twice, once by me and once by a research assistant. I then compare the output of both matches and correct mistakes.

C.3. Detailed description of social capital proxies

I use several proxies for geographic social capital: organ donation, association density, registered organization density, voter turnout, and a modified version of the social capital index of Rupasingha, Goetz and Freshwater (2006).

Organ donation density is the number of organ donations per capita in each state, calculated using data from the Organ Procurement and Transplantation Network (OPTN). The organ donation measure has been used in previous studies in the finance and economics literature as a proxy for social capital (e.g., Guiso, Sapienza and Zingales (2004), Buonanno, Montolio and Vanin (2009) and Hasan et al. (2017b)).

Association density is the number of non-profit and/or recreational associations per capita in each US county, obtained from the County Business Patterns (CBP) compiled by the Census Bureau. The following association types are included: civic and social organizations, bowling centers, golf courses and country clubs, fitness and recreational centers, sports teams and clubs, religious organizations, political organizations, labor unions and similar labor organizations, business associations and professional organizations. Registered organization density is the number of tax-exempt non-profit organizations per capita in each county, sourced from the National Center for Charitable Statistics (NCCS). Voter turnout is the ratio of the number of votes cast in the closest presidential election to the population eligible to vote. Data on votes cast and population eligible to vote are obtained from the American Community Survey and the MIT Election Lab, respectively.

These three measures build on the work of Putnam (2000) in the sociology literature and were introduced in the economics literature by Rupasingha, Goetz and Freshwater (2006) in the form of a social capital index. Since then, this index has dominated the empirical literature on social capital (e.g., Hasan et al. (2017a), Hasan et al. (2017b), Lin and Pursiainen (2018)). The social capital index is usually constructed as the first principal component of these three measures and the county-level response rate to the Census Bureau's decennial census.

I deviate from the index construction methodology of Rupasingha, Goetz and Freshwater (2006) because, as explained in detail in Lin and Pursiainen (2018), the index comes with several methodological shortcomings. These include the variables being contaminated by significant outliers (e.g., voting rates higher than 100%) and the index not being available on a yearly basis (thus requiring data extrapolation across years). Following Lin and Pursiainen (2018), I deal with these issues in two ways. First, I do not use census response rates data to eliminate the need to extrapolate data across years. Instead, I construct the index as the first principal component of association density, registered organization density and voter turnout. Second, I winsorize each variable at the 1% and 99% levels within each year to remove the influence of extreme observations. In a final step, I standardize the variables within each year to capture cross-sectional differences in social capital as opposed to time trends and compute the within-year first principal component of the three variables.

It is also worth noting that I deviate slightly from the literature in that I use both the principal component and the three individual variables as alternative proxies for social capital. Two reasons justify proceeding in this way. First, I allow for the fact that these measures capture different dimensions of social capital. Regional and organizational density capture the frequency of social interactions and proxy for the existence of dense networks that enforce cooperation (Putnam (2000)). Voter turnout and organ donation, however, are measures of civic engagement (Scrivens and Smith (2013)).[45] There is, to the best of my understanding, little motivation to ignore the possibility that different dimensions of social capital matter in different settings. Second, the measure of organizational density has received some criticism as it ignores the emergence of new forms of organizations and technologies that sustain interpersonal networks (e.g., Sobel (2002)). Therefore, variation in these measures may reflect a substitution between types of organizations instead of actual changes in social capital. Hence, given these concerns, using a principal component methodology may mask important dynamics. Nevertheless, I show that the results are robust to using the principal component approach.

As in the extant literature, I obtain firm-specific measures of social capital by assigning county-level measures of social capital to each firm based on the county where the firm is headquartered. A drawback of this measure is that it does not account for the fact that many of a firms' social peers are headquartered in a different county. Therefore, it may be a poor proxy for the amount of social capital in a firms' social network. To account for this possibility, I also create a measure of firm-specific local network social capital (as opposed

[45]Refer to Scrivens and Smith (2013) for an in-depth discussion on the different dimensions of social capital and corresponding empirical measures.

to own social capital) by averaging the social capital of all the peers of a given firm, including the firm itself.

C.4. Network summary statistics

The histogram in Figure C.1 depicts the distribution of the clustering coefficient of Watts and Strogatz (1998). This coefficient measures how close-knit the local network around each firm is. A score of one (zero) occurs when all (none) of a firms' connections are connected to one another. We observe that 25% of the firms exhibit high clustering coefficients above 0.5. To put this in perspective note that, if the network was generated by a Erdős and Rényi (1959) random graph model, the expected clustering coefficient would be 15 times smaller than the observed mean coefficient of 0.4.[46] This suggests that the structure of corporate social networks may be able to sustain information exchange and social norms of behavior within close-knit groups through schemes of reward and punishment. This is the case because locally dense networks allow social peers to jointly punish those who fail to cooperate, thus making deviations costlier (e.g., Karlan et al. (2009), Lippert and Spagnolo (2011)).

Figure C.2, left panel, compares the spatial correlation of CSR scores among firms that are at different distances from each other in the social network.[47] To remove trend effects, I compute the statistic separately for each year and then average across years. I also exclude social connections among firms in the same one-digit SIC division to remove industry peer effects. We observe a strong

[46] The Erdős and Rényi (1959) random graph model is a model that generates a random network by randomly connecting pairs of nodes with some prespecified probability. The expected value of the local clustering coefficient for the Erdős and Rényi (1959) random graph is equal to the probability that any two given nodes are connected. Hence, computing the sample probability that any two given nodes are connected enables me to compute the expected value of the clustering coefficient under the assumption that the network is generated by the Erdős and Rényi (1959) random graph model.

[47] This corresponds to Moran's I statistic, a measure of spatial correlation for nodes located at different distances in the network and that takes into account the strength of connections (Kelejian and Prucha (2001)).

positive correlation amongst directly connected firms. Most striking is that the cross-firm commonality in CSR completely vanishes once we consider indirect social peers. This result is reassuring in that it is inconsistent with the existence of strong unobservable common shocks that could bias the results. If omitted common shocks would completely drive the spatial correlation, we would expect to find stronger positive correlations between the CSR policies of firms that are not directly connected. Moreover, the fact that correlations are high and low exactly when they should be also suggests that the social networks are able to capture meaningful social links despite involving substantial aggregation of individual-level social networks.

A related question is whether or not the absence of strong positive corre-

Figure C.1. Local clustering coefficient distribution

This figure shows the distribution of the local clustering coefficient of Watts and Strogatz (1998) for the corporate social network in 2009 (the median year in the sample). The local clustering coefficient for a given firm is defined as the number of connections among the social peers of that firm divided by the number of possible connections. It takes value one (zero) if all (none) of a firm's connections are connected to each other.

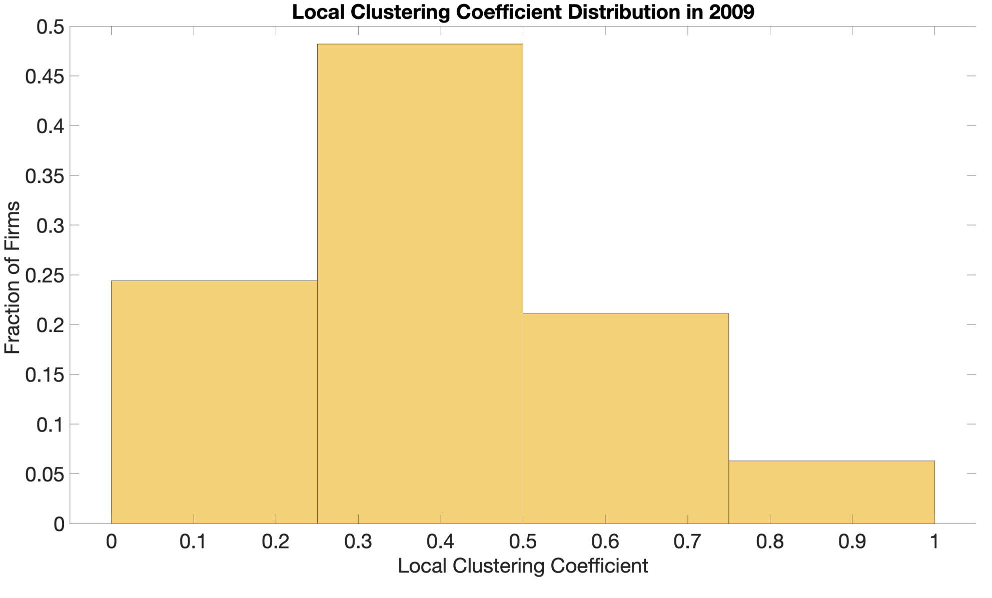
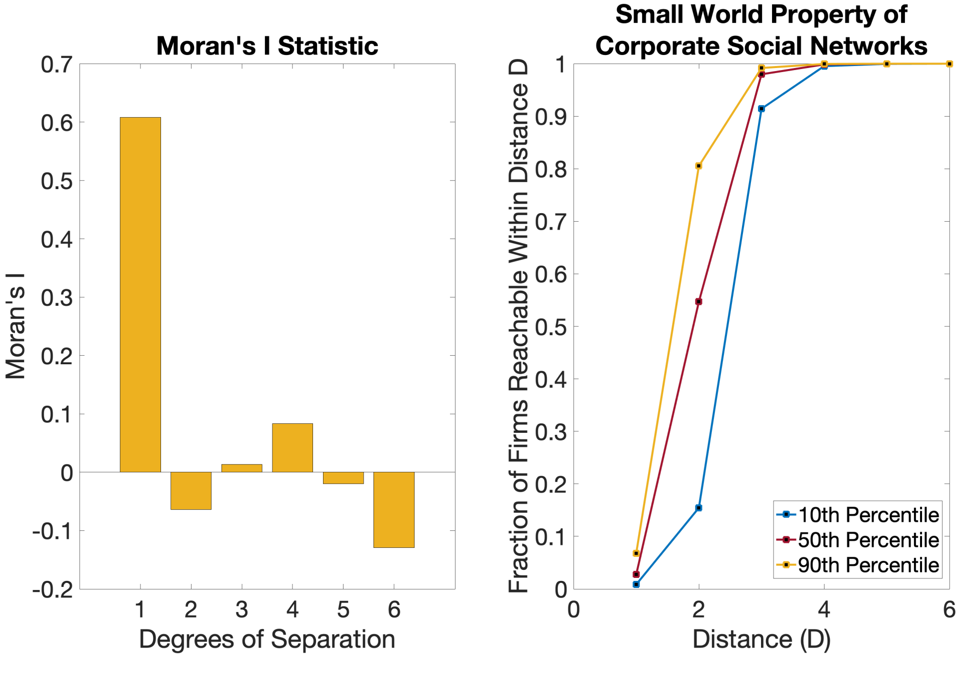


Figure C.2. Spatial cross-correlation of CSR scores and small world property of corporate social networks

The histogram on the left shows the spatial cross-correlation (Moran's I statistic) of CSR scores of socially connected firms for various degrees of separation between 2001 and 2016. To control for time trends in CSR scores, Moran's I statistic is first computed separately for each year and then averaged over the time dimension. To remove industry peer effects, I only consider social peers in different one-digit SIC industries. The plot on the right depicts the 10th, 50th and 90th percentiles of the cross-firm distribution of the fraction of firms that are within a given distance of each sample firm in the corporate social network. The fractions are computed separately for each firm-year-distance combination and then aggregated across firms over the period 2001-2016 for each distance.



lations between CSR policies of indirectly linked firms is consistent with firms bridging information selectively between their social peers (consistent with information selection effort and information being valuable). Indeed, the observed pattern could simply be a mechanical artifact of a sparse network. In sparse networks, the average shortest path length across all pairs of nodes is large and, therefore, it is difficult for information originating in a given node to percolate across the network. In denser networks with a few highly connected hubs, however, information originating in a given node in the network is likely to quickly reach a highly connected hub which can then spread the information to many other nodes (e.g., Pastor-Satorras and Vespignani (2001), Barthélemy et al. (2004), Acemoglu, Bimpikis and Ozdaglar (2014), Rantala (2019)).

To provide some insight into this question, I plot the 10th, 50th and 90th percentiles of the cross-firm distribution of the fraction of firms that are within a given distance of each sample firm, averaged over the period 2001-2016. The results are shown in the right panel of Figure C.2. Consistent with the idea that social networks are small worlds in which everyone is just a few hops away from everyone else (e.g., Milgram (1967), Barabási (2003)), most firms can reach the entire network within 3 steps. More precisely, half of the firms can reach at least half of the network within 2 steps and 90% of the firms can reach at least 90% of the entire network within 3 steps. Overall, and with the caveat of being purely descriptive, this evidence suggests that the architecture of US corporate social networks is able to sustain the exchange of valuable information on CSR.

C.5. Network placebo tests

The main challenge underlying the empirical setting of the paper is to separate endogenous peer effects from correlated effects. While the results from other robustness tests and the partial identification strategy suggest that correlated effects play a limited role, I conduct a stricter test of this statement by forming placebo peer groups. If the results are driven by latent common factors, we would expect to find that the average CSR of a firm's social peers is systematically related to the CSR decisions of other firms.

I construct placebo peer groups by randomly matching each firm's social and indirect peer groups to another firm each year. A welcome feature of this approach is that it preserves the specific latent common factors captured by the average CSR score of social peers that potentially bias the results. The matching process is repeated 1,000 times, both with and without replacement. The regressions control for state-of-incorporation-by-year fixed effects, industry-by-year fixed effects, CSA-by-year fixed effects and all firm-level and peer-level control variables used throughout the paper, including the additional controls customer awareness and R&D. The results reported in Appendix Table C.2 show that the t-statistics and associated coefficient estimates obtained in the non-placebo regression (3) in Table 4.2 occur in fewer than 1% of the placebo simulations. Moreover, the mean and median values of estimated placebo peer effects are zero. This suggests that the results are unlikely driven by latent common factors. Plots with the full distribution of placebo coefficients and t-statistics are shown in Appendix Figure C.3.

C.6. Tests of endogenous sorting into networks

An additional concern is that the results are driven by endogenous sorting into networks. For instance, Giuli and Kostovetsky (2014) document that firms with Democrat directors and CEOs invest less in CSR compared to Republican-leaning firms. If Democrats are more likely to be socially connected to Democrats than to Republicans, peer effects may be an artifact of common preferences across socially linked firms. To alleviate this concern, I use a community detection algorithm, the Louvain algorithm (Blondel et al. (2008)), to partition the social networks into densely connected communities of

Table C.2. Falsification tests

This table shows the mean and percentiles of the distribution of placebo peer effects based on 1,000 runs of model (3) in Table 4.2. In each run, each sample firm-year is randomly matched with another firm's social and indirect peer groups in that year. Results are shown for the cases of random matching with and without replacement. Social peers are defined based on the social networks of executives and directors. The instrument is the average CSR score of indirect placebo peers. A firm's indirect placebo peers are defined as the three-digit SIC industry peers of the social peers of a randomly selected firm subject to the restrictions that the indirect peers and that firm: (i) operate in different industries; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). All regressions include all firm-level and peer-level control variables described in C.1. Each peer-level control variable is computed as a weighted average of that variable across a firm's real (non-placebo) social peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The coefficients are measured in standard deviation units. All regressions include state-of-incorporation-by-year fixed effects, CSA-by-year fixed effects and industry-by-year fixed effects. Standard errors are heteroskedasticity-robust and clustered at the firm-level.

	Without Replacement		With Replacement	
	Coefficient	t-statistic	Coefficient	t-statistic
Percentile 1%	-0.171	-2.187	-0.157	-2.249
Percentile 5%	-0.130	-1.630	-0.108	-1.589
Percentile 10%	-0.101	-1.318	-0.086	-1.255
Percentile 50%	0.001	0.004	0.002	0.023
Percentile 90%	0.096	1.247	0.086	1.207
Percentile 95%	0.128	1.623	0.113	1.640
Percentile 99%	0.177	2.142	0.156	2.201
Mean	-0.001	-0.018	0.002	0.021

firms. This makes it possible to employ different types of network community-by-year fixed effects that vary with average community size and to cluster standard errors to account for within-community dependence. The results are shown in C.3. Depending on the size of the communities, the magnitude of the peer effects and associated t-statistics are either very similar or slightly larger than the baseline estimates in Table 4.2. The stability of the estimates is, therefore, consistent with the notion that the instrumental variable is orthogonal to the omitted variables that simultaneously influence firm network formation and CSR investment decisions.

Figure C.3. Distribution of placebo CSR peer effect estimates

These histograms show the distribution of placebo CSR peer effect estimates and associated t-statistics obtained from 1000 runs of model (3) in Table 4.2. In each run, the social and indirect peer groups of each firm are randomly matched to a firm that is active in that year. The top plots are obtained from simulations with random matching without replacement. The bottom plots are obtained from simulations with random matching with replacement. The red lines indicate the location of the real (non-placebo) estimates.

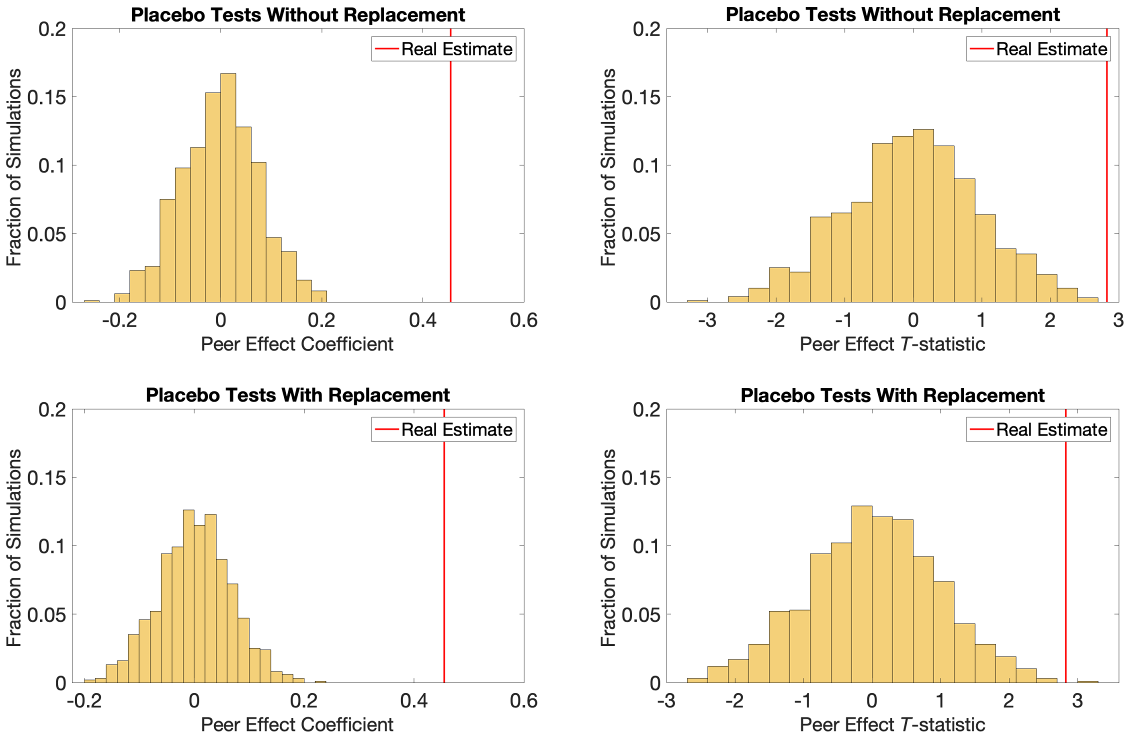


Table C.3. Tests of endogenous sorting into networks

This table reports the output of two-stage least squares (2SLS) regressions of firm CSR scores on social peers' CSR scores when controlling for endogenous sorting into networks. Social peers are defined based on the social networks of executives and directors. The Louvain algorithm (Blondel et al. (2008)) is used to partition the social network each year into communities of different sizes: small, intermediate and large. Communities are defined as sets of firms that are highly connected among themselves and sparsely connected to firms outside the community. Each size category corresponds to one of the three algorithm recursions needed for convergence. These communities are used to cluster standard errors and define community-by-year fixed effects. The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) operate in different industries; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The additional controls are customer awareness and R&D investment. The coefficients are measured in standard deviation units. The Kleiberg-Paap F-stat is the cluster-robust Kleibergen and Paap (2006) F-statistic for weak instruments. t-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at either the firm-level or double clustered at the firm-level and community-by-year level. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

	Small Network Communities (1)	Intermediate Network Communities (2)	Large Network Communities (3)	Small Network Communities (4)	Intermediate Network Communities (5)	Large Network Communities (6)
Peers' CSR	0.539***	0.469***	0.455***	0.539***	0.469**	0.455**
	(3.078)	(2.871)	(2.827)	(3.009)	(2.594)	(2.557)
Kleiberg-Paap F-stat	63.373***	66.228***	66.855***	55.006***	55.803***	46.294***
First Stage Instrument	0.208***	0.207***	0.209***	0.208***	0.207***	0.209***
	(7.960)	(8.140)	(8.180)	(7.420)	(7.470)	(6.800)
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Community-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes	Yes
Firm Clustered SEs	Yes	Yes	Yes	Yes	Yes	Yes
Community Clustered SEs	No	No	No	Yes	Yes	Yes
No. Community-Years	905	62	18	905	62	18
No. Obs.	25,741	25,808	25,808	25,741	25,808	25,808

C.7. Evidence from deaths of executives and directors

This quasi-natural experiment mirrors the one in Fracassi (2017) and proceeds in two stages. In a first stage I regress firm-level CSR scores on the set of control variables and high-dimensional fixed effects used throughout the paper:

$$y_{ijklt} = \alpha + \lambda' \bar{X}_{-ijklt} + \gamma' X_{ijklt} + \mu_{jt} + \delta_{kt} + \zeta_{lt} + \epsilon_{ijklt}, \tag{C.1}$$

where the unit of observation is a firm *i* in year *t*, operating in industry *j*, headquartered in CSA region *k* and incorporated in state *l*. The dependent variable y_{ijklt} is the CSR score of firm *i*. The model further controls for the average characteristics of firm i's peer group ($\bar{X}_{-ijklt}$), its own characteristics (X_{ijklt}) and a set of three-digit SIC industry-by-year (μ_{jt}), CSA-by-year (δ_{kt}) and state-of-incorporation-by-year (ζ_{lt}) fixed effects.

The residuals of this regression capture the idiosyncratic component of firms' CSR policy that is orthogonal to time-varying common shocks and firm-specific observables. I then define a measure of idiosyncratic CSR comovement for each firm-pair-year as the absolute value of the difference between the idiosyncratic CSR policies of that firm-pair in that year:

$$\tilde{y}_{a,b,t} \stackrel{\text{def}}{=} |\epsilon_{a,t} - \epsilon_{b,t}|, \tag{C.2}$$

where the pair *(a,b)* indexes a firm-pair. The larger the value of this measure, the less the CSR policies of the firms comove. A value of zero indicates perfect comovement.

In a second stage I collect all firm-pairs that experience a death during the sample period. There are 4,263 deaths in total. Thirty-seven percent of firm-pairs experience a death and 3.5% of those experience a death that was connecting the firm-pair. Using this sample, I run the following diff-in-diff regression:

$$\tilde{y}_{a,b,t} = \alpha + \beta_1 Death_{a,b,t} + \beta_2 Death_{a,b,t} \times Connected_{a,b,t} + \phi' W_{a,b,t} + \tau_{a,b} + \omega_t + \eta_{a,b,t} \tag{C.3}$$

In words, I regress the idiosyncratic CSR comovement measure on a dummy variable (*Death*) that equals one after a death event occurs and on the interaction between this variable and another dummy variable (*Connected*) that equals one if the firm-pair is in the treatment group. The treatment group is composed of firm-pairs for which at least one deceased individual was connecting the firm-pair. The control group consists of the remaining firm-pairs for which the deceased were not connecting the pair. I also include firm-pair fixed effects ($\tau_{a,b}$), year fixed effects (ω_t) and firm-pair control variables ($W_{a,b,t}$). The control variables are the pairwise sums and the absolute values of the pairwise differences of all firm-level control variables listed in C.1. I exclude firm-pairs in which the deceased worked in both firms simultaneously. This rules out the possibility that comovement is driven by the preferences of one individual instead of being driven by social interactions. Standard errors are clustered at the firm-pair level.

I present the results in C.4. Columns (1), (3) and (5) show the results using the network of directors, executives and both, respectively. The remaining columns (2), (4) and (6) add firm-pair control variables. The results indicate that the death of a director connecting a firm-pair leads to a decrease in idiosyncratic CSR comovement relative to the death of a director that is not connecting the firm-pair. There is, however, no evidence that the death of an executive connecting a firm-pair impacts CSR comovement more than the death of an executive not connecting the firm-pair. Overall, this is consistent with the results of the IV analysis.[48]

[48]Note that the effect of the death of an individual not connecting a firm-pair is to increase

Table C.4. Quasi-experimental evidence from the deaths of directors and executives

This table reports the output of a difference-in-differences regression measuring the impact of the deaths of directors and executives socially connecting two firms on the comovement of CSR scores of those firms. The dependent variable is a measure of idiosyncratic CSR comovement at the firm-pair-year level. Idiosyncratic CSR scores are computed as the residuals of a regression of firm-level CSR scores on CSA-by-year, three-digit SIC industry-by-year and state-of-incorporation-by-year fixed effects as well as the full set of firm-level and peer-level controls described in C.1. Idiosyncratic CSR comovement for a given firm-pair-year is defined as the absolute value of the difference in idiosyncratic CSR scores for that firm-pair in that year. *Death* is equal to one in the period after the death of the individual and zero before. *Connected* is equal to one if the deceased was socially connecting the firm-pair. The controls in columns (2), (4) and (6) are the pairwise sums and the absolute values of the pairwise differences of all firm-level control variables listed in C.1. All regressions control for firm-pair fixed effects and year fixed effects. All non-categorical variables are standardized. *t*-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-pair level. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

	Directors Network		Executives Network		Aggregate Network	
	(1)	(2)	(3)	(4)	(5)	(6)
Death	-0.015***	-0.019***	-0.026*	-0.034**	-0.020***	-0.024***
	(-3.416)	(-4.467)	(-1.773)	(-2.336)	(-4.879)	(-5.852)
Death × Connected	0.027**	0.024**	-0.009	-0.001	0.027**	0.024**
	(2.268)	(2.035)	(-0.182)	(-0.026)	(2.399)	(2.131)
Peers' Size Diff.		0.093***		0.024		0.013***
		(10.519)		(1.266)		(2.701)
Peers' MB Ratio Diff.		0.008***		0.023***		0.017***
		(7.211)		(3.772)		(11.058)
Peers' Debt Ratio Diff.		-0.006*		-0.007		0.003
		(-1.767)		(-0.886)		(1.399)
Peers' ROA Diff.		0.006***		0.022***		0.011***
		(2.681)		(3.091)		(6.128)
Peers' Net Income Diff.		0.005*		0.003		0.007**
		(1.663)		(0.385)		(2.418)
Peers' Cash Ratio Diff.		-0.002		-0.043***		-0.018***
		(-0.498)		(-4.605)		(-6.817)
Peers' Divid. Ratio Diff.		-0.024***		0.004		0.009**
		(-5.134)		(0.273)		(2.516)
Peers' R&D Diff.		0.057**		-0.168**		-0.127***
		(2.405)		(-2.314)		(-5.332)
Peers' Inst. Own. Diff.		-0.012***		0.014***		0.004**
		(-6.872)		(2.607)		(2.416)
Peers' Cust. Awa. Diff.		0.028***		-0.019		-0.015*
		(2.952)		(-0.633)		(-1.841)
R^2	0.450	0.450	0.447	0.448	0.450	0.451
No. Obs.	851,102	851,102	74,049	74,049	865,883	865,883

In C.5 below I show the results are robust to (i) allowing the treatment effect to be a function of the number of deaths involving a given firm-pair, and (ii) excluding firm pairs in the same one-digit SIC industry.

C.8. Evidence from close-call CSR proposals

In this section I use a regression discontinuity design to examine the response of firms' CSR decisions to the passage of close-call shareholder-sponsored CSR proposals by their social peers. The identifying assumption is that whether the social peers of a firm pass or fail CSR proposals around the pass threshold (e.g., 51% versus 49% of the votes) is as good as random with respect to the other determinants of that firm's CSR. This seems reasonable in light of the fact that previous literature using CSR proposals in a similar setting has consistently failed to find evidence for manipulation (e.g., Flammer (2015a), Cao, Liang and Zhan (2019), Dai, Liang and Ng (2020)). Under this assumption, I estimate the causal peer effects of CSR by comparing the CSR outcomes of firms whose peers failed to pass CSR proposals by a small number of votes with the CSR outcomes of firms whose peers barely passed CSR proposals. The estimation is done using nonparametric local linear regression and the Imbens and Kalyanaraman (2012) optimal bandwidth selection method.[49]

I test for violations of the identifying assumption in two ways. First, I confirm that there is no evidence of a discontinuity around the approval threshold using the non-parametric density estimator test of Cattaneo, Jansson and Ma (2020). The p-value of the test is 0.28. The method of Cattaneo, Jansson and

comovement in CSR policies. This is consistent with the results of Fracassi (2017) who also finds a similar effect on the comovement of firms' investment spending. A plausible reason for this is that the changes in leadership that follow deaths lead to the adoption of less idiosyncratic investment policies. Refer to Fracassi (2017) for a discussion of this.

[49] In a previous version of the paper I used global polynomial regression. The switch to local linear regression methods is motivated by the desire to avoid the shortcomings of global polynomial methods discussed in Gelman and Imbens (2019).

Table C.5. Quasi-experimental evidence from the deaths of directors and executives: robustness tests

This table reports the output of a difference-in-differences regression measuring the impact of the deaths of directors and executives socially connecting two firms on the comovement of CSR scores of those firms. The sample is restricted to firm-pairs not belonging to the same three-digit SIC industry. The dependent variable is a measure of idiosyncratic CSR comovement at the firm-pair-year level. Idiosyncratic CSR scores are computed as the residuals of a regression of firm-level CSR scores on CSA-by-year, three-digit SIC industry-by-year and state-of-incorporation-by-year fixed effects as well as the full set of firm-level and peer-level controls described in C.1. Idiosyncratic CSR comovement for a given firm-pair-year is defined as the absolute value of the difference in idiosyncratic CSR scores for that firm-pair in that year. *Death* is equal to one in the period after the death of the individual and zero before. In the binary treatment, *Connected* is equal to one if the deceased was socially connecting the firm-pair. In the continuous treatment, *Connected* is the number of deceased individuals socially connecting the firm-pair. The controls in columns (1) through (6) are the pairwise sums and the absolute values of the pairwise differences of all firm-level control variables listed in C.1. All regressions control for firm-pair fixed effects and year fixed effects. All non-categorical variables are standardized. t-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-pair level. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

	Directors Network		Executives Network		Aggregate Network	
	Binary Treatment (1)	Continuous Treatment (2)	Binary Treatment (3)	Continuous Treatment (4)	Binary Treatment (5)	Continuous Treatment (6)
Death	-0.015***	-0.015***	-0.042***	-0.043***	-0.021***	-0.021***
	(-3.456)	(-3.496)	(-2.807)	(-2.821)	(-4.957)	(-4.967)
Death × Connected	0.025**	0.024**	0.005	0.008	0.024**	0.022**
	(2.015)	(2.237)	(0.100)	(0.162)	(2.063)	(2.150)
Peers' Size Diff.	0.092***	0.092***	0.023	0.023	0.013***	0.013***
	(10.157)	(10.149)	(1.144)	(1.143)	(2.643)	(2.643)
Peers' MB Rat. Diff.	0.008***	0.008***	0.022***	0.022***	0.017***	0.017***
	(6.894)	(6.894)	(3.451)	(3.450)	(10.463)	(10.463)
Peers' Debt Rat. Diff.	-0.005	-0.005	-0.010	-0.010	0.003	0.003
	(-1.411)	(-1.405)	(-1.184)	(-1.184)	(1.176)	(1.171)
Peers' ROA Diff.	0.007***	0.007***	0.025***	0.025***	0.012***	0.012***
	(2.995)	(2.996)	(3.341)	(3.340)	(6.511)	(6.510)
Peers' Net Inc. Diff.	0.004	0.004	0.002	0.002	0.006**	0.006**
	(1.282)	(1.280)	(0.247)	(0.247)	(2.061)	(2.059)
Peers' Cash Rat. Diff.	-0.002	-0.002	-0.041***	-0.041***	-0.018***	-0.018***
	(-0.364)	(-0.367)	(-4.179)	(-4.180)	(-6.586)	(-6.585)
Peers' Divid. Rat. Diff.	-0.025***	-0.025***	0.003	0.003	0.010***	0.010***
	(-5.310)	(-5.320)	(0.213)	(0.215)	(2.782)	(2.789)
Peers' R&D Diff.	0.061**	0.061**	-0.162**	-0.162**	-0.137***	-0.137***
	(2.411)	(2.408)	(-1.986)	(-1.987)	(-5.363)	(-5.362)
Peers' Inst. Own. Diff.	-0.012***	-0.012***	0.012**	0.012**	0.004**	0.004**
	(-6.703)	(-6.706)	(2.166)	(2.166)	(2.262)	(2.260)
Peers' Cust. Awa. Diff.	0.026***	0.026***	-0.009	-0.009	-0.015*	-0.015*
	(2.738)	(2.739)	(-0.285)	(-0.285)	(-1.777)	(-1.779)
R^2	0.451	0.451	0.451	0.451	0.452	0.452
No. Obs.	819,908	819,908	69,362	69,362	834,005	834,005

Ma (2020) improves on the test of McCrary (2008) by providing robust bias-corrected confidence intervals and by overcoming the need to pre-bin the data, thus yielding more statistical power. Second, I show in C.6 that there is little evidence of strong pre-treatment differences in firm attributes within narrow voting windows of 5% or less around the pass threshold. I also find, however, that there is evidence for pre-treatment differences in four firm attributes once I consider a voting window of 10%. While this does not necessarily invalidate the design, I report the results with and without controls to mitigate potential selection biases.

I construct treatment and control groups as follows. A non-voting firm-year is assigned to the treatment (control) group if at least one social peer proposal passes (fails) in the previous year. Therefore a firm is either in the control or the treatment group. Furthermore, as in Cuñat, Gine and Guadalupe (2012) and Flammer (2015a), I aggregate the votes of all the peer proposals associated with a given non-voting firm in a given year as follows. If the firm is in the treatment group, I sum the distances to the pass threshold across all peer proposals that pass. Similarly, if a firm is in the control group, I sum across all failed proposals. This ensures that observations will only lie within a short-window of the threshold if all the peer proposals that pass or fail are individually close to threshold. In addition, if the peers of a firm vote on proposals in year t, I only include the firm in the analysis if the firm did not pass any proposals itself in the period ranging from year t-2 until t+1. This ensures that I do not spuriously attribute peer effects to firms passing proposals around the same time their peers happen to be voting on their own proposals. I also exclude social peers in the

Table C.6. Regression discontinuity design tests of pre-treatment differences in treatment and control groups

This table tests the validity of the regression discontinuity design by comparing whether or not treated and control firms are fundamentally different in terms of several attributes. The attributes are measured in the year prior to the vote date. The composition of treatment and control groups is defined in terms of different bandwidths around the voting threshold: 1%, 2.5%, 5% and 10%. The table reports the difference in means between treatment and control groups for each attribute as well as the t-statistic associated with the test of the null hypothesis that the difference in means is zero. To account for multiple hypothesis testing, the table further reports the number of attributes for which the null hypothesis is rejected using the Benjamini and Hochberg (1995) Procedure. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

	Bandwidth Around Threshold							
	1%		2.5%		5%		10%	
	Coeff	t-stat	Coeff	t-stat	Coeff	t-stat	Coeff	t-stat
Firm Attributes								
Size	-0.028	-0.054	-0.508	-1.158	-0.733	-3.245	-0.968	-7.106
MB Ratio	-0.508	-0.661	-0.348	-0.607	-0.293	-1.017	-0.093	-0.579
Debt	-0.512	-0.929	-0.194	-0.445	0.210	0.917	-0.061	-0.485
ROA	-0.294	-0.747	-0.353	-0.851	-0.556	-2.501	-0.314	-2.748
Income	-0.447	-0.712	-0.515	-0.903	-0.466	-1.623	-0.685	-3.436
Cash	-0.148	-0.408	0.401	1.264	-0.088	-0.504	-0.033	-0.358
Dividend	-0.771	-1.031	-0.747	-1.434	-0.382	-1.430	-0.228	-1.518
R&D	-0.314	-0.437	-0.401	-0.624	-0.372	-1.119	-0.578	-2.510
Inst. Own.	0.210	0.400	0.376	0.901	0.465	2.066	0.223	1.707
Cust. Awa.	-0.378	-0.591	-0.088	-0.183	0.044	0.179	-0.060	-0.456
No. Obs.	142		350		443		705	
Benjamini-Hochberg Procedure								
No. rejections assuming:								
False Discovery Rate = 10%	0		0		2		4	
False Discovery Rate = 5%	0		0		1		4	

same three-digit SIC industry to abstract from industry peer effects.[50]

This leads to a total of 25,648 non-voting firm-years. However, it is important to stress that only 14 proposals, out of a total of 3,010 proposals, pass within the 10% threshold. The small number of passing proposals is consistent with previous literature (e.g., Flammer (2015a)). Hence, while this strategy in principle scores high on internal validity, its external validity is not guaranteed.

I report the results in Panel A of C.7 using both triangular and rectagular kernels. I present results with control variables (columns (2) and (4)) and without (columns (1) and (3)). In addition, the results in columns (3) and (4) are obtained using only variation in CSR scores that is orthogonal to three-digit SIC industry-by-year fixed effects. These orthogonalized CSR scores are obtained through a first stage regression as the residuals of an ordinary least squares regression of CSR scores on industry-by-year fixed effects. To mitigate selection biases, I use all firms used in the IV analysis in this first stage regression irrespective of whether or not they (or their peers) vote for proposals. This orthogonalization step is important because it alleviates the concern that social peer effect estimates are picking up industry peer effects.

The most conservative estimates in columns (2) and (4) indicate that firms increase their CSR scores by 20% to 30% of a standard deviation in response to their social peers passing CSR proposals. In comparison, Cao, Liang and Zhan (2019) find that firms increase their CSR scores by 30% of a standard deviation in response to their industry peers passing CSR proposals. Hence, the

[50] An alternative approach in the literature (Cao, Liang and Zhan (2019) and Dai, Liang and Ng (2020)) consists of stacking all pairs of voting and non-voting firms together in a regression discontinuity design framework. My method has two desirable features compared to the stacking approach. First, the running variable deterministically assigns observations to either the treatment or control group. Second, the effect of proposals far away from the threshold is separated from the effect of proposals close to the threshold. Suppose a firm is associated with two proposals j_1 and j_2 that pass with 5% and 30%, respectively. Further assume that the firm responds to j_2 but not j_1. In this scenario, the estimate obtained via stacking will attribute the (non-causal) effects of proposal j_2 far away from the threshold to proposal j_1 close to the threshold.

economic magnitudes of social and industry peers effects of CSR are comparable.

A major concern with this approach is the possibility that the results depend on the choice of how proposals are aggregated. To alleviate this concern, I define an alternative aggregation rule (henceforth denoted by *MaxMin* aggregation method to distinguish it from the previous aggregation method). Instead of summing across proposal votes to define the running variable, I define the running variable based on the most extreme outcomes of peer proposal votes. If the firm is in the treatment group, I set the running variable to be equal to the largest voting distance to the threshold across all peer proposals that pass. If a firm is in the control group, I set the running variable to be equal to the lowest voting distance to the threshold across all peer proposals that fail. For instance, if a firm is associated with five proposals that pass by 5%, the running variable takes value 5%. If I would aggregate by summing across proposals the running variable would take the value of 25% instead. Since each individual proposal is close to the threshold, and therefore a valid source of exogenous variation, it is reasonable to argue that the running variable should also take a value close to the threshold. Another advantage of aggregating via the *MaxMin* method is that it increases statistical power by moving observations closer to the threshold.

The results in Panel B of C.7 show the conclusions are robust to using the *MaxMin* method. In Panel C of C.7 I show the results occur through the network of the board of directors and not through the network of executives. This is exactly the pattern observed when using the IV and the diff-in-diff analyses. The fact that the results are robust across such disparate methodologies suggests that social peer effects of CSR are a causal phenomenon.

Table C.7. Quasi-experimental evidence from close-call CSR proposals

This table reports the output of regression discontinuity design regressions measuring the response of firms' CSR decisions to the passage of close-call CSR shareholder proposals by their social peers in the previous year. The estimation is done using nonparametric local linear regression and the Imbens and Kalyanaraman (2012) optimal bandwidth selection method. *Industry-Year Adj.* indicates that only variation in CSR scores that is orthogonal to industry-by-year fixed effects is used. These orthogonalized CSR scores are obtained as the residuals of an ordinary least squares regression of CSR scores on three-digit SIC industry-by-year fixed effects. The control variables include all firm-level control variables described in C.1. The regression coefficients are measured in standard deviation units. t-statistics are reported in parentheses. Standard errors are clustered at the firm level. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

Panel A: Baseline Results

	(1)	(2)	(3)	(4)
Triangular Kernel	0.743***	0.208**	0.826***	0.314***
	(6.521)	(2.129)	(11.785)	(6.099)
Rectangular Kernel	0.815***	0.305***	0.807***	0.306***
	(6.806)	(2.928)	(11.887)	(5.922)
Industry-Year Adj.	No	No	Yes	Yes
Controls	No	Yes	No	Yes
Bandwidth	Optimal Bandwidth			

Panel B: *MaxMin* Aggregation Method

	(1)	(2)	(3)	(4)
Triangular Kernel	0.741***	0.254***	0.883***	0.293***
	(7.381)	(3.136)	(8.588)	(3.770)
Rectangular Kernel	0.775***	0.261***	0.923***	0.325***
	(7.655)	(3.064)	(8.591)	(3.762)
Industry-Year Adj.	No	No	Yes	Yes
Controls	No	Yes	No	Yes
Bandwidth	Optimal Bandwidth			

Panel C: Directors versus Executives Networks

	Directors Network		Executives Network	
	(1)	(2)	(3)	(4)
Triangular Kernel	0.257***	0.274***	0.042	0.217
	(3.257)	(3.445)	(0.273)	(1.620)
Rectangular Kernel	0.290***	0.324***	0.048	0.218*
	(3.451)	(3.748)	(0.294)	(1.671)
Industry-Year Adj.	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes
Bandwidth	Optimal Bandwidth			

In C.8 I conduct a series of robustness tests. First, I show the results are robust to using alternative bandwidths. Second, I show that there is no evidence for discontinuity jumps around placebo thresholds. This is reassuring because the existence of discontinuities at placebo thresholds would be a warning sign that the discontinuities at the true threshold could be contaminated by those same omitted factors that cause jumps at the placebo thresholds.

Table C.8. Quasi-experimental evidence from close-call CSR proposals: robustness tests

This table reports the output of regression discontinuity design regressions measuring the response of firms' CSR decisions to the passage of close-call CSR shareholder proposals by their social peers in the previous year. The estimation is done using nonparametric local linear regression and the Imbens and Kalyanaraman (2012) optimal bandwidth selection method. Proposals are aggregated using the *MaxMin* method. *Industry-Year Adj.* indicates that only variation in CSR scores that is orthogonal to industry-by-year fixed effects is used. These orthogonalized CSR scores are obtained as the residuals of an ordinary least squares regression of CSR scores on three-digit SIC industry-by-year fixed effects. The control variables include all firm-level control variables described in C.1. The regression coefficients are measured in standard deviation units. t-statistics are reported in parentheses. Standard errors are clustered at the firm level. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

Panel A: Smaller Bandwidths				
	(1)	(2)	(3)	(4)
Triangular Kernel	0.704***	0.223**	0.830***	0.218**
	(6.083)	(2.312)	(6.792)	(2.295)
Rectangular Kernel	0.681***	0.235**	0.835***	0.278**
	(5.214)	(2.117)	(6.083)	(2.536)
Industry-Year Adj.	No	No	Yes	Yes
Controls	No	Yes	No	Yes
Bandwidth	80% of Optimal Bandwidth			

Table C.8. Quasi-experimental evidence from close-call CSR proposals: robustness tests

(continued)

Panel B: Larger Bandwidths				
	(1)	(2)	(3)	(4)
Triangular Kernel	0.806***	0.328***	1.090***	0.411***
	(9.674)	(5.199)	(13.238)	(6.858)
Rectangular Kernel	0.760***	0.268***	0.971***	0.408***
	(8.151)	(3.542)	(10.349)	(5.634)
Industry-Year Adj.	No	No	Yes	Yes
Controls	No	Yes	No	Yes
Bandwidth	120% of Optimal Bandwidth			

Panel C: Placebo Discontinuity Tests				
	(1)	(2)	(3)	(4)
Triangular Kernel	0.025	0.231	0.474	-0.061
	(0.342)	(1.215)	(1.143)	(-0.256)
Rectangular Kernel	0.026	0.207	0.012	-0.061
	(0.361)	(1.128)	(0.070)	(-0.256)
Industry-Year Adj.	No	No	No	No
Controls	No	No	No	No
Placebo Threshold	-20%	-10%	10%	20%
Bandwidth	Optimal Bandwidth			

C.9. Additional analyses and robustness tests

Table C.9. Robustness tests of baseline results

This table reports the output of two-stage least squares (2SLS) regressions of firm CSR scores on social peers' CSR scores under a variety of different specifications. Social peers are defined based on the social networks of executives and directors. The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) operate in different industries; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The control variables include all firm-level and peer-level control variables described in C.1. The coefficients are measured in standard deviations. The Kleiberg-Paap F-stat is the cluster-robust Kleibergen and Paap (2006) F-statistic for weak instruments. t-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

	Panel A: Alternative Definitions of Peer Groups						
	Excludes Firms with < 10 Peers	Excludes Firms with > 250 Peers	Excludes Ind. Peers in Firm's SIC1 Industry	Excludes Ind. Peers in Firm's Headquarters State	Excludes Direct Board Interlocks	Includes S&P 1500 Firms Only	Controls for Hoberg-Philips Peers' CSR score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Peers' CSR	0.712***	0.462***	0.458***	0.383**	0.402**	0.556**	0.444***
	(3.012)	(2.867)	(2.751)	(2.394)	(2.326)	(2.553)	(2.687)
Kleiberg-Paap F-stat	90.072***	65.018***	76.889***	67.829***	52.268***	51.157***	59.609***
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Incorp. State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes	Yes	Yes
All Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs	23,377	25,567	25,541	25,808	25,668	17,516	23,808

Table C.9. Robustness tests of baseline results

(continued)

	Panel B: Additional Combinations of Fixed Effects and Alternative Specifications						
	Headquarters State-by-Year FE (1)	Firm and Year FE (2)	Lagged Instrument (3)	Lagged Inst. and Controls (4)	Twice Lagged Instrument (5)	Twice Lagged Inst. Board Net. (6)	No Controls (7)
Peers' CSR	0.299**	0.151*	0.598**	0.600**	0.581*	0.665**	0.363**
	(2.241)	(1.883)	(2.242)	(2.097)	(1.809)	(1.995)	(2.266)
Kleiberg-Paap F-stat	79.742***	119.705***	31.979***	28.267***	24.289***	23.400***	48.435***
CSA-by-year FE	No	No	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	No	Yes	Yes	Yes	Yes	Yes
Incorp. State-by-year FE	Yes	No	Yes	Yes	Yes	Yes	Yes
Headquarters State-by-year FE	Yes	No	No	No	No	No	No
Firm and Year FE	No	Yes	No	No	No	No	No
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes	Yes	Yes
All Controls	Yes	Yes	Yes	Yes	Yes	Yes	No
Lagged Controls	No	No	No	Yes	Yes	Yes	No
Lagged Instrument	No	No	Yes	Yes	Yes	Yes	No
No. Obs.	25,787	25,794	22,958	22,958	20,126	20,020	25,808

Table C.10. Robustness tests of baseline results: constraining the sample period to end in 2013

This table reports the results of two-stage least squares (2SLS) regressions of firm CSR scores on social peers' CSR scores when restricting the sample period to 2001-2013. Social peers are defined based on the social networks of executives and directors. The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) operate in different industries; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The additional controls are customer awareness and R&D investment. The coefficients are measured in standard deviation units. The Kleiberg-Paap F-stat is the cluster-robust Kleibergen and Paap (2006) F-statistic for weak instruments. t-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

	Contemporaneous			Lagged		
	(1)	(2)	(3)	(4)	(5)	(6)
Peers' CSR	0.569***	0.606***	0.598***	0.570**	0.623**	0.626**
	(2.588)	(2.598)	(2.871)	(2.049)	(2.053)	(2.293)
Peers' Size	-0.225**	-0.230**	-0.209**	-0.198	-0.213	-0.199*
	(-2.004)	(-2.011)	(-2.391)	(-1.449)	(-1.483)	(-1.843)
Peers' MB Ratio	-0.005	-0.005	-0.005	-0.002	-0.002	-0.003
	(-0.639)	(-0.586)	(-0.620)	(-0.212)	(-0.209)	(-0.354)
Peers' Debt Ratio	0.037**	0.030**	0.025*	0.032*	0.026	0.020
	(2.482)	(2.093)	(1.818)	(1.923)	(1.607)	(1.276)
Peers' ROA	0.010	0.004	0.001	0.027*	0.022*	0.017
	(0.716)	(0.335)	(0.064)	(1.747)	(1.688)	(1.383)
Peers' Net Income	-0.017	-0.034	-0.016	-0.019	-0.039	-0.028
	(-0.354)	(-0.674)	(-0.509)	(-0.351)	(-0.674)	(-0.786)
Peers' Cash Ratio	-0.091	-0.088*	-0.061*	-0.087	-0.089	-0.066
	(-1.619)	(-1.660)	(-1.755)	(-1.262)	(-1.355)	(-1.605)
Peers' Divid. Ratio	-0.036**	-0.033*	-0.036**	-0.031*	-0.025	-0.029*
	(-2.286)	(-1.934)	(-2.406)	(-1.828)	(-1.280)	(-1.714)
Peers' Inst. Own.	0.002	-0.007	-0.010	0.005	-0.006	-0.009
	(0.233)	(-0.765)	(-1.046)	(0.449)	(-0.499)	(-0.868)
Peers' Cust. Awa.			-0.018			-0.020
			(-1.008)			(-0.970)
Peers' R&D			-0.053			-0.046
			(-1.152)			(-0.766)
Kleiberg-Paap F-stat	45.480***	36.167***	43.626***	32.980***	26.032***	32.373***
First Stage Instrument	0.173***	0.167***	0.186***	0.155***	0.146***	0.161***
	(6.740)	(6.010)	(6.600)	(5.740)	(5.100)	(5.690)
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	No	No	Yes	No	No	Yes
Ex. Industry Peers	No	Yes	Yes	No	Yes	Yes
No. Obs.	21,521	21,521	21,521	18,734	18,734	18,734

Table C.11. Robustness tests of baseline results: using Thomson Reuters CSR scores

This table reports the output of two-stage least squares (2SLS) regressions of firm CSR scores on social peers' CSR scores. Social peers are defined based on the social networks of executives and directors. The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) operate in different industries; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). E+S refers to the aggregate CSR scores which are constructed as the average of the environmental and social sustainability scores. Every control variable is included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The control variables include all firm-level and peer-level control variables described in C.1. The coefficients are measured in standard deviations. The Kleiberg-Paap F-stat is the cluster-robust Kleibergen and Paap (2006) F-statistic for weak instruments. t-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

Panel A: Includes All Firms						
	Contemporaneous			Lagged		
	E+S	Env. (E)	Social (S)	E+S	Env. (E)	Social (S)
	(1)	(2)	(3)	(4)	(5)	(6)
Peers' CSR	0.319**	0.295*	0.369**	0.321*	0.292	0.350*
	(2.215)	(1.906)	(2.304)	(1.870)	(1.553)	(1.854)
Kleiberg-Paap F-stat	53.616***	46.859***	43.729***	40.847***	34.542***	32.744***
First Stage Instrument	0.205***	0.202***	0.191***	0.193***	0.190***	0.178***
	(7.320)	(6.850)	(6.610)	(6.390)	(5.880)	(5.720)
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs	8,802	8,802	8,802	7,623	7,623	7,623

Table C.11. Robustness tests of baseline results: using Thomson Reuters CSR scores

(continued)

Panel B: Excludes Firms With Only One Social Peer

	Contemporaneous			Lagged		
	E+S (1)	Env. (E) (2)	Social (S) (3)	E+S (4)	Env. (E) (5)	Social (S) (6)
Peers' CSR	0.344**	0.326**	0.382**	0.335**	0.315*	0.356**
	(2.547)	(2.206)	(2.526)	(2.125)	(1.806)	(1.999)
Kleiberg-Paap F-stat	61.069***	54.860***	50.590***	49.586***	43.388***	38.422***
First Stage Instrument	0.213***	0.211***	0.197***	0.199***	0.200***	0.179***
	(7.810)	(7.410)	(7.110)	(7.040)	(6.590)	(6.200)
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	8,755	8,755	8,755	7,602	7,602	7,602

Panel C: Excludes Firms With Less Than 10 Social Peers or More Than 100 Peers

	Contemporaneous			Lagged		
	E+S (1)	Env. (E) (2)	Social (S) (3)	E+S (4)	Env. (E) (5)	Social (S) (6)
Peers' CSR	0.508***	0.606***	0.433**	0.572***	0.617**	0.483**
	(2.726)	(2.763)	(2.212)	(2.705)	(2.501)	(2.134)
Kleiberg-Paap F-stat	63.383***	50.630***	52.343***	47.681***	41.981***	48.061***
First Stage Instrument	0.198***	0.179***	0.204***	0.178***	0.171***	0.174***
	(7.960)	(7.120)	(7.230)	(6.910)	(6.480)	(5.990)
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	6,624	6,624	6,624	5,710	5,710	5,710

Table C.12. Why do firms mimic? The role of institutional ownership

This table reports the output of two-stage least squares (2SLS) regressions of firm CSR scores on social peers' CSR scores. Social peers are defined based on the social networks of directors. The magnitude of peer effects is allowed to vary as a function of one of the following measures of institutional ownership concentration: institutional ownership Herfindahl-Hirschman (HHI) index, ownership by the five largest institutional shareholders and institutional ownership by blockholders. D_{High}, D_{Med} and D_{Low} are binary indicators equal to one if the associated variable in a given firm-year belongs to the third, second and first tercile of the within-year distribution of that variable, respectively. The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) operate in different industries; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The additional controls are customer awareness and R&D investment. The coefficients are measured in standard deviation units. *P(H = L)* is the p-value obtained from testing the hypothesis that peer effects are equal across firms in the highest and lowest terciles. The Sanderson-Windmeijer F-stat refers to the Sanderson and Windmeijer (2016) weak instrument F-test for models with multiple endogenous variables. t-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

	Levels			First Differences		
	Inst. Own. HHI Index	Inst. Own. Largest 5	Inst. Own. Blockhold.	Inst. Own. HHI Index	Inst. Own. Largest 5	Inst. Own. Blockhold.
	(1)	(2)	(3)	(4)	(5)	(6)
Peer's CSR × D_{Low}	0.564***	0.525***	0.483***	0.314***	0.260***	0.256**
	(3.980)	(3.514)	(3.232)	(3.360)	(2.671)	(2.409)
Peer's CSR × D_{Med}	0.475***	0.471***	0.455***	0.182*	0.177*	0.218**
	(3.227)	(3.181)	(3.047)	(1.844)	(1.790)	(2.020)
Peer's CSR × D_{High}	0.361**	0.416***	0.393**	0.033	0.063	0.082
	(2.364)	(2.719)	(2.543)	(0.322)	(0.609)	(0.722)
P(H=L)	0.000	0.001	0.004	0.000	0.000	0.000
Sanderson-Windmeijer F-Stat						
Ind. Peer's CSR × D_{Low}	83.510***	78.830***	81.080***	99.000***	97.540***	94.620***
Ind. Peer's CSR × D_{Med}	79.420***	83.780***	86.070***	91.200***	98.310***	90.530***
Ind. Peer's CSR × D_{High}	77.120***	84.870***	85.420***	90.070***	96.320***	93.730***
First Stage Instrument						
Ind. Peer's CSR × D_{Low}	0.597***	0.515***	0.525***	0.620***	0.553***	0.579***
	(24.240)	(18.320)	(19.210)	(36.300)	(27.180)	(27.770)
Ind. Peer's CSR × D_{Med}	0.588***	0.567***	0.548***	0.548***	0.578***	0.578***
	(29.790)	(24.830)	(22.550)	(28.000)	(30.160)	(29.840)
Ind. Peer's CSR × D_{High}	0.422***	0.522***	0.535***	0.451***	0.489***	0.482***
	(15.270)	(22.040)	(21.910)	(20.360)	(25.190)	(24.210)
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	25,664	25,661	23,489	22,833	22,827	20,175

Table C.13. Social norms channel: robustness tests

This table reports the output of two-stage least squares (2SLS) regressions of changes in firm CSR scores on the changes in social peers' CSR scores. Social peers are defined based on the social networks of directors. The magnitude of peer effects is allowed to vary as a function of each firm's own geographic social capital. Own geographic social capital is proxied by of one of the following variables: organ donation density, voter turnout, registered organization density, association density or the principal component of the previous three variables. D_{High}, D_{Med} and D_{Low} are binary indicators equal to unity if the associated variable in a given firm-year belongs to the third, second and first tercile of the within-year distribution of that variable, respectively. The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) operate in different industries; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is first differenced and included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The additional controls are customer awareness and R&D investment. The coefficients are measured in standard deviation units. *P(H = L)* is the p-value obtained from testing the hypothesis that peer effects are equal across firms in the highest and lowest terciles. The Sanderson-Windmeijer F-stat refers to the Sanderson and Windmeijer (2016) weak instrument F-test for models with multiple endogenous variables. t-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

	Own Geographic Social Capital				
	Organ Don.	Voter Turnout	Reg. Density	Org. Density	PC Index
	(1)	(2)	(3)	(4)	(5)
Δ Peer's CSR × D_{Low}	0.204*	0.225**	0.184*	0.163	0.183*
	(1.938)	(2.173)	(1.865)	(1.585)	(1.783)
Δ Peer's CSR × D_{Med}	0.237**	0.171*	0.094	0.198*	0.142
	(2.290)	(1.709)	(0.888)	(1.940)	(1.344)
Δ Peer's CSR × D_{High}	0.093	0.135	0.261**	0.204*	0.221**
	(0.800)	(1.216)	(2.528)	(1.885)	(2.060)
P(H = L)	0.177	0.235	0.130	0.554	0.594
Sanderson-Windmeijer F-Stat					
Δ Ind. Peer's CSR × D_{Low}	112.880***	104.270***	96.640***	98.060***	103.100***
Δ Ind. Peer's CSR × D_{Med}	112.300***	110.510***	107.970***	103.150***	98.660***
Δ Ind. Peer's CSR × D_{High}	88.800***	99.850***	106.780***	113.710***	106.230***
First Stage Instrument					
Δ Ind. Peer's CSR × D_{Low}	0.516***	0.539***	0.548***	0.540***	0.517***
	(17.940)	(19.890)	(22.970)	(20.160)	(18.500)
Δ Ind. Peer's CSR × D_{Med}	0.476***	0.554***	0.510***	0.553***	0.489***
	(15.590)	(23.770)	(20.090)	(24.820)	(18.490)
Δ Ind. Peer's CSR × D_{High}	0.394***	0.421***	0.543***	0.483***	0.523***
	(12.330)	(16.800)	(25.300)	(20.110)	(24.620)
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes
No. Obs.	22,653	22,653	22,653	22,653	22,653

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Nederlandse samenvatting (Summary in Dutch)

Het eerste essay toont dat rendementen van grondstoffenfutures een totaalbeeld geven van verspreide informatie over toekomstige macro-economische factoren en aandelenrendementen op landenniveau in tal van landen wereldwijd. Opmerkelijk is dat we ook hebben vastgesteld dat de afhankelijkheid van landen van de grondstoffenhandel niet de belangrijkste verklaring voor dit verschijnsel is. Deze bevinding geldt zelfs als we rekening houden met indirecte blootstelling aan afhankelijkheid van grondstoffenhandel die ontstaat door financiële en handelsintegratie tussen landen.

Dit suggereert dat de rol die grondstoffenmarkten spelen voor het openbaren van informatie uitermate complex en van absoluut wereldwijde schaal is. Deze interpretatie kunnen we verder onderbouwen met onze bevinding dat de informatie die grondstoffenmarkten hebben over toekomstige opbrengsten van voorraden wereldwijd gelijk verdeeld is tussen grondstoffensectoren en tussen landen. Met andere woorden, de informatiestromen van de grondstoffenmarkten naar de aandelenmarkten beperken zich niet tot de energiesector en een klein aantal landen met specifieke kenmerken.

Onze resultaten zijn dus consistent met een van de belangrijkste onderdelen van het invloedsmodel van Sockin and Xiong (2015): het idee dat grondstoffenfutures informatie hebben over de staat van de wereldeconomie. Onze bevindingen als zodanig geven enige plausibiliteit aan de door Sockin and Xiong (2015) onderzochte theoretische mogelijkheid dat de handel in grondstoffenfutures (mogelijk verstoorde) prijssignalen kan genereren die van invloed zijn op productiebeslissingen van bedrijven in de hele wereld en op spotprijzen van grondstoffen.

Onze hoop is dat dit essay een motivering zal zijn voor verder onderzoek naar de wisselwerking tussen de handel in grondstoffenfutures en de reële economie. Alleen zo kunnen we echt begrijpen of we de speculatiegolf in de markten van de grondstoffenfutures die we sinds het begin van de jaren 2000 hebben gezien, als een zege moeten beschouwen of in bedwang moeten houden (bv. Cheng and Xiong (2014)).

Het tweede essay toont aan dat duurzaam beleggen op basis van ESG-ratings de beleggingsresultaten de afgelopen twintig jaar waarschijnlijk niet systematisch heeft verbeterd of geschaad. Dit is een buitengewoon sterke conclusie die standhoudt in de meeste regio's van de wereld, in verschillende tijdsperiodes, in verschillende economische sectoren, bij het gebruik verschillende ESG-ratings van drie grote beoordelaars, bij gebruik van combinaties van deze ratings, en bij het gebruik van ESG-ratings met zowel niveaus als veranderingen (ESG-momentum).

Hoewel wij ons ervan bewust zijn dat de resultaten van duurzaam beleggen in het vervolg anders kunnen zijn, geloven we dat onze bevindingen interessante inzichten bieden. Het goede nieuws is dat onze resultaten erop wijzen dat duurzame aandelen, als categorie, op dit moment waarschijnlijk niet worden overgewaardeerd. Dit vermindert de door meerdere beleidsmakers en beleggers geuite zorg dat we ons in een "ESG-bubbel" bevinden. In dit verband wijzen onze bevindingen er ook op dat het mogelijk zou kunnen zijn duurzaam te beleggen zonder te moeten inboeten aan rendement. Dit is een buitengewoon positieve bevinding, aangezien dit suggereert dat het mogelijk is dat beleggers een niet-geldelijke bijdrage leveren die niet ten koste gaat van hun pensioenen en materieel welzijn.

Deze resultaten kunnen echter ook op een negatievere manier worden geïnterpreteerd. Juist omdat het verband tussen ESG-ratings en beleggingsrendementen nihil is, kan er mogelijk niet op worden gerekend dat duurzaam beleggen altijd de kapitaalkosten van duurzame bedrijven verlaagt. Onze resultaten verschaffen hiermee enige empirische onderbouwing van het standpunt dat duurzaam beleggen geen betrouwbare oplossing is om de wereld duurzamer te maken.

Wij erkennen dat theoretische modellen (bv. Pastor, Stambaugh and Taylor (2020)) voorspellen dat duurzame bedrijven hun onduurzame concurrenten zelfs in een kort tijdsbestek kunnen overtreffen als gevolg van onverwachte ontwikkelingen die de behaalde rendementen van duurzame bedrijven verhogen, waarbij duurzame bedrijven niet eens lagere kapitaalkosten moeten hebben. Onze bevindingen trekken deze modellen niet in twijfel en wijzen er evenmin op dat duurzaam beleggen in de toekomst een belangrijke aanjager van positieve verandering zal zijn.

Wel benadrukken ze dat het mogelijk is dat veel duurzame bedrijven lange tijd, tot wel twintig jaar lang, niet zullen worden beloond met lagere kapitaalkosten. Dit is zorgwekkend, omdat twintig jaar volgens velen in de strijd tegen maatschappelijke problemen zoals klimaatverandering nauwelijks als korte termijn kan worden beschouwd. Klimaatdeskundigen geven bijvoorbeeld vaak 2030 als deadline voor ambitieuze reducties van CO2-emissies. Het is daarom onzeker of duurzaam beleggen bedrijven voldoende zal stimuleren om tijdig de gewenste duurzaamheidsdoelstellingen te behalen. Onze resultaten laten dus zien dat het wellicht riskant is om in te zetten op duurzaam beleggen in plaats van overheidsbeleid om de wereld duurzamer te maken.

In het derde essay identificeer ik een nieuw kanaal dat bestuursraden van bedrijven volgen om hun MVO-praktijken vorm te geven: sociaal leren via sociale netwerken. Meer specifiek toon ik aan dat de diverse links voor opleidings-, recreatieve en werkgelegenheidskansen die bedrijfsdirecteuren delen, een sociaal netwerk creëren dat als markt voor de uitwisseling van informatie over MVO fungeert.

Deze socialenetwerkeffecten op het gebied van MVO zijn met name waarneembaar in bedrijven die er baat bij kunnen hebben om meer over MVO te leren, bedrijven die dankzij een goede positie in het sociale netwerk toegang hebben tot waardevolle informatie, en bedrijven waar de ambities van managers en aandeelhouders op één lijn liggen. Mijn resultaten wijzen er dus op dat het traditionele concept van goed bestuur waar aandeelhouderskapitalisme op berust, ten minste in zekere mate consistent is met MVO.

Een boeiend aspect van dit essay is dat een smal kanaal wordt geïdentificeerd dat in het bedrijfsleven door directeuren wordt gevolgd om MVO-beleid vorm te geven. Dit verduidelijkt enigszins hoe een specifiek corporate governance-mechanisme – de raad van bestuur – MVO beïnvloedt. Hiermee draagt het ook bij aan het ontrafelen van het geheim van de totstandkoming van MVO-beslissingen in de praktijk. Deze twee zaken zijn essentieel om het verband tussen corporate governance en MVO volledig te doorgronden. Als zodanig kunnen de in dit essay uiteengezette bevindingen een nuttige inbreng zijn voor het debat over hoe de rol van bedrijven in de maatschappij eruit moet zien.

Dit is uiteraard al lang onderwerp van een groot publiek debat. Bijzonder twijfelachtig is het initiatief *duurzame corporate governance* van de Europese

Commissie, dat gebaseerd is op het onderzoek "Study on directors' duties and sustainable corporate governance" van Ernst & Young. Een van de doelen van dit initiatief is dat directeuren meer rekening houden met de belangen van aandeelhouders. Volgens het onderzoek is het probleem dat directeuren de belangen van aandeelhouders negeren omdat hun beleid te sterk is afgestemd op de drang van aandeelhouders naar maximale rendementen op korte termijn.

Mijn resultaten laten zien dat dit idee wellicht genuanceerder ligt. In elk geval voor wat betreft de socialenetwerkeffecten bij MVO zijn bedrijven met een beter afgestemd beleid juist degenen die meer doen om bij te leren over MVO. Het is dus niet zo dat afstemming van het beleid en traditionele corporate governance noodzakelijkerwijs tot gevolg hebben dat directeuren de belangen van aandeelhouders negeren.[51]

Wat betekent dit voor bestuurshervormingen zoals het initiatief van de Europese Commissie? Mijn resultaten suggereren niet dat het niveau van maatschappelijke verantwoordelijkheid dat bedrijven vrijwillig kiezen, optimaal is vanuit het perspectief van de maatschappij als geheel. Mijn resultaten suggereren evenmin dat er geen ruimte is voor overheidsbeleid om de negatieve externe effecten op milieu en maatschappij die bedrijven veroorzaken, tegen te gaan. Wat mijn resultaten suggereren, is dat het verspreiden van informatie over de verdiensten van MVO ertoe kan leiden dat sommige bedrijven meer investeren in MVO op een manier die consistent is met goed bestuur. Dit is

[51] Ik erken dat de externe geldigheid van mijn resultaten zich beperkt tot de reikwijdte van socialenetwerkeffecten op het gebied van MVO. In het onderzoek van Ferrell, Liang and Renneboog (2016) wordt echter ook geconcludeerd dat er meer in het algemeen een positief verband bestaat tussen corporate governance-indicatoren en MVO. Mijn onderzoek vult het genoemde onderzoek aan doordat het een smal kanaal identificeert waarin het positieve verband tussen corporate governance en MVO zichtbaar wordt. Dit is om twee redenen interessant. Ten eerste geeft het onderzoek naar dit smalle kanaal mij de kans om een aantal tests en identificatiestrategieën te gebruiken die meer geloofwaardigheid geven aan het idee dat MVO in zekere mate consistent is met goede corporate governance. Ten tweede zijn er zeer goede redenen om aan te nemen dat socialenetwerkeffecten op het gebied van MVO een uiting van principaal-agentproblemen kunnen zijn. Voor zover dit het geval is, vormen mijn resultaten een bijzonder solide onderbouwing van het idee van het positieve verband tussen goed bestuur en MVO.

precies het doel van het MVO-programma van de United Nations Industrial Development Organization (UNIDO), waarin wordt samengewerkt met regeringen en bedrijven over de hele wereld. Mijn resultaten laten echter ook zien dat goede corporate governance tot op zekere hoogte parallel aan MVO kan worden toegepast. Of dit wenselijk is, valt te betwisten. Voor voorstellen om de huidige situatie van corporate governance te veranderen, is het wellicht nuttig om zorgvuldig rekening te houden met de mogelijkheid dat dergelijke veranderingen economisch relevante opportuniteitskosten met zich meebrengen.

About the author

Rómulo Alves was born in Santarém, Portugal, on April 30, 1991. Before the start of his PhD in 2016, he completed a Bachelor in Management at the Nova School of Business and Economics in Lisbon, a Master in Finance (*cum laude*) at the Rotterdam School of Management, Erasmus University, and a Research Master in Economics at the Tinbergen Institute in Amsterdam. During this period he co-founded Nova University's debate club, co-organized the first Leadership Tournament in Portugal, worked in microfinance in India, held leadership positions in AIESEC, and received a best master thesis award and a citizenship award.

During his PhD at the Erasmus University Rotterdam, Rómulo presented his work at various conferences and seminars around the world, such as the annual meetings of the American Finance Association, the European Finance Association, and the SFS Cavalcade Asia-Pacific. He also received two best paper awards and went on a research visit to the London Business School.

Rómulo now lives in Paris where he is Assistant Professor at SKEMA Business School. His current research interests include corporate governance, corporate social responsibility, corporate finance, and sustainable finance.

Portfolio

Working papers

- Alves, Romulo, and Marta Szymanowska, 2019, The information content of commodity futures markets, Available at SSRN 3352822.
- Alves, Romulo, 2020, Social networks and corporate social responsibility, Available at SSRN 3710868.
- Alves, Romulo, Philipp Krueger, and Mathijs van Dijk, 2021, Drawing up the bill: Does sustainable investing affect stock returns around the world?, Working Paper.

Conferences and seminars[52]

2021

- American Economic Association Poster Session (Virtual)
- Bank of Portugal Seminar (Virtual)
- European Financial Management Association PhD Workshop (Virtual)
- French Finance Association 37th International Conference (Virtual)

[52] Presentations by co-authors marked with a *.

- KU Leuven Seminar (Virtual)
- Midwest Finance Association Annual Meeting (Virtual)
- Paris Dauphine University Seminar (Virtual)
- Royal Economic Society Annual Meeting (Virtual)
- SKEMA Business School Seminar (Virtual)

2020

- American Finance Association Annual Meeting Poster Session (San Diego)
- Econometric Society European Winter Meetings (Nottingham)
- Erasmus University PhD Seminar (Rotterdam)
- European Economic Association Annual Meeting (Rotterdam)
- European Finance Association Annual Meeting PhD Poster (Helsinki)
- Global Research Alliance for Sustainable Finance PhD Day (New York)
- London Business School PhD Seminar (London)
- Northern Finance Association PhD Symposium (Banff)
- Pompeu Fabra University Seminar (Barcelona)
- 4th Shanghai-Edinburgh Green Finance Conference (Shanghai)
- 5th SDU Finance Workshop (Odense)
- 17th Corporate Finance Day (Liège)
- 28th Finance Forum PhD Consortium (Lisbon)

2019

- Commodity & Energy Markets Association Annual Meeting (Pittsburgh)
- European Economic Association Annual Meeting (Manchester)
- European Economics and Finance Society Annual Meeting (Genoa)
- Financial Management Association Annual Meeting (New Orleans)*
- SAFE Asset Pricing Workshop (Frankfurt)*
- SFS Cavalcade Asia-Pacific (Hong Kong)
- XXII Applied Economics Meeting (Cartagena)
- 3rd Commodity Market Winter Workshop (Hannover)
- 27th Finance Forum (Madrid)

2018

- Erasmus Finance Day (Rotterdam)
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2017

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Teaching experience

Lecturer

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- 2018 – Banking (BSc.)

- 2017-20 – Thesis Trajectory Lectures on Board Diversity and Firm Value (BSc.)
- 2016 – Thesis Trajectory Lecture on Investor Sentiment (BSc.)

Teaching Assistant

- 2020 – Valuation (MSc.)
- 2019 – Alternative Investments (BSc.)

Theses Supervision

- 2017-21 – 68 MSc. students
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Grants and Awards

- Netspar research grant (joint with Philipp Krueger and Mathijs van Dijk)
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- Talent Grant for outstanding PhD candidates, RSM/ERIM, 2020
- Hermes Kring Londen Fonds Grant, Research Visit at London Business School, 2020
- Best Paper Award on Derivatives, 27th Finance Forum, 2019
- Conference Travel Grant, Asociacíon Libre de Economia, 2019
- Oxford Scholarship and ERIM Travel Grant, Oxford Economic Networks Summer Programme, 2018
- ERIM Travel Grant, Workshop Causal Inference, Northwestern and Duke Universities, 2017

- Best Master Thesis Award, Erasmus University Rotterdam, 2014
- Citizenship Award, NOVA School of Business and Economics, 2013
- CG&G Certification, Outstanding Performance in Macroeconomics Course, NOVA School of Business and Economics, 2010

The ERIM PhD Series

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