Universitat Pompeu Fabra
Departament d'Economia i Empresa

DOCTORAT EN ECONOMIA, FINANCES I EMPRESA

Essays in Applied Economics

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TESI DOCTORAL UPF / 2021

ACKNOWLEDGEMENTS

I would like to thank Antonio Ciccone, Ruben Enikolopov and Barbara Rossi, without whose support, advice, encouragement and patience this dissertation would not have been possible. Their work has inspired me, and I learned a lot working with them.

Thanks to my friends, Kinga Tchorzewska, Christopher Evans and Ilja Kantorovitch, with whom we went through all ups and downs of this process together. Thanks to Milena Djourelova, Ines Martins Xavier, Angelo Gutierrez-Daza, Sébastien Willis, Donghai Zhang, Dijana Zejcirovic, Sebastian Ellingsen, Josep Gisbert, Juan Imbet, and many other friends, colleagues, and faculty members, who have supported me in a myriad of ways. Thanks to Marta Araque and Laura Agusti for their support throughout these years—we at UPF are extremely lucky to have you.

Thanks to my family, especially my sister Dilyaram, who believed in me and motivated me throughout this journey.

Abstract

This thesis consists of two independent articles. In the first article, we examine the relationship between uncertainty and uncovered interest rate parity. It is well-known that uncovered interest rate parity does not hold empirically, especially at short horizons. But is it so? We conjecture that uncovered interest rate parity is more likely to hold in low uncertainty environments, relative to high uncertainty ones, since arbitrage opportunity gains become more uncertain in a highly unpredictable environment, thus blurring the relationship between exchange rates and interest rate differentials. We first provide a new exchange rate uncertainty index, that measures how unpredictable exchange rates are relative to their historical past. Then we use the new measure of uncertainty to provide empirical evidence that uncovered interest rate parity does hold in five industrialized countries vis-à-vis the US dollar at times when uncertainty is not exceptionally high and breaks down during periods of high uncertainty. In the second article, we examine the effect of rainfall on agricultural output and democratization in the world's most agricultural countries. Like the agricultural economics literature, we find that the relationship between rainfall and agricultural output has an inverted U-shape, as agriculture is harmed by both droughts and very wet conditions. We also find the effect of rainfall on agricultural output to be transitory. At the same time, the relationship between rainfall and democratization is U-shaped in the short run and this effect persists in the long run, meaning that democratic transitions outlast the (transitory) rainfall shocks that started the democratization process. We show that the U-shaped relationship between rainfall and democratization is consistent with rainfall affecting democratization through its (inverted-U-shaped) effect on agricultural output.

Resum

Aquesta tesi consta de dos articles independents. En el primer article, examinem la relació entre la incertesa i la paritat de tipus d'interès no coberta. És ben sabut que la paritat de tipus d'interès no coberta no es manté empíricament, especialment en horitzons curts. Però és realment així? Conjecturem que la paritat de tipus d'interès no coberta és més probable que es mantingui en entorns de baixa incertesa, en comparació amb els d'alta incertesa, ja que els guanys d'oportunitat d'arbitratge es tornen més incerts en un entorn altament impredictible, cosa que dilueix la relació entre tipus de canvi i diferencials de tipus d'interès. Primer proporcionem un nou índex d'incertesa del tipus de canvi, que mesura quan impredictibles son els tipus de canvi en relació amb el seu passat històric. A continuació, utilitzem la nova mesura d'incertesa per proporcionar evidència empírica que la paritat de tipus d'interès no coberta es manté en cinc països industrialitzats respecte al dòlar dels EUA en moments en què la incertesa no és excepcionalment alta i es trenca durant els períodes d'alta incertesa. En el segon article, examinem l'efecte de les pluges en la producció agrícola i en la democratització als països més agrícoles del món. Igual que el que diu la literatura d'economia agrícola, trobem que la relació entre les pluges i la producció agrícola té una forma d'U invertida, ja que l'agricultura es veu perjudicada tant per les sequeres com per les condicions molt humides. També trobem que l'efecte de la pluja sobre la producció agrícola és transitori. Al mateix temps, la relació entre les pluges i la democratització té forma d'U a curt termini i aquest efecte persisteix a llarg termini, és a dir, que les transicions democràtiques perduren més que els xocs de pluja (transitoris) que van iniciar el procés de democratització. Mostrem que la relació en forma d'U entre les pluges i la democratització és coherent amb les pluges que afecten la democratització a través del seu efecte (en forma d'U invertida) sobre la producció agrícola.

Preface

This thesis consists of two independent articles. These two articles focus on different applied economic questions, however both attempt to tackle these questions in an environment where the quantity of data available is scarce due to country-level observations, the rarity of occurring events or the presence of structural changes.

In the first article, together with Barbara Rossi, we study the conditions under which uncovered interest rate parity is less likely to hold. We show that uncovered interest rate parity does hold when uncertainty is not exceptionally high and breaks down during periods of high uncertainty by constructing a new measure of uncertainty based on exchange rate forecasting errors. We also show how one can obtain this measure even for countries for which high-quality forecast surveys or measures of uncertainty, such as the ones based on media data, might not be available. More in detail, we examine the relationship between uncertainty and uncovered interest rate parity. It is well-known that uncovered interest rate parity does not hold empirically, especially at short horizons. We conjecture that uncovered interest rate parity is more likely to hold in low uncertainty environments, relative to high uncertainty ones, since arbitrage opportunity gains become more uncertain in a highly unpredictable environment, thus blurring the relationship between exchange rates and interest rate differentials. We first provide a new exchange rate uncertainty index, that measures how unpredictable exchange rates are relative to their historical past. Then we use the new measure of uncertainty to provide empirical evidence that uncovered interest rate parity does hold in five industrialized countries vis-à-vis the US dollar at times when uncertainty is not exceptionally high and breaks down during periods of high uncertainty

In the second article, together with Antonio Ciccone, we look at how transitory shocks can lead to persistent democratization. The persistence of democratization following transitory economic

shocks plays an important role in the theory of political institutions. We focus on countries where agricultural shocks are still very important and find evidence consistent with transitory shocks—agricultural shocks in this case—affecting democratization through its (inverted-U-shaped) effect on agricultural output. Like the agricultural economics literature, we find that the relationship between rainfall and agricultural output has an inverted U-shape, as agriculture is harmed by both droughts and very wet conditions. We also find the effect of rainfall on agricultural output to be transitory. At the same time, the relationship between rainfall and democratization is U-shaped in the short run and this effect persists in the long run, meaning that democratic transitions outlast the (transitory) rainfall shocks that started the democratization process, even after ten years. Among other things, these findings suggest that even if they are short-lived, crises such as the COVID-19 crisis could potentially tip the scales against some authoritarian regimes and lead to persistent democratization.

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UNCERTAINTY AND DEVIATIONS FROM UNCOVERED INTEREST RATE PARITY

Joint with Barbara Rossi (Universitat Pompeu Fabra)

Published as Ismailov, A., and Rossi, B. (2018). Uncertainty and Deviations from Uncovered Interest Rate Parity. *Journal of International Money and Finance*, 88, 242–259

1.1 INTRODUCTION

A well-known empirical fact in international finance is that uncovered interest rate parity (UIRP) does not hold, especially at short horizons. UIRP states that, in the absence of arbitrage opportunities, the returns from investments in two countries should be equalized, once they are converted into the same currency. The implication is that interest rate differentials should predict bilateral nominal exchange rate appreciations or depreciations. UIRP is an important building block of most international macroeconomic models, and the lack of its validity is of such importance to deserve the term "UIRP puzzle". Another puzzling empirical fact about UIRP is that not only the coefficients do not have the values predicted by the theory, but also that they are unstable over time. This paper offers an explanation to both these puzzles by arguing that uncertainty is one of the reasons explaining the empirical invalidity of the UIRP; that the coefficients in UIRP regressions are more likely to be close to the values predicted by UIRP at times of low uncertainty; and that their time variation is, at least partly, due to the fact that UIRP holds when uncertainty is low but does not when uncertainty is high. As we discuss further below, a large body of literature argues that the UIRP puzzle is not really a puzzle since it can be explained by time-varying risk premia. Our empirical results are consistent with this literature, as we argue that, for example, high uncertainty can be related to rare disasters, which can theoretically generate the time-varying risk premia we observe in the data. Our paper, however, has the advantage of providing both an empirical analysis as well as an empirically observable proxy that can explain deviations from UIRP.

More in detail, this paper makes two main contributions. First, it proposes a new measure of exchange rate uncertainty, which is based on Rossi and Sekhposyan (2015). To our knowledge, this is the first paper to propose an index of exchange rate uncertainty. We measure uncertainty at a point in time by the likelihood of observing the realized exchange rate forecast error at that point in time, relative to the historical distribution of exchange rate forecast errors. Since the uncertainty measure is based on forecast errors, it depends on the model used to forecast exchange rates. To minimize the dependence of our empirical results on the choice of a specific model, we use Consensus survey forecasts, which have the favourable feature of being survey-based and timely incorporating a large amount of information. These survey forecasts have been used recently by Ozturk and Sheng (2018) to measure macroeconomic uncertainty; instead, we use them to construct an index of exchange rate uncertainty.

The second contribution is to make a step towards understanding why UIRP does not empirically fit the data. Typical estimates of the slope are either negative or zero or too large to be reconciled with the theory (Froot and Thaler, 1990); UIRP also fails to produce competitive out-of-sample forecasts relative to the random walk (Meese and Rogoff, 1983a, Meese and Rogoff, 1983b, Meese and Rogofp, 1988; Cheung et al., 2005; Alquist and Chinn, 2008 - see Rossi (2013) for a recent survey. There are several possible explanations that have been put forward in the literature. An important potential explanation is the presence of time-varying risk premia (Fama, 1984; Li et al., 2011). Other explanations include: imprecise standard errors (Baillie and Bollerslev, 2000; Rossi, 2007); small samples (Chinn and Meredith, 2004; Chinn and Quayyum, 2013; and Chen and Tsang, 2013); and rare disasters, such as currency crashes (Brunnermeier et al., 2009; Farhi and Gabaix, 2016).[1] In this paper, we investigate an alternative explanation for the UIRP puzzle, namely the fact that the uncovered interest rate parity might not hold in highly uncertain environments, while it is more likely to hold when uncertainty is low. In fact, when uncertainty is high, investors might postpone their investment decisions, and thus create deviations from what is expected in the absence of arbitrage opportunities. Our result does not depend on the measure of uncertainty we use: in fact, the result is robust to using other measures of uncertainty, as we demonstrate in the paper. In addition, as we show, deviations from UIRP cannot be explained solely by differences in monetary policy: while it is true that for some countries (such as Switzerland and the European Union—EU thereafter) UIRP is more likely to hold during the zero-lower bound period, the result is not true for all the countries in our sample. Furthermore, our results have direct implications for the risk premium: in fact, as we discuss, the risk premium is correlated with interest rate differentials in periods of high uncertainty, but not significantly correlated in periods of low uncertainty.

On the one hand, our main results focus on an uncertainty index based on survey forecasts, which has the advantage of not depending on a specific forecasting model; however, on the other hand, exchange

[1] Avdjiev et al. (2019) document instead large deviations from covered interest rate parity during the recent financial crisis, which they attribute to the lack of banks' ability to take on additional leverage.

rate survey forecasts are available only for a few countries, which limits the scope of the analysis. In order to extend the sample of countries, we construct an exchange rate uncertainty index based on the random walk, thus making our index suitable for big data. Among forecasting models of exchange rate determination, the random walk is a difficult benchmark to beat (Rossi, 2013). We show that our results for the main countries in our sample (Canada, the EU, Japan, Switzerland and the UK) are robust no matter whether we use surveys or the random walk to construct an uncertainty index. More importantly, we show that the UIRP puzzle is alleviated in low uncertainty environments for several of the additional countries that the extension to random walk forecast errors allows us to consider (Australia, Sweden, Denmark). For some other countries, although low uncertainty typically moves the coefficient in the right direction, it does not fully resolve the puzzle (South Africa and New Zealand); however, the latter are "commodity countries" (Chen and Rogoff, 2003; Chen et al., 2010), for which commodity prices might play a role in determining exchange rate fluctuations, which we abstract from.

This paper is related to several recent strands in the literature. The first strand is the empirical literature on the UIRP puzzle. While it is uncontroversial that the UIRP does not hold at short horizons, Chinn and Meredith (2004), Lothian and Wu (2011) and Chinn and Quayyum (2013) find more empirical evidence in favour of UIRP at longer horizons.[2] In particular, Chinn and Meredith (2004) argue that the lack of empirical evidence in favour of UIRP is due to small samples, and find that UIRP holds at longer horizons (above one year) in the longer sample of data they have available. Lothian and Wu (2011) examine historical data from 1800 to 1999 and find that the UIRP regression slope is positive for the longest sample, and the strong negative relation found in the literature is a feature of the late 1970s and the 1980s. Finally, Chinn

[2] Note that monetary models of exchange rates are more likely to hold at long horizons as well (Mark, 1995).

and Quayyum (2013) extend the analysis in Chinn and Meredith (2004) by a decade and find that the results in the latter are robust; however, the evidence is slightly weaker, potentially because the longer sample includes the zero-lower bound period. In this paper, differently from the contributions listed above, we focus instead on the lack of empirical validity of UIRP in the short run, which still remains a puzzle in the literature, and argue that uncertainty plays a potentially important role in explaining the puzzle.

Our paper is also related to the recent literature that has developed theoretical models to explain the UIRP puzzle. Two possible explanations for the lack of empirical validity of the UIRP are the presence of time-varying risk premia and expectational errors (Lewis, 1995). For example, Fama (1984) attributes the lack of empirical validity of the UIRP to time-varying risk premia. His paper shows that, in order to fit the empirical evidence, the implied risk premia of a country must be negatively correlated with its expected rate of depreciation and have greater variance. However, asset pricing models had not been able to produce risk premia with these properties, hence the term "puzzle". There are several possible theoretical explanations for time-varying risk premia, among which the most recent include Brunnermeier et al. (2009) and Farhi and Gabaix (2016). Brunnermeier et al. (2009) look at currency crashes and carry trades, where traders borrow low-interest-rate currencies and lend high-interest currencies. One of their findings is that higher levels of the VIX and TED spread predict higher future returns on the carry trade, implying larger UIRP violations. Farhi and Gabaix (2016) link time-varying risk premia in currency markets to rare but extreme disasters: since both the probability of these disasters as well as each country's exposure to them is time varying, the model can potentially generate the lack of UIRP, as relatively riskier countries end up with a higher interest rate to compensate investors in case the disaster happens. However, their evidence is limited to a calibration analysis showing that the theoretical predictions of the models are consistent with empirical puzzles (such as UIRP), as opposed to demonstrating empirically the link in the data. The reason is that rare disasters realize sporadically in the data, and thus it is difficult to find empirical evidence in favour of their model.[3]

Our empirical results provide potential empirical support in favour of Farhi and Gabaix (2016) in the following sense. An unexpected rare disaster that realizes in the data will increase our uncertainty index; conversely, even a situation where agents expect a rare disaster that does not realize in the data will increase our uncertainty index, as the expectations will be different from the realization. Thus, at times of rare disasters, uncertainty goes up and it is more likely that the UIRP does not hold, while, during normal times, uncertainty decreases and it is more likely that the UIRP holds, consistently with our empirical results. However, our uncertainty index more broadly captures not only rare disasters but also any deviation between agents' expectations of exchange rate fluctuations and their realizations. In addition, our robustness results to using the VIX as a measure of uncertainty are consistent with Brunnermeier et al. (2009).[4]

The third strand is the literature on uncertainty. Several recent

[3]Other theoretical explanations of the lack of empirical validity of the UIRP include Colacito and Croce (2011), Verdelhan (2010) and Bacchetta and van Wincoop (2010). On the one hand, Colacito and Croce (2011) consider long-run risks models as a potential explanation of several exchange rate puzzles, including UIRP, where the long run risk is related to a small predictable component in consumption growth. On the other hand, Verdelhan (2010) shows that habit models with time-varying risk aversion and procyclical real interest rates can also theoretically generate time-varying risk premia in currency markets. However, Verdelhan (2010) shows that the exchange rates series simulated by his calibrated model are too volatile and too much correlated with consumption growth shocks. Similarly, Bacchetta and van Wincoop (2010) discuss and calibrate a theoretical model that attributes deviations from UIRP to infrequent portfolio decisions.

[4]In unreported results we investigated whether the failure of UIRP is more likely to be caused by expectation errors or by risk premia using Froot and Frankel (1989) decomposition. The failure seems more likely to derive from expectation error for Switzerland and from risk premia for Canada, Japan and the UK; in the case of Europe, both are equally likely.

papers have analyzed the effects of uncertainty on the economy; for example, Bloom (2009), among others, has measured uncertainty as the volatility in financial markets. In this paper, we use survey forecasts to measure uncertainty, similarly to Ozturk and Sheng (2018), who use survey forecasts to measure global and country-specific macroeconomic uncertainty, and Rossi et al. (2016), who use survey density forecasts to understand the sources of macroeconomic uncertainty. However, differently from them, we focus on exchange rate uncertainty. The literature on the relationship between exchange rates and uncertainty is more limited. Berg and Mark (2018) and Mueller et al. (2017), for example, study the relationship between trading strategies in exchange rate markets and uncertainty. The former study the exposure of carry-trade currency excess returns to global fundamental macroeconomic risk. Their measure of global macroeconomic uncertainty, defined as the cross-country high-minus-low conditional skewness of the unemployment gap, is a factor priced in currency excess returns. Mueller et al. (2017) instead study whether trading strategies of going short on one currency and long on other currencies exhibits significantly larger excess returns on FOMC announcement days, and find that the excess returns are higher the higher is uncertainty about monetary policy. Menkhoff et al. (2012) propose a new risk factor capable of explaining the cross-section of excess returns: the global foreign exchange volatility risk; they find that high interest rate currencies are negatively related to global foreign exchange volatility, and thus deliver low returns when volatility is unexpectedly high, at times when low interest rate currencies provide positive returns. Belke and Kronen (2019) analyze the role of uncertainty in explaining exchange rate bands of inaction and their effects on exports. Similarly to these contributions, our paper also studies the effects of uncertainty in exchange rate markets but focuses instead on explaining the UIRP puzzle, as opposed to explaining larger excess returns in cross-section carry-trade strategies or fluctuations in exports.

This paper is organized as follows. The next section describes the data used in this study and Section 3 discusses the exchange rate uncertainty index that we use. Section 4 revisits the empirical evidence on UIRP in our sample, while Section 5 investigates whether deviations from UIRP can be explained by uncertainty. Section 6 performs robustness analyses using other uncertainty indices, while Section 7 discusses results for a larger set of countries using uncertainty indices based on random walk forecast errors. Section 8 concludes.

1.2 Data

We collect monthly data from November 1993 (1993:M11) to January 2015 (2015:M1) on exchange rates, three-month Euro LIBOR rates, and the uncertainty measures. In our benchmark results, we focus on industrialized countries and consider five currency pairs: the Swiss franc, the Canadian dollar, the British pound, the Japanese yen, and the Euro against the US dollar. We focus on exchange rates for industrialized countries for which the survey expectations necessary to construct our uncertainty index are available. Robustness results for additional countries are discussed in Section 7. The period has been chosen based on the availability of the uncertainty index. In fact, the data on our uncertainty measure start in 1993:M11 and end in 2015:M1 for all currencies except the Euro (for the Euro it begins on 2001:M7)—see below for more details on the uncertainty measure. The data on the exchange rates for the five currency pairs are from WM/Reuters. The exchange rates are values of the national currencies relative to one US dollar. For the interest rates, we collect monthly data on three-month Euro LIBOR rates for the respective five countries and the United States. The data are from the Financial Times. All data have been collected via Datastream. More details (including mnemonics) are provided in Table 1.1, which also includes a description of the additional data we use in the robustness analysis to the larger set of countries.

Table 1.1: Data Description

Country	Period	Code	Description
		Exchange rates:	
Switzerland	1994M1:2015M1	SWISSF$	SWISS FRANC TO US $ - EXCH. RATE
Canada	1994M1:2015M1	CNDOLL$	CANADIAN $ TO US $ - EXCH. RATE
United Kingdom	1993M11:2015M1	UKDOLLR	UK £ TO US $ - EXCH. RATE
Japan	1993M11:2015M1	JAPAYE$	JAPANESE YEN TO US $ - EXCH. RATE
EU	2001M7:2015M1	EUDOLLR	EURO TO US $ - EXCH. RATE
South Africa	1997M4:2016M10	COMRAN$	SOUTH AFRICA RAND TO US $ - EXCH. RATE
Australia	1997M4:2016M10	AUSTDOI	AUSTRALIAN $ TO US $ - EXCH. RATE
Norway	1997M4:2016M10	NORKRO$	NORWEGIAN KRONE TO US $ - EXCH. RATE
Sweden	1997M4:2016M10	SWEKRO$	SWEDISH KRONA TO US $ - EXCH. RATE
Denmark	1997M4:2016M10	DANISH$	DANISH KRONE TO US $ - EXCH. RATE
New Zealand	1997M4:2016M10	NZDOLLI	NEW ZEALAND $ TO US $ - EXCH. RATE
		Interest rates:	
Switzerland	1993M11:2015M1	ECSWF3M	Euro LIBOR 3-month rate, middle rate
Canada	1993M11:2015M1	ECCAD3M	Euro LIBOR 3-month rate, middle rate
United Kingdom	1993M11:2015M1	ECUKP3M	Euro LIBOR 3-month rate, middle rate
Japan	1993M11:2015M1	ECJAP3M	Euro LIBOR 3-month rate, middle rate
EU	2001M7:2015M1	ECEUR3M	Euro LIBOR 3-month rate, middle rate
United States	1993M11:2015M1	ECUSD3M	Euro LIBOR 3-month rate, middle rate
South Africa	1997M4:2016M10	ECSAR3M	Euro LIBOR 3-month rate, middle rate
Australia	1997M4:2016M10	ECAUD3M	Euro LIBOR 3-month rate, middle rate
Norway	1997M4:2016M10	ECNOR3M	Euro LIBOR 3-month rate, middle rate
Sweden	1997M4:2016M10	ECSWE3M	Euro LIBOR 3-month rate, middle rate
Denmark	1997M4:2016M10	ECDKN3M	Euro LIBOR 3-month rate, middle rate
New Zealand	1997M4:2016M10	ECNZD3M	Euro LIBOR 3-month rate, middle rate

Note: The table reports mnemonics and descriptions for our data. All interest rates are "middle rates". All exchange rate data are from WM/Reuters, while all interest rate data are from FT/Reuters.

1.3 THE EXCHANGE RATE UNCERTAINTY INDEX

Regarding uncertainty, several methodologies and strategies to construct uncertainty indices are available. Bloom (2009) proposes to measure macroeconomic uncertainty using the volatility in stock prices, while Baker et al. (2016) propose a measure of macroeconomic policy uncertainty. Since we are interested in exchange rate uncertainty, their measures are not the most appropriate. Jurado et al. (2015) and Ludvigson et al. (2021) propose to measure uncertainty as the time-varying volatility of forecast errors in predicting macroeconomic and financial variables, while Scotti (2016) measures uncertainty as macroeconomic news announcements.

The uncertainty series that we construct are similar in spirit to Jurado et al. (2015) but they are obtained using the methodology in Rossi and Sekhposyan (2015). Rossi and Sekhposyan's (2015) uncertainty index is constructed by comparing the realized forecast error of the target variable with the unconditional forecast error distribution of the same variable. The intuition is that, if the observed realization of the forecast error is in the tails of the distribution, then the realization was very difficult to predict; thus, such an environment is deemed very uncertain. One of the advantages of the Rossi and Sekhposyan (2015) index is that it allows for asymmetry: in other words, it can separately distinguish between uncertainty due to unexpectedly high and low exchange rates - an important feature that is not shared by uncertainty indices based on the volatility of forecast errors.[5]

We construct the exchange rate uncertainty index based on fixed-horizon forecast errors from surveys conducted by Consensus Economics.[6] The uncertainty index is monthly and the forecast horizon is three months; therefore, the interest rate differential is based on three-month interest rates. Let the bilateral nominal exchange rate between a country and the US at time t be denoted by S_t and let $s_t = \ln(S_t)$. Furthermore, let the h-step-ahead forecast error for the rate of growth of the exchange rate between time t and time $t+h$ be denoted by $e_{t+h} = (s_{t+h} - s_t) - E_t(s_{t+h} - s_t)$, and its unconditional forecast error distribution be denoted by $p(e)$. Rossi and Sekhposyan's (2015) index is based on the cumulative density of forecast errors evaluated at the realized forecast error, e_{t+h}: $U_{t+h} = \int_{-\infty}^{e_{t+h}} p(e)\mathrm{d}e$. A large value of the index indicates a realization of the exchange rate that is very different from the expected value. In particular, a realized value much bigger (smaller) than the expected value, which is 0.5 measures a positive (negative) "shock". The overall exchange

[5] We perform a robustness analysis to using alternative uncertainty indices in Section 6.

[6] We use the average forecasts from a sample of approximately 250 professional forecasters.

rate uncertainty index that does not distinguish between positive and negative "shocks" is:

$$U^*_{t+h} = \frac{1}{2} + \left| U_{t+h} - \frac{1}{2} \right|$$

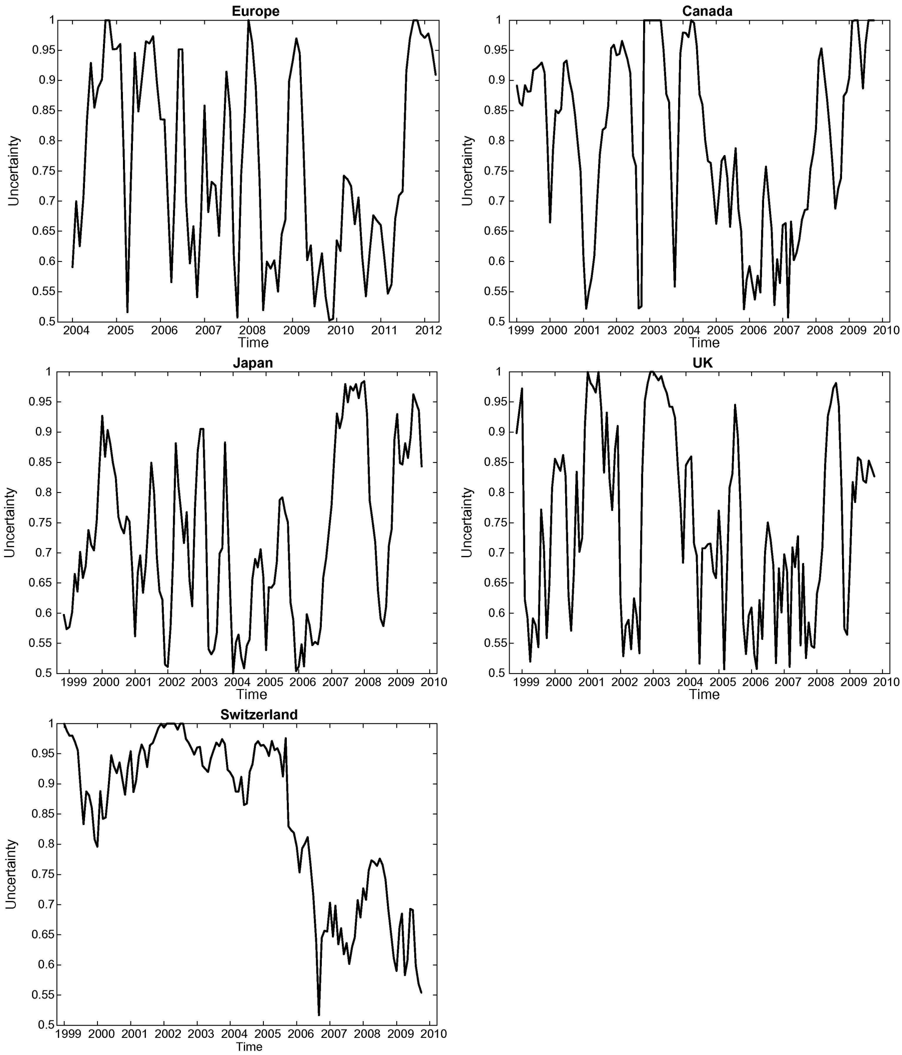
Values of U^*_{t+h} close to unity indicate high uncertainty, while values close to 0.5 indicate low uncertainty.

Figure 1.1 plots the exchange rate uncertainty indices for the countries in our sample. The time series fluctuations of the uncertainty indices are consistent with several events that affected these countries over time. For example, focusing on the EU, the two periods of high uncertainty during the latest financial crisis are clearly visible; they are related to the two recent recessions in the Euro-area: the first from 2008:Q1 to 2009:Q2 and the second from 2011:Q3 to 2013:Q1. In particular, the Euro debt crisis shows up as an upward trend in uncertainty in the EU since mid-2011. A similar pattern affects the UK during the same period. Note also the upward trend in uncertainty visible in Canada during the recent US financial crisis starting in 2007. Finally, another notable event taking place in 2006 is the Bank of Japan raising interest rates for the first time in several years, which might have caused the drastic increase in uncertainty around mid-2006.

1.4 REVISITING UNCOVERED INTEREST RATE PARITY

Uncovered interest rate parity (UIRP) states that in a world of perfect foresight and a nominal bilateral exchange rate S_t. investors can buy $1/S_t$ units of foreign bonds using one unit of the home currency, where S_t denotes the price of foreign currency in terms of home currency. Suppose the foreign bond pays one unit plus the foreign interest rate between time t and $t+h$, i^*_{t+h}, where h is the horizon of the investment. At the end of the period, the foreign return can be converted back into the home currency with a value of $S_{t+h}\left[\left(1+i^*_{t+h}\right)/S_t\right]$ in expectation.

Figure 1.1: Exchange Rate Uncertainty Indices



Note: The figure plots the overall exchange rate uncertainty index for the benchmark countries in our sample.

In the absence of transaction costs, by no-arbitrage, this return must be in expectation equal to the return of the home bond, $(1 + i_{t+h})$. Therefore, $\left(1 + i^*_{t+h}\right) E_t \left(S_{t+h}/S_t\right) = (1 + i_{t+h})$, where $E_t(\cdot)$ denotes the expectation at time t. By taking logarithms and ignoring Jensen's inequality, the uncovered interest rate parity equation follows directly:

$$E_t \left(s_{t+h} - s_t\right) = \alpha + \beta(i_{t+h} - i^*_{t+h}) \tag{1.1}$$

where the UIRP parameters α and β have the theoretical values $\alpha = 0$ and $\beta = 1$.

Overall, the empirical evidence is not favourable to UIRP—see Rossi (2013) for a recent survey. It is well-known that the constant, α, is different from zero, and the slope, β, is either negative or close to zero, or sometimes positive and very large in magnitude. Similarly, the empirical evidence is equally not supportive of UIRP in out-of-sample forecast evaluation; in fact, it is also well-known, since the early work by Meese and Rogoff (1983a, 1983b, 1988), that equation (1) does not forecast exchange rates out-of-sample better than the random walk. The same result was reinforced by Cheung et al. (2005),Alquist and Chinn (2008) and Chinn and Quayyum (2013). Slightly more positive findings have been reported by Clark and West (2006) at short-horizons; however, as Rossi (2013) pointed out, the reason for the positive findings in Clark and West (2006) is mainly due to the use of an alternative test of predictive ability.[7]

We start by confirming the existing findings in the literature, namely that UIRP does not hold in the data. Panel A in Table 1.2 estimates regression (1) in our sample and shows that, for several countries, β is very small, and in the case of Switzerland, Canada and Japan, it is negative and statistically significantly different from one. Only for the EU and the UK the slope is positive and statistically indistinguishable

[7] One could potentially consider forecasting real exchange rates using real interest rates; however, the survey forecasts are for the nominal, not the real, exchange rate—which nevertheless is what is considered in the aforementioned literature.

from its theoretical value under the UIRP. The constant instead is small and insignificantly different from zero for most countries.[8]

Our results are similar to those in the literature, except that our estimates are slightly smaller than those reported in the earlier literature. For instance, Chinn and Quayyum (2013) use quarterly data spanning 1975:Q1–2011:Q4 for the same set of currency pairs, and they find slope estimates ranging from -1.85 to -2.25 with the exception of the Canadian dollar, whose slope is -0.17. However, a detailed analysis reveals that the large negative values are driven by sample selection. Firstly, the rolling-window estimates which we report later in the paper show that the slope coefficients have been increasing over time: our sample is shorter than, e.g. Chinn and Quayyum (2013), and in particular, it omits the seventies and the eighties; the latter are decades with large deviations from UIRP according to Lothian and Wu (2011).[9] Secondly, if we consider the sample up to 2011:M10, that is, omitting the last four years to better match the sample used in Chinn and Quayyum (2013), the estimates become negative for four countries out of five and the negative coefficients are larger in magnitude in absolute value (see Table 1.2, Panel B).

A comparison of the results in the two panels in Table 1.2 also points out another important empirical feature of UIRP: the well-known fact that the UIRP parameters are unstable over time. For example, note how the slope coefficient for the Euro data turns from positive to negative depending on the sample, and how its magnitude varies in Japanese data. Rossi (2006) investigated the instability of the parameters in exchange rate monetary models (that is, models that explain exchange rate fluctuations using output, money and interest rate differentials) and found ample evidence of instabilities based on conventional tests

[8] The 95% confidence intervals reported in parentheses in this paper are based on a Newey and West (1987) HAC estimator for the covariance matrix, using a truncation lag equal to two.

[9] Our sample is shorter since it is determined by the availability of the uncertainty index.

of parameter instability. Furthermore, she argued that the empirical rejections of the monetary exchange rate model could be due to parameter instabilities; in fact, by using alternative and more powerful tests that evaluate Granger-causality robust to instabilities, she found that monetary models' predictors helped forecasting exchange rates at some point in time. However, she did not consider the UIRP in her analysis, so it is important to investigate whether UIRP fails in the data regardless of the presence of instabilities in the data, a question we explore in the rest of this section.

Table 1.2: Traditional UIRP Regressions

Country:	Panel A. Full Sample		Panel B. Sub-sample ending in 2011	
	α	β	α	β
Switzerland	-0.01	-0.59	-0.023	-0.817
	(-0.028;-0.002)	(-1.090;-0.100)	(-0.039;-0.007)	(-1.382;-0.252)
EU	-0.007	0.391	-0.004	-0.351
	(-0.016;0.002)	(-0.576;1.358)	(-0.016;0.007)	(-1.178;0.476)
Canada	-0.001	-0.196	-0.003	-0.383
	(-0.007;0.004)	(-0.706;0.312)	(-0.010;0.003)	(-0.906;0.140)
UK	-0.004	0.378	-0.005	0.410
	(-0.012;0.004)	(-0.513;1.271)	(-0.014;0.004)	(-0.502;1.324)
Japan	-0.002	-0.118	-0.023	-0.585
	(-0.015;0.011)	(-0.533;0.296)	(-0.036;-0.010)	(-0.988;-0.181)

Note: The table reports estimates of UIRP regressions (and 95% confidence intervals in parentheses) in the full sample as well as a sub-sample ending in 2011.

We first investigate the stability of the UIRP parameters over time by plotting their estimates in rolling windows over ten years of data in the top plots in Figure 1.2 and Figure 1.3 (a-e). The figures confirm the presence of instabilities throughout the sample that we consider. For Canada, the value of the constant is small throughout the sample, but the slope value changes significantly from negative to positive. The slope changes drastically for the EU as well, ranging from values close to zero at the beginning of the sample to almost four towards the end of the sample. In the case of Japan, the coefficient is close to zero for almost all of the sample except the beginning and the end. Switzerland and the UK are two other countries where the slope changes drastically from negative to large and positive values. For the latter country, the constant also is very unstable, taking both positive and negative values depending on the sample period.

We investigate more formally whether instabilities affect UIRP in Tables 3–5. We consider the following regression:

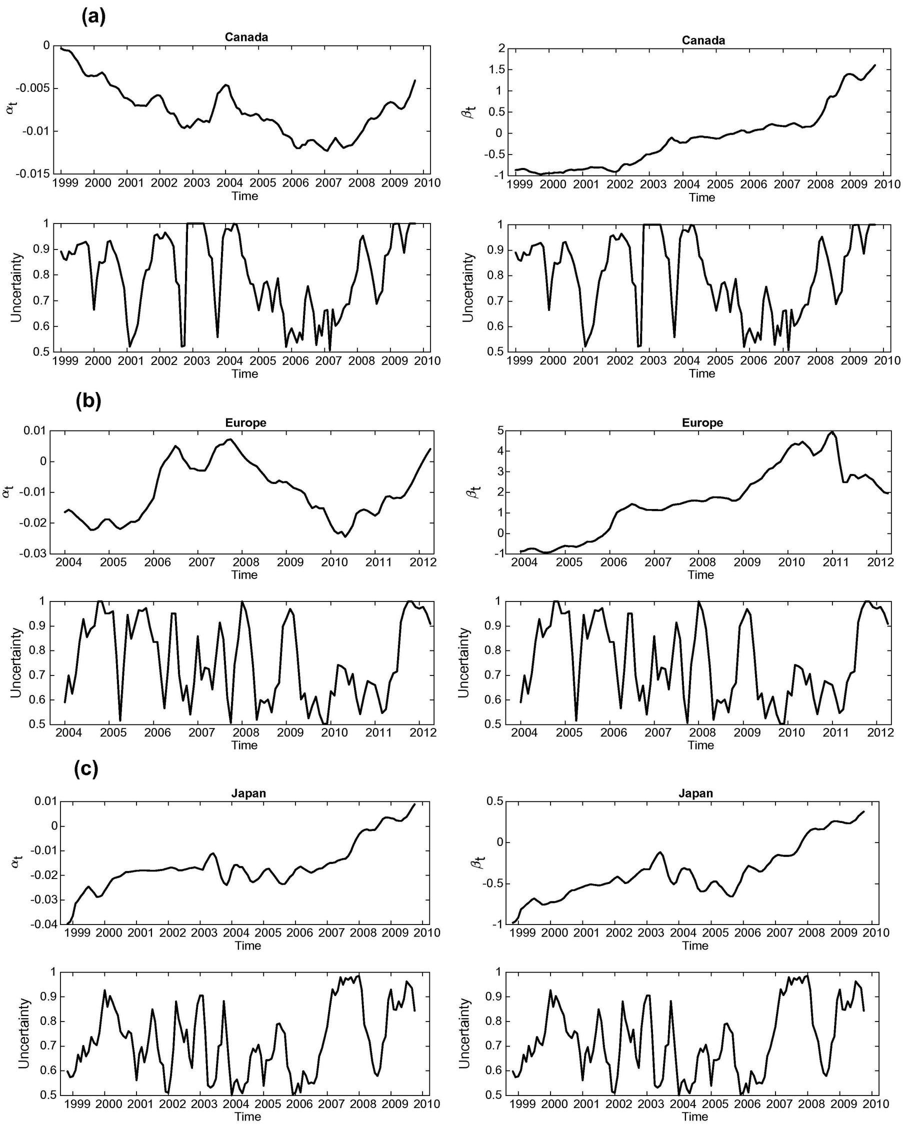
$$E_t \left(s_{t+h} - s_t \right) = \alpha_t + \beta_t \left(i_{t+h} - i^*_{t+h} \right) \tag{1.2}$$

where the constant, or the slope parameter, or potentially both, might be time-varying. The absence of time variation manifests itself in constant parameters, that is: $\alpha_t = \alpha$ and/or $\beta_t = \beta$. We test parameter stability using a battery of tests, including Andrews's (1993) Quandt Likelihood Ratio test (QLR), Andrews and Ploberger's (1994) Exponential-Wald (Exp-W), as well as Nyblom's (1989) test. The tests differ depending on the type of instability they allow for; in particular, Andrews (1993) and Andrews and Ploberger (1994) allow for a one-time structural change, while Nyblom (1989) considers smoother and more frequent changes.

Table 1.3 reports results for testing the joint stability in both the constant and the slope parameters. It is clear that the stability is overwhelmingly rejected, with p-values that are zero in all cases. We then investigate whether the instability is more pronounced in the constant or in the slope. Table 1.4 reports tests of stability on the constant. The table shows that the constant is unstable for most countries except the UK. Table 1.5 reports tests of stability on the slope; the table shows that the slope is unstable for all countries, including the UK.

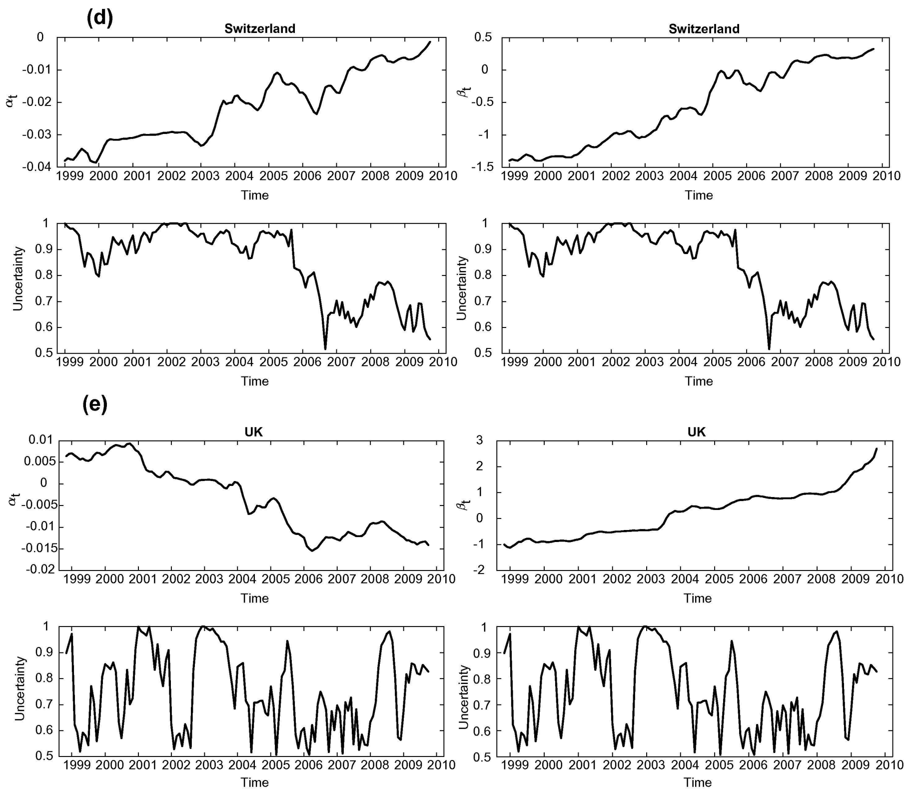
Since the parameters are time-varying, the UIRP tests presented in Table 1.2 are invalid, as they assume stability in the parameters. Therefore, we complement the analysis with tests that are robust to

Figure 1.2: Exchange Rate Uncertainty Indices and UIRP Coefficients. Panels (a)-(c)



Note: The top plots in each figure plot the UIRP coefficients estimated in rolling windows (the constant is depicted on the left and the slope on the right). The bottom plots in each figure plot the overall exchange rate uncertainty index.

Figure 1.3: Exchange Rate Uncertainty Indices and UIRP Coefficients. Panels (d)-(e)



Note: The top plots in each figure plot the UIRP coefficients estimated in rolling windows (the constant is depicted on the left and the slope on the right). The bottom plots in each figure plot the overall exchange rate uncertainty index.

Table 1.3: Instability Tests: Joint Test on α and β

Country		QLR	Exp-W	Nyblom
Switzerland	Test statistic	39.08	15.11	3.55
	P-value	0	0	0
EU	Test statistic	35.69	13.98	3.27
	P-value	0	0	0
Canada	Test statistic	24.54	9.44	2.44
	P-value	0	0	0
UK	Test statistic	50.09	19.90	1.98
	P-value	0	0	0
Japan	Test statistic	44.52	18.10	4.5
	P-value	0	0	0

Note: The table reports joint tests of parameter instabilities on the two UIRP regression coefficients.

parameter instabilities. In particular, we implement the Exp-W*, Mean-W*, Nyblom* and QLR* tests proposed by Rossi (2005), which are valid to test the UIRP conditions that $\alpha_t = 0$ and $\beta_t = 1$ even in the presence of time-variation in the parameters.[10] Tables 6–8 show that the results in Table 1.2 are robust. In particular, Table 1.6 shows that both parameters are significantly different from the values predicted by the UIRP; Tables 7 and 8 report results for the constant and the slope separately, and show that the rejections are mostly due to the fact that the slope is different from unity, especially for Canada, the UK and Japan.[11]

[10]The difference among the Exp-W*, Mean-W*, QLR* and Nyblom* tests is, again, that they focus on different types of instabilities. In particular, the first three focus on the case of a one-time structural change while Nyblom* allows smoother and more frequent changes.

[11]Note that, in Table 1.7, the Exp-W* test does not reject for some countries

Table 1.4: Instability Tests: Test on the Constant α

Country		QLR	Exp-W	Nyblom
Switzerland	Test statistic	23.73	7.38	0.67
	P-value	0	0	0.15
EU	Test statistic	34.06	13.52	1.98
	P-value	0	0	0
Canada	Test statistic	16.40	4.76	0.86
	P-value	0	0	0.08
UK	Test statistic	3.15	0.54	0.17
	P-value	0.81	0.83	0.85
Japan	Test statistic	51.40	21.00	1.58
	P-value	0	0	0

Note: The table reports tests of parameter instabilities on the constant coefficient in the UIRP regressions.

The analysis in this section shows that the coefficients estimated in UIRP regressions are very unstable over time and that UIRP does not hold in the data, regardless of the presence of instabilities. However, the analysis does not shed light on why there are time-varying deviations from UIRP. The next section will tackle this important question.

1.5 Can Uncertainty Explain UIRP Deviations?

The previous section has confirmed the existence of two important puzzles in the empirical literature in international finance: UIRP coefficients are both different from their theoretical values and unstable over time. This

while the Mean-W*, Nyblom* and QLR* tests do reject. The reason why the tests disagree is that they consider different types of instabilities: the Nyblom* test, for example, has more power when parameters are smoothly time-varying.

Table 1.5: Instability Tests: Test on the Slope β

Country		QLR	Exp-W	Nyblom
Switzerland	Test statistic	26.81	8.74	1.75
	P-value	0	0	0
EU	Test statistic	45.34	18.88	3.46
	P-value	0	0	0
Canada	Test statistic	27.28	10.68	2.42
	P-value	0	0	0
UK	Test statistic	26.44	8.54	1.06
	P-value	0	0	0.04
Japan	Test statistic	26.66	8.92	1.18
	P-value	0	0	0.02

Note: The table reports tests of parameter instabilities on the slope coefficient in the UIRP regressions.

paper tries to offer an explanation to both these puzzles by arguing that uncertainty is one of the reasons explaining the empirical invalidity of the UIRP; that the coefficients in UIRP regressions are more likely to be close to the values predicted by UIRP in times when uncertainty is low; and that their time variation is, at least partly, due to the fact that UIRP holds when uncertainty is low but does not when uncertainty is high.

As discussed in the introduction, a typical explanation for the UIRP puzzle is the existence of time-varying risk premia; but what generates time-varying risk premia? The most recent theoretical explanations include rare disasters (Farhi and Gabaix, 2016; Brunnermeier et al., 2009), habits (Verdelhan, 2010) or long run risks related to a small predictable component in consumption growth (Colacito and Croce, 2011). Our empirical results provide potential empirical support in

Table 1.6: Granger-Causality Tests: Joint Test on α and β

Country		Exp-W*	Mean-W*	Nyblom*	QLR*
Switzerland	Test statistic	68.91	121.76	31.93	146.45
	P-value	0	0	0	0
EU	Test statistic	23.09	26.05	6.02	54.09
	P-value	0	0	0	0
Canada	Test statistic	57.23	89.39	16.28	120.36
	P-value	0	0	0	0
UK	Test statistic	44.90	48.06	8.28	98.60
	P-value	0	0	0	0
Japan	Test statistic	77.34	129.91	31.96	163.74
	P-value	0	0	0	0

Note: The table reports tests of UIRP robust to parameter instabilities. The tests are performed jointly on both the constant and the slope in the UIRP regressions.

favour of Farhi and Gabaix (2016) in the following sense. An unexpected rare disaster that realizes in the data increases our uncertainty index; conversely, even a situation where agents expect a rare disaster and it does not realize in the data will show up as an increase in our uncertainty index, as the expectations will be different from the realization. Thus, at times of rare disasters, uncertainty goes up and it is more unlikely that the UIRP does not hold, while, during normal times, uncertainty decreases and it is more likely that UIRP holds, consistently with our empirical results. However, our uncertainty index includes not only rare disasters but also any deviation between agents' expectations of exchange rate fluctuations and their realizations.

A visual analysis of the relationship between uncertainty and the rolling estimates of the UIRP parameters is presented in Figures 1.2 and

Table 1.7: Granger-Causality Tests: Joint Test on the Constant α

Country		Exp-W*	Mean-W*	Nyblom*	QLR*
Switzerland	Test statistic	11.19	16.55	3.52	29.48
	P-value	0	0	0.02	0
EU	Test statistic	11.32	13.16	3.02	29.36
	P-value	0	0	0.03	0
Canada	Test statistic	3.95	4.10	0.68	14.14
	P-value	0.12	0.40	0.55	0.05
UK	Test statistic	1.12	1.81	0.94	4.44
	P-value	0.82	0.85	0.40	0.82
Japan	Test statistic	17.31	6.84	1.75	43.65
	P-value	0	0.12	0.15	0

Note: The table reports tests of UIRP robust to parameter instabilities. The tests are performed on the constant coefficient in the UIRP regression.

1.3. The top panels in Figures 1.2 and 1.3 show the rolling estimates of the parameters while the bottom panels display the uncertainty index for each country; the bottom panels plot the exchange rate uncertainty index, U^*_{t+h}. The figures show that there is a correlation between uncertainty and the UIRP coefficients for most countries: when uncertainty is substantially high, there are more deviations from UIRP, both in terms of deviations of α from zero as well as deviations of β from unity. For example, the case of Switzerland (depicted in Figure 1.3d) is emblematic: the negative values of the slope and the constant are clearly visible at the beginning of the sample, and that is also when uncertainty is the highest. Similarly, in the case of the UK and Canada (depicted in Figure 1.3e and Figure 1.2a, respectively), the slope approaches unity around 2005–2008, which is exactly when uncertainty is the lowest, and very different from unity both at the beginning (when the slope is negative)

Table 1.8: Granger-Causality Tests: Joint Test on the Slope β

Country		Exp-W*	Mean-W*	Nyblom*	QLR*
Switzerland	Test statistic	50.58	85.23	36.43	110.40
	P-value	0	0	0	0
EU	Test statistic	19.34	18.55	3.33	46.76
	P-value	0	0	0.02	0
Canada	Test statistic	55.25	86.58	16.13	115.60
	P-value	0	0	0	0
UK	Test statistic	19.81	18.93	4.45	48.26
	P-value	0	0	0	0
Japan	Test statistic	42.50	60.26	24.18	93.75
	P-value	0	0	0	0

Note: The table reports tests of UIRP robust to parameter instabilities. The tests are performed on the slope coefficient in the UIRP regression.

and towards the end of the sample (when the slope is positive and large), when uncertainty is the highest. For the EU, depicted in Figure 1.2b, uncertainty is high for most of the sample we consider. Finally, in the case of Japan (depicted in Figure 1.2c) too, both the slope and the intercept are negative at the beginning of the sample, when the uncertainty is often at high levels.

To investigate more formally whether uncertainty can explain the UIRP puzzle, we estimate the following regression:

$$E_t\left(s_{t+h}-s_t\right)=\alpha_1\left(1-d_t\right)+\beta_1\left(1-d_t\right)\left(i_{t+h}-i^*_{t+h}\right)+\alpha_2 d_t+\beta_2 d_t\left(i_{t+h}-i^*_{t+h}\right) \tag{1.3}$$

where d_t is a dummy variable equal to one if the uncertainty is exceptionally high. Since the uncertainty indices are quite volatile, we smooth them using the same rolling window that we used to estimate the parameters in the UIRP regression, equal to ten years of data. Time periods of high uncertainty are identified by situations in which uncertainty (U^*_{t+h}) is in the upper quantile of its distribution, i.e. we identify high uncertainty periods with sub-samples with the 25% highest values of uncertainty.

Table 1.9 reports the estimates of equation (3). The table shows that the empirical evidence in favour of UIRP is weakest in periods where uncertainty is exceptionally high, and substantially stronger in periods where uncertainty is around normal values. More in detail, we note that, in the case of Switzerland, both values of α_2 and β_2 are negative and large in absolute value; since α_2 and β_2 are the constant and slope of the UIRP in periods of high uncertainty, the regression results confirm the existence of large deviations from UIRP when uncertainty is exceptionally high. However, in periods of low uncertainty, both α_1 and β_1 are closer to their theoretical values and insignificantly different from them. Japan is another case where the slope switches from negative values (and significantly different from unity) during periods of high uncertainty, to positive values close to unity (and statistically insignificantly different from unity). In Canada, again, the slope is negative and close to zero in periods of high uncertainty, while it becomes positive and closer to unity in periods of low uncertainty; the constant also gets closer to its theoretical value of zero in periods of low uncertainty. In the case of the EU and the UK, the uncertainty state also drives the slope coefficient closer to its theoretical value; in all cases, the point estimates are more precisely estimated in periods of low uncertainty.

Note that our results have direct implications for the risk premium. In fact, let $R_{t+h,t} = (s_{t+h} - s_t) - i_{t+h} - i^*_{t+h}$ denote the risk premium. The regression:

$$E_t(R_{t+h}) = \alpha_1 (1 - d_t) + \beta_1 (1 - d_t) \left(i_{t+h} - i^*_{t+h}\right) + \alpha_2 d_t + \beta_2 d_t \left(i_{t+h} - i^*_{t+h}\right)$$

yields exactly the same coefficients α_1 and α_2 (and their confidence

Table 1.9: UIRP and Exchange Rate Uncertainty

Country	Low Uncertainty		High Uncertainty	
	α_1	β_1	α_2	β_2
Switzerland	0.001	0.469	-0.034	-9.389
	(-0.017;0.019)	(-0.274;1.213)	(-0.074;0.006)	(-19.342;0.564)
EU	-0.001	1.918	-0.012	3.445
	(-0.015;0.013)	(0.188;3.649)	(-0.081;0.056)	(-3.518;10.407)
Canada	-0.005	1.632	-0.009	-0.114
	(-0.015;0.005)	(0.525;2.738)	(-0.041;0.024)	(-4.606;4.379)
UK	-0.007	0.332	-0.033	6.951
	(-0.017;0.003)	(-0.485;1.150)	(-0.067;0.000)	(4.754;9.147)
Japan	0.009	0.739	-0.002	-0.331
	(-0.007;0.025)	(0.089;1.390)	(-0.030;0.026)	(-1.186;0.523)

Note: The table reports parameter estimates in equation (3), where the measure of uncertainty is overall exchange rate uncertainty(95% confidence intervals in parentheses).

intervals) as the regression in equation (3), and the slope coefficients β_1 and β_2 are exactly the same as the estimated slope coefficients we report in equation (3) minus one (and similarly for their confidence intervals). Thus, the results in equation (3) directly tell us that risk premia are more correlated with interest rate differentials during periods of high uncertainty than during low uncertainty, and significantly so for Switzerland and Japan. Notice that risk premia are never significantly correlated to interest rate differentials during periods of low uncertainty for any of the countries.

Finally, we investigate whether uncertainty can help explain UIRP deviations directly by estimating the following regression:

$$E_t\left(s_{t+h} - s_t\right) = \alpha + \beta\left(i_{t+h} - i^*_{t+h}\right) + \gamma U^*_{t+h} \tag{1.4}$$

and testing whether γ is significantly different from zero using the tests robust to instabilities. The results are reported in Table 1.10. Indeed, the table shows that uncertainty does significantly help in explaining deviations from UIRP for all countries.

Table 1.10: Does Uncertainty Granger-Cause Exchange Rates?

Country		Exp-W*	Mean-W	Nyblom	QLR
Switzerland	Test statistic	68.91	121.70	31.93	146.40
	P-value	0	0	0	0
EU	Test statistic	23.09	26.05	6.02	54.09
	P-value	0	0	0	0
Canada	Test statistic	57.23	89.39	16.28	120.30
	P-value	0	0	0	0
UK	Test statistic	44.90	48.05	8.28	98.60
	P-value	0	0	0	0
Japan	Test statistic	77.34	129.90	31.96	163.70
	P-value	0	0	0	0

Note: The table reports results for test statistics robust to parameter instabilities. The statistics test whether uncertainty is a significant predictor in UIRP regressions in equation (4).

It is interesting to investigate whether time-variation in the UIRP can be explained by differences in monetary policy alone. Table 1.11 estimates the UIRP in the sub-sample of the zero-lower bound in the US (December 2008 to December 2014), a time period where the interest rate was close to zero and, hence, the traditional monetary policy prescription of lowering interest rates in the presence of the recession was infeasible. By comparing Table 1.11 with Table 1.2 it is clear that although for Switzerland and the EU the estimates of UIRP coefficients during the zero lower bound period are closer to their theoretical value than during the full sample, the same result does not hold for Canada, Japan and the UK.

Table 1.11: UIRP Regressions during the Zero-Lower Bound

Country	α_1	β_1
Switzerland	-0.004	1.047
	(-0.026;0.018)	(-3.206;5.299)
EU	-0.002	1.684
	(-0.019;0.014)	(-0.54;3.909)
Canada	-0.025	3.555
	(-0.054;0.004)	(-0.246;7.355)
UK	-0.004	0
	(-0.027;0.019)	(-5.01;5.01)
Japan	0.007	-2.003
	(-0.009;0.023)	(-5.427;1.42)

Note: The table reports estimates (and 95% confidence intervals in parentheses) of UIRP regressions in the zero lower bound sub-sample for the US, estimated to last between December 2008 and December 2014.

1.6 The Effects of Global Uncertainty

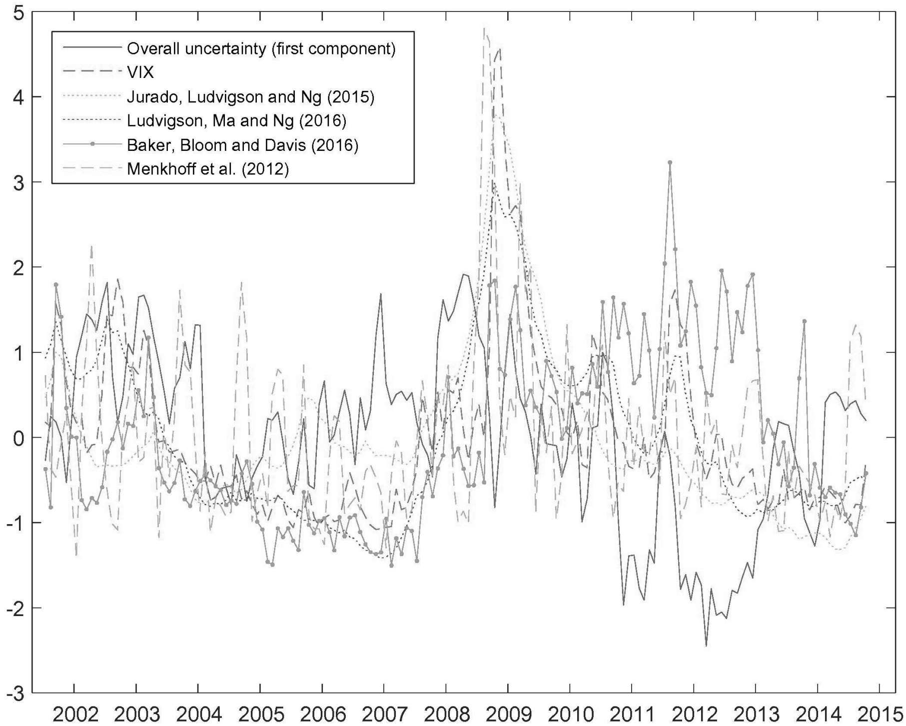
In the previous sections, we focused attention on indices that measure uncertainty in bilateral exchange rates, which is a relevant measure for our purposes since it proxies exchange rate uncertainty in financial markets. The uncertainty index we used was based on Rossi and Sekhposyan's (2015) methodology, whose advantage is that it can be easily tailored to measure uncertainty in any variable subject to the minimal requirement of availability of time series of forecast errors. Given the bilateral nature of the exchange rate data we used, the indices may include both global as well as country-specific idiosyncratic uncertainty. But which one is more relevant for explaining deviations from UIRP: global uncertainty or country-specific idiosyncratic uncertainty in financial markets? We attempt to answer this question in this section.

We construct an index of global uncertainty in financial markets by taking the common component of the Rossi and Sekhposyan (2015) uncertainty indices for the currency pairs we consider in Section 3[12], which captures global uncertainty in exchange rate financial markets, cleaned from any idiosyncratic or country-specific component. There are also many other uncertainty indices available in the literature that one could alternatively use, such as: the CBOE Volatility Index (VIX) (Bloom, 2009); the Jurado et al. (2015) macroeconomic uncertainty index; the Ludvigson et al. (2021) financial uncertainty index; and the Baker et al. (2016) economic policy uncertainty index. These alternative uncertainty indices are available mainly for the US and can be thought of as a measure of global macroeconomic and/or political uncertainty given the prominent role of the US on the international scene. We also consider the Menkhoff et al. (2012) global foreign exchange volatility risk measure. Figure 1.4 depicts all the global uncertainty indices - they are very correlated in the sample we focus on.

We estimate equation (3) using each one of these indices as a measure of global uncertainty in exchange rates, the economy or financial markets. The results are reported in Table 1.12. For all countries, in the case of the VIX, the Jurado et al. (2015) and the Ludvigson et al. (2021) uncertainty indices, the estimate of the slope coefficient on the interest rate differential gets closer to the theoretical value of unity during periods of low uncertainty while the coefficient can be quite different from its

[12]The common component is measured by the first principal component estimated with a factor model from all the bilateral exchange rate uncertainty indices.

Figure 1.4: Global Uncertainty Indices



Note: The figure depicts time series of global uncertainty indices: the first principal component of the bilateral exchange rate uncertainty indices described in Section 3, labelled "Overall uncertainty (first component)"; the VIX; the Jurado et al. (2015) macroeconomic uncertainty index; the Ludvigson et al. (2021) financial uncertainty index; the Baker et al. (2016) economic policy uncertainty index; and the Menkhoff et al. (2012) global foreign exchange volatility risk.

theoretical value in periods of high uncertainty.[13] So, in most cases, what matters is the global uncertainty. Results are similar for the Menkhoff et al. (2012) global foreign exchange volatility risk measure. The only exception is the Baker et al. (2016) measure for the case of Japan; the index predicts a negative slope for Japan during the periods of low uncertainty and a positive slope when uncertainty is high; however, the Baker et al. (2016) index captures economic policy uncertainty in the US, which contains information above and beyond global uncertainty in financial markets, including market reforms etc., and in some cases relevant only for US internal purposes, and thus may have little power to explain the UIRP in a country like Japan.

By comparing Panel E in Table 1.12 (where we use the principal component from our cross-section of bilateral exchange rate uncertainty indices) and Table 1.9 (where we use our country-specific bilateral exchange rate uncertainty index), we note that the principal component is not as effective in explaining time-varying UIRP deviations as the country-specific uncertainty indices. Thus, not only global shocks in international financial markets are important, but also country-specific idiosyncratic uncertainty shocks.

1.7 EXPLORING A LARGER SET OF COUNTRIES

The exchange rate uncertainty index described in Section 3 is based on survey forecast errors. On the one hand, using survey forecasts is desirable since it ensures that, if one is willing to make the realistic assumption that forecasters use all the available information when making their forecasts (including soft information from news), then the largest possible information set is used when constructing forecast errors; in

[13] The standard errors are quite large in periods of high uncertainty; so the confidence intervals typically contain the theoretical value of unity even in periods of high uncertainty, although the point estimate is typically further away from its theoretical value.

Table 1.12: UIRP and Alternative Uncertainty Indices

Country	Low Uncertainty		High Uncertainty		Low Uncertainty		High Uncertainty	
	α_1	β_1	α_2	β_2	α_1	β_1	α_2	β_2
	Panel A. VIX				Panel B. Jurado et al. (2015) Macroeconomic Uncertainty Index			
Switzerland	0.011	0.761	-0.009	7.045	0.007	0.637	-0.008	4.915
	(-0.006;0.029)	(0.015;1.508)	(-0.034;0.019)	(0.539;13.550)	(-0.009;0.023)	(-0.076;1.349)	(-0.039;0.023)	(-3.608;13.437)
EU	-0.001	1.867	-0.003	3.34	-0.002	1.864	-0.004	2.854
	(-0.016;0.014)	(0.361;3.373)	(-0.019;0.012)	(1.441;5.238)	(-0.016;0.011)	(0.176;3.551)	(-0.061;0.053)	(-3.281;8.526)
Canada	0.002	1.601	-0.040	3.145	-0.002	1.623	-0.024	2.28
	(-0.010;0.014)	(0.459;2.742)	(-0.067;-0.012)	(-0.646;6.935)	(-0.015;0.011)	(0.454;2.792)	(-0.042;-0.005)	(-1.004;5.563)
UK	-0.005	1.188	-0.013	0.309	-0.011	1.351	-0.016	4.119
	(-0.016;0.005)	(-0.202;2.578)	(-0.061;0.034)	(-9.284;9.901)	(-0.024;0.002)	(-0.048;2.749)	(-0.053;0.021)	(-3.152;11.389)
Japan	0.023	0.759	-0.005	5.556	0.026	0.846	-0.010	6.33
	(0.002;0.044)	(0.131;1.386)	(-0.019;0.008)	(1.876;9.237)	(0.006;0.046)	(0.234;1.458)	(-0.023;0.004)	(1.116;11.544)
	Panel C. Ludvigson et al.'s (2015) Financial Uncertainty				Panel D. Economic Policy Uncertainty Index			
Switzerland	0.005	0.451	-0.022	0.204	-0.008	0.139	0.017	2.848
	(-0.014;0.025)	(-0.452;1.355)	(-0.049;0.005)	(-0.821;1.228)	(-0.030;0.014)	(-0.732;1.009)	(-0.029;0.063)	(-9.289;14.985)
EU	-0.002	1.851	-0.002	3.267	-0.001	1.941	0.001	5.117
	(-0.017;0.013)	(0.309;3.392)	(-0.019;0.014)	(1.559;4.975)	(-0.016;0.014)	(0.560;3.322)	(-0.015;0.016)	(-2.002;12.237)
Canada	0.001	1.236	-0.026	0.455	-0.009	1.082	-0.014	3.345
	(-0.013;0.014)	(-0.157;2.629)	(-0.042;-0.011)	(-1.055;1.965)	(-0.022;0.004)	(-0.223;2.338)	(-0.102;-0.074)	(-6.923;13.613)
UK	-0.004	1.094	-0.022	2.423	-0.013	1.417	-0.005	1.818
	(-0.015;0.007)	(-0.334;2.522)	(-0.043;-0.001)	(-1.826;6.672)	(-0.026;0.001)	(-0.010;2.84405)	(-0.030;-0.021)	(-4.356;7.991)
Japan	0.019	0.726	-0.016	-0.372	-0.011	-0.104	0.046	5.214
	(0.001;0.037)	(0.085;1.368)	(-0.037;0.004)	(-1.033;0.289)	(-0.026;0.004)	(-0.592;0.384)	(0.006;0.085)	(-9.746;20.173)
	Panel E. Principal Component from Uncertainty Indices				Panel F. Global Foreign Exchange Volatility Risk			
Switzerland	0.001	0.443	-0.009	-1.052	-0.019	-1.777	-0.014	-1.257
	(-0.01;0.02)	(-0.27;1.15)	(-0.05;0.03)	(-12.7;10.6)	(-0.03;-0.01)	(-2.57;-0.99)	(-0.04;0.01)	(-3.64;1.13)
EU	-0.004	2.276	0.003	1.201	-0.006	0.319	0.001	3.951
	(-0.02;0.01)	(0.53;4.02)	(-0.02;0.03)	(-0.54;2.94)	(-0.02;0.01)	(-0.51;1.14)	(-0.03;0.03)	(1.25;6.66)
Canada	-0.003	1.802	-0.01	0.627	-0.012	-0.5	0.006	-0.755
	(-0.02;0.01)	(0.61;2.99)	(-0.04;0.02)	(-3.35;4.60)	(-0.02;-0.01)	(-1.56;0.56)	(-0.01;0.02)	(-2.27;0.76)
UK	-0.012	1.368	-0.011	3.06	-0.008	1.283	-0.023	0.774
	(-0.02;0.00)	(-0.03;2.77)	(-0.05;0.03)	(-4.62;10.7)	(-0.02;0.01)	(0.47;2.09)	(-0.04;-0.01)	(-0.20;1.75)
Japan	0.023	0.765	-0.008	4.577	0	0.307	0.005	-0.582
	(0.00;0.04)	(0.14;1.39)	(-0.02;0.01)	(-0.66;9.81)	(-0.01;0.01)	(-0.52;1.14)	(-0.01;0.02)	(-1.99;0.83)

Note: The table reports parameter estimates (and 95% confidence intervals in parentheses) in equation (3), where the measures of uncertainty are the VIX (Panel A), the Jurado et al. (2015) Macroeconomic Uncertainty Index (Panel B), the Ludvigson et al. (2015) Financial Uncertainty Index (Panel C), the Baker et al. (2016) Economic Policy Uncertainty Index (Panel D), our Global Uncertainy Index (Panel E) and the Menkoff et al. (2012) Global Foreign Exchange Volatility Risk (Panel F).

addition, the forecasts do not depend on any specific theoretical model of exchange rate fluctuations. On the other hand, survey-based uncertainty indices have the disadvantage that they can only be constructed when survey forecasts are available, which may substantially limit the set of countries that a researcher can analyze. However, if a researcher is interested in measuring uncertainty in countries where survey forecast errors are not available, it is still possible to construct an uncertainty index based on models' forecasts.

In this section, we construct exchange rate uncertainty indices based on random walk forecasts. Since Meese and Rogoff (1983a, 1983b, 1988), the random walk model has been considered the best benchmark when forecasting exchange rates (Rossi, 2013), and hence it is a good candidate for generating the uncertainty index. The random walk model sets $E\left(s_{t+h} - s_t\right) = 0$; the forecast errors, $s_{t+h} - s_t$, can then be used to construct the uncertainty index U^*_{t+h} as in Section 3. We calculate the overall uncertainty index and study UIRP in times of high and low uncertainty.

We start by considering the same set of countries that we considered in Section 5 to verify the robustness of the results. The results, reported in Table 1.13, support the main findings in Section 5: the empirical evidence in favour of UIRP is weakest in periods where uncertainty is exceptionally high and substantially stronger in periods where uncertainty is around normal values. For instance, the coefficient on the interest rate differential is positive and closer to unity when uncertainty is low for Switzerland, Canada and Japan, while it is negative or zero when uncertainty is high. In periods of low uncertainty, the slope coefficients of all countries get closer to their theoretical value (equal to one) relative to periods of high uncertainty.

We then extend our results to other countries for which survey forecasts and/or other uncertainty indices are not avail able. In particular, we extend our dataset to include Australia, Sweden, South Africa, Norway, New Zealand and Denmark; as before, the bilateral exchange rates are

Table 1.13: UIRP and Overall Uncertainty Based on the Random Walk

Country	Low Uncertainty		High Uncertainty	
Switzerland	0	1.091	-0.01	-0.22
	(-0.017;0.018)	(-0.034;2.217)	(-0.061;0.042)	(-1.848;1.409)
EU	-0.003	2.279	-0.004	3.091
	(-0.018;0.012)	(0.672;3.887)	(-0.02;0.011)	(-1.205;7.387)
Canada	-0.006	1.534	0.002	0.12
	(-0.019;0.008)	(0.093;2.975)	(-0.03;0.034)	(-3.498;3.739)
UK	-0.012	1.745	-0.012	3.06
	(-0.026;0.002)	(0.286;3.203)	(-0.039;0.014)	(-2.356;8.476)
Japan	0.01	0.895	-0.002	-0.355
	(-0.007;0.026)	(0.212;1.577)	(-0.035;0.031)	(-1.171;0.461)

Note: The table reports parameter estimates in equation (3), where the measure of uncertainty is the overall exchange rate uncertainty index constructed based on random walk forecast errors.

against the US dollar. This subset of countries includes both commodity and noncommodity currencies, both emerging and developed markets, and currencies of various degrees of historical volatility.

Firstly, Panel A in Table 1.14 revisits the empirical evidence for the UIRP relationship for these counties in the full sample. For all countries, the point estimate of the coefficient on the interest rate differential is far from one, and for all countries except New Zealand, we reject that it equals unity. In other words, the UIRP is violated for this set of countries as well.

We then calculate the uncertainty measure based on random walk forecast errors to investigate whether high uncertainty can explain the deviations from the UIRP. The results are reported in Table 1.14, Panels B and C. For all countries except Norway, the estimate of the slope in periods of low uncertainty is closer to the theoretical value than

Table 1.14: UIRP Regressions for Additional Countries

Country	Panel A. Full sample		Panel B. Low Uncertainty		Panel C. High Uncertainty	
Australia	0.006	-0.302	-0.057	2.252	0.009	-0.634
	(-0.008;0.021)	(-1.007;0.403)	(-0.126;0.012)	(-0.322;4.825)	(-0.033;0.051)	(-1.909;0.641)
Sweden	0.001	-0.219	-0.004	0.451	0.013	-0.789
	(-0.009;0.011)	(-0.831;0.394)	(-0.019;0.011)	(-0.476;1.377)	(-0.004;0.03)	(-2.043;0.464)
South Africa	0.062	-0.706	0.046	-0.397	0.111	-1.685
	(0.031;0.094)	(-1.192;-0.219)	(-0.08;0.173)	(-2.57;1.776)	(-0.13;0.351)	(-5.417;2.047)
Norway	0.001	0.045	-0.049	3.33	-0.005	1.579
	(-0.009;0.01)	(-0.526;0.615)	(-0.093;-0.006)	(0.582;6.078)	(-0.042;0.032)	(-1.285;4.442)
New Zealand	-0.005	0.151	-0.123	4.147	-0.125	4.343
	(-0.028;0.017)	(-0.711;1.014)	(-0.19;-0.056)	(1.975;6.32)	(-0.299;0.049)	(-2.794;11.479)
Denmark	-0.001	-0.414	0.002	1.239	-0.001	0.397
	(-0.009;0.008)	(-1.073;0.245)	(-0.01;0.014)	(-0.223;2.701)	(-0.021;0.02)	(-0.755;1.548)

Note:The table reports parameter estimates of the traditional UIRP regression (Panel A) as well as parameter estimates in equation (3), where the measures of uncertainty is the overall exchange rate uncertainty index constructed based on random walk forecast errors (Panel B).

when uncertainty is high. Thus, the UIRP puzzle is alleviated in low uncertainty environments for several of the additional countries that the extension to random walk forecast errors allows us to consider (Australia, Sweden and Denmark). For some other countries, although low uncertainty typically moves the coefficient in the right direction, it does not fully resolve the puzzle (South Africa and New Zealand); however, the latter (and Norway, for which the puzzle is not resolved) are "commodity countries", for which commodity prices might play a role in determining exchange rate fluctuations, which we abstract from.

1.8 CONCLUSIONS

This paper has investigated whether uncertainty can explain the short-run deviations from UIRP that we empirically observe in the data. We have found that deviations from UIRP are stronger in periods of high uncertainty, while UIRP tends to hold in periods of low uncertainty. While it is well-known that deviations from UIRP are large and time-varying, this is the first paper that provides an economic rationale for both the UIRP puzzle and the presence of time variation in UIRP coefficient estimates by linking UIRP deviations to uncertainty. The result is robust to using various measures of economic uncertainty as well as uncertainty indices based on random walk forecasts. Our empirical results are consistent with the existence of time-varying risk premia potentially linked to rare disasters.

Additional analyses that could be carried out in the future include investigating whether similar results hold at long horizons; however, the UIRP puzzle is really a puzzle at short horizons, which is what we focused on in this paper.

2

RAINFALL, AGRICULTURAL OUTPUT, AND PERSISTENT DEMOCRATIZATION

Joint with Antonio Ciccone (Mannheim University)

2.1 INTRODUCTION

We examine the effect of rainfall on agricultural output and democratization in the world's most agricultural countries. We focus on these countries as the vast majority was ruled by nondemocratic regimes before 1950, but today nearly half of them are democratic. Moreover, the large economic weight of agriculture in these countries makes rainfall a source of exogenous, and potentially transitory, variation in agricultural output over time. Hence, the world's most agricultural countries since 1950 are a logical time and place to examine the effect of shocks to rainfall and agricultural output on democratic transitions.

The effect of economic shocks on democratization is discussed by Haggard and Kaufman (1995), Geddes (1999), Acemoglu and Robinson (2006), Burke and Leigh (2010), Brückner and Ciccone (2011), Caselli and Tesei (2016), and Dorsch and Maarek (2020). We contribute by examining whether economic shocks can result in *persistent* democratization even when shocks are transitory. Put differently, our analysis focuses on the question of whether democratic transitions outlast the economic shocks that triggered the democratization process. More broadly, we also contribute to the literature on the economic determinants of democratization (e.g. Przeworski and Limongi, 1997; Barro, 1999; Acemoglu et al., 2008; Aidt and Franck, 2015; Aidt and Leon, 2016) and on whether political institutions are shaped permanently by random events at critical junctures (e.g. Lipset, 1959; Mahoney, 2001; Capoccia and Kelemen, 2007; Acemoglu and Robinson, 2012; Benati and Guerriero, 2021).

The two main theories of democratization we draw on are Acemoglu and Robinson (2001, 2006) and Besley and Persson (2019). Both theories imply that transitory shocks can start a process leading to permanent democratization depending on certain predetermined factors. The constellations of predetermined factors where this is a possibility can be thought of as *democratic tipping points* and our analysis can be seen as checking on the existence of such tipping points.

We start by examining the effect of rainfall on agricultural output since 1961 (the start date of the agricultural output data set) in the world's most agricultural countries. This group is defined as countries with agricultural GDP shares in the top quintile of the distribution, or equivalently, as countries with agricultural GDP shares above the Sub-Saharan African median. We choose this cutoff as it is used in Brückner and Ciccone's (2011) analysis of the effect of rainfall on short-run democratization in Sub-Saharan Africa since 1980 (we also examine the persistence of democratization for this group of countries and time period). Like the agricultural economics literature, we find that the relationship between rainfall and agricultural output has an inverted U-shape as agricultural output is harmed by both droughts and very wet conditions (e.g., Schlenker and Lobell, 2010; Lobell et al., 2011). We also find the effect of rainfall on agricultural output to be transitory.

We go on to examine the relationship between rainfall and democratization in the world's most agricultural countries since 1945 (different democratization data sets have different start and end dates). We find the relationship to be U-shaped in the short run. This relationship persists in the long run. The U-shaped relationship between rainfall and democratization is consistent with rainfall affecting democratization through its inverted-U-shaped effect on agricultural output. The U-shaped relationship between rainfall and democratization holds for all three of the main dichotomous political regime classifications we use: the classification of Acemoglu et al. (2019); of Geddes et al. (2014); and of Przeworski et al. (2000) as updated by Cheibub et al. (2010) and Bjørnskov and Rode (2020). It also holds using the Polity Project polity score (Marshall et al., 2014); the Freedom House index of political rights (Freedom House, 2014); and the recent dichotomous political regime classification of Gründler and Krieger (2021).

Two theories of political transitions that fit our empirical examination are Acemoglu and Robinson (2001, 2006) and Besley and Persson (2019). The main conclusion of both theories is that transitory shocks can trigger persistent democratization. In Acemoglu and Robinson (2001, 2006), countries are initially ruled by nondemocratic regimes. The disenfranchised poor majority can contest the authoritarian rule. As the opportunity cost of doing so is lower following transitory negative economic shocks, such shocks may put the disenfranchised in a temporary position to demand democratization. As a result, transitory negative economic shocks can lead to democratization.[1] Democratization may be followed by nondemocratic reversal or may be permanent, depending on the constellation of several factors—income inequality and the cost of coups for example. We refer to the constellations of preconditions where a transitory economic shock would lead to persistent democratization as *democratic tipping points.* The persistence of democratization plays an important role in Acemoglu and Robinson's theory of political transitions. The disenfranchised poor could demand policy concessions rather than

[1] While this view fits with our empirical work, it is not the only possibility. For example, an alternative possibility Aidt and Leon (2016) point to is that agricultural-output shocks could trigger internal migration and the tension this causes in the receiving regions could spark riots and ultimately demands for democratization.

contest authoritarian rule. When they demand democratization, it is because democratization is more difficult to reverse. Put differently, the demand for democratization is based on the expectation that democratization will tend to persist beyond the transitory events that backed up democratization demands. In Besley and Persson (2019), there also is a conflict of interest over democratic institutions between a political elite and its opposition. But the political elite chooses whether to install a democracy or an autocracy in each time period. A key factor for this decision is the proportion of individuals with democratic values who may fight for democracy against autocracy. This proportion evolves endogenously. The model gives rise to a complementarity between the number of individuals holding democratic values and democracy that can create persistent democratization following transitory shocks.

Our empirical work contributes to the literature on the economic determinants of democratization, see Przeworski and Limongi (1997), Barro (1999), Przeworski et al. (2000), Acemoglu et al. (2008), Brückner and Ciccone (2011), Aidt and Franck (2015), Aidt and Leon (2016), Caselli and Tesei (2016), Benati et al. (2019), Dorsch and Maarek (2020), and Benati and Guerriero (2021). Our work is most closely related to Brückner and Ciccone (2011). They examine whether adverse rainfall shocks opened a window of opportunity for democratization in Sub-Saharan Africa over the 25-year period from 1980 to 2004. Their main finding is that adverse rainfall shocks lead to short-run democratic improvements in the group of 21 countries with agricultural GDP shares above the Sub-Saharan-African median but not in the group of 20 countries with agricultural GDP shares below the median. With Brückner and Ciccone, we have in common that we also examine the effect of rainfall shocks on democratization. There are four main differences. First, we examine whether democratization persists after the window of opportunity opened by adverse rainfall shocks has closed. Brückner and Ciccone solely examine the impact of rainfall shocks on short-run changes in democratic institutions.[2] Second, we build on the evidence in agricultural economics that the relationship between rainfall and agricultural output has an inverted U-shape, as agriculture is harmed by both droughts and very wet conditions. Brückner and Ciccone assume a monotonic effect of rainfall on output and on the probability of democratization in their empirical analysis. Specifically, they assume that output and the probability of democratization depend on the log-level of rainfall, which imposes monotonicity but allows for weaker marginal effects at higher levels of rainfall. In our Online Appendix[3], we show that results using this specification point in the same direction as the quadratic specification we focus on. Third, we look at the most agricultural countries in the world for the largest possible period since 1945, which results in a substantially larger and longer sample. Fourth, in addition to measuring democratization using the Polity project combined polity score as in Brückner and Ciccone, we incorporate the political regime classifications of Acemoglu et al. (2019); of Geddes et al. (2014); Bjørnskov and Rode's (2020) revision of the original Przeworski et al. (2000) regime classification; and Gründler and Krieger's (2021) political regime classification obtained with machine learning.

Our work is also related to Acemoglu et al. (2008) and Benati and Guerriero (2021). With Acemoglu *et al.*, we have in common that we examine the economic determinants of democratization over shorter and longer periods. The main difference is that we focus on democratization following transitory economic shocks, while Acemoglu *et al.* analyze the effect of more persistent changes in income.[4] With Benati and

[2] Brückner and Ciccone also estimate specifications where short-run democratic change is linked to rainfall shocks as well as lagged democracy indices. These democracy indices capture the persistence of all democratization events, including democratization events driven by persistent socio-economic shocks in the country or persistent shocks to the international political environment. Our interest is specifically in the persistence of democratization events that are triggered by transitory shocks.

[3] Available at https://doi.org/10.1111/ecca.12405

[4] Dell (2012) also examines longer-run effects of transitory rainfall shocks. She shows that local variation in drought severity just before the Mexican Revolution

Guerriero we have in common that we look at long-run institutional change following weather shocks. Their evidence is for Bronze Age Mesopotamia, while we focus on the world's most agricultural countries after 1945.

The paper is structured as follows. Section 2 presents the data and the empirical specifications. Section 3 discusses our empirical results and their robustness. Section 4 concludes.

2.2 DATA AND EMPIRICAL FRAMEWORK

2.2.1 *Data*

The agricultural output data we use is the real crops production index from the United Nation's Food and Agricultural Organization (FAOSTAT, 2016). We use this index to examine the effect of rainfall on agricultural output in countries grouped by their average share of agriculture in GDP. The data for agricultural GDP shares is from the World Development Indicators (2016) and is available since 1970. Table 2.1 contains the start and end dates of the real crops production index for countries with 1970–2013 agricultural GDP shares above the Sub-Saharan-African median or, what turns out to be equivalent, countries with agricultural GDP shares in the top quintile of the distribution. This sample is a logical starting point as the median agricultural GDP share in Sub-Saharan Africa is the cutoff used in Brückner and Ciccone (2011). The main difference between their and our analysis is that we include all countries in the world with agricultural GDP shares above this cutoff.

The rainfall data we use comes from the United States Government's National Oceanic and Atmospheric Administration and the temperature data from the United States Government's Center for Environmental Prediction. This data is available globally on a grid of approximately

affected long-run local development.

Table 2.1: Agricultural Output and Democratization Data Coverage for the World's Most Agricultural Countries

Country	FAO Real Agricultural Output		Acemoglu *et al.* (2019) Political Regime Data		Przeworski *et al.* (2000) Political Regime Data		Geddes *et al.* (2014) Political Regime Data		Polity IV Project Combined Polity Score		Freedom House Index of Political Rights	
	Start Year	End Year	Start Year	End Year	Start Year	End Year	Start Year	End Year	Start Year	End Year	Start Year	End Year
Afghanistan	1961	2013	1960	2010	1946	2010	1950	2010	1946	2000	1972	2013
Albania	1961	2013	1960	2010	1946	2010	1950	2010	1946	2013	1972	2013
Bhutan	1971	2013	1971	2010					1971	2013	1972	2013
Burkina Faso	1961	2013	1961	2010	1961	2010	1961	2010	1961	2013	1972	2013
Burundi	1963	2013	1961	2010	1963	2010	1963	2010	1963	2013	1972	2013
Cambodia	1961	2013	1961	2010	1954	2010	1954	2010	1954	2013	1972	2013
Central African Republic	1961	2013	1961	2010	1961	2010	1961	2010	1961	2013	1972	2012
Chad	1961	2013	1961	2010	1961	2010	1961	2010	1961	2013	1972	2013
Comoros	1976	2013	1961	2010					1976	2013	1976	2013
Equatorial Guinea	1969	2013	1961	2010					1969	2013	1972	2013
Ethiopia	1993	2013	1961	2010	1946	2010	1950	2010	1946	2013	1972	2013
Ghana	1961	2013	1961	2010	1958	2010	1958	2010	1960	2013	1972	2013
Guinea-Bissau	1974	2013	1961	2010	1975	2010	1975	2010	1974	2013	1974	2013
Laos	1961	2013	1961	2010	1954	2010	1954	2010	1954	2013	1973	2013
Liberia	1961	2013	1961	2010	1946	2010	1950	2010	1946	2013	1972	2013
Madagascar	1961	2013	1961	2010	1961	2010	1961	2010	1961	2013	1972	2013
Malawi	1965	2013	1961	2010	1965	2010	1965	2010	1965	2013	1972	2013
Mali	1961	2013	1961	2010	1961	2010	1961	2010	1961	2013	1972	2013
Mauritania	1961	2013	1961	2010	1961	2010	1961	2010	1961	2013	1972	2013
Mozambique	1976	2013	1961	2010	1976	2010	1976	2010	1976	2013	1976	2013
Myanmar (Burma)	1961	2013	1961	2010	1949	2010	1950	2010	1949	2013	1972	2013
Nepal	1961	2013	1961	2010	1946	2010	1950	2010	1946	2013	1972	2013
Niger	1961	2013	1961	2010	1961	2010	1961	2010	1961	2013	1972	2013
Papua New Guinea	1976	2013	1961	2010					1976	2013	1976	2013
Rwanda	1963	2013	1961	2010	1963	2010	1963	2010	1963	2013	1972	2013
Sierra Leone	1962	2013	1961	2010	1962	2010	1962	2010	1962	2013	1972	2013
Solomon Islands	1979	2013	1961	2010					1979	2013	1979	2013
Somalia	1961	2013	1961	2010	1961	1991	1961	1991	1961	2013	1972	2013
Sudan	1961	2010	1961	2010	1957	2010	1957	2010	1957	2010	1972	2010
Togo	1961	2013	1961	2010	1961	2010	1961	2010	1961	2013	1972	2013
Uganda	1963	2013	1961	2010	1963	2010	1963	2010	1963	2013	1972	2013
Vietnam	1977	2013	1961	2010					1977	2013		

Note: The table shows the data coverage for countries with agricultural GDP shares in the top quintile of the 1970–2013 distribution. Start year and end year indicate the first and the last year of observation respectively and omitted years indicate that the data was not available for that particular country. Acemoglu *et al.* (2019) refers to Acemoglu, Naidu, Restrepo, and Robinson (2019); Przeworski *et al.* (2000) to Przeworski, Alvarez, Cheibub, and Limongi (2000) as updated and extended by Cheibub, Gandhi, and Vreeland (2010) and Bjørnskov and Rode (2017); and Geddes *et al.* (2014) to Geddes, Wright, and Frantz (2014). Real agricultural output is measured by the real crops production index from the United Nation's Food and Agricultural Organization (FAO).

50×50 km at the equator since 1945. Country-year rainfall and temperature are measured as average annual rainfall and average temperature within a country's territory.

We use three main data sets that classify regimes into democracies or nondemocracies and two multivalued measures of democratic quality. The three main dichotomous regime classifications are Acemoglu et al. (2019); Geddes et al. (2014)[5]; and Przeworski et al. (2000) as updated by Cheibub et al. (2010) and Bjørnskov and Rode (2020). The Acemoglu *et al.* classification is available for the broadest sample of countries and combines information from several different sources.[6] The two

[5]Using their regime classification we code a country as democratic if it is a democracy or if it is ruled by a provisional government overseeing its transition to democracy. We drop years where according to Geddes, Wright, and Frantz the country is not independent, it is occupied by a foreign nation, or there is no government controlling most of the territory.

[6]Acemoglu et al. (2019) combine information from Freedom House and Polity IV, supplemented by dichotomous measures from Cheibub et al. (2010) and Boix et al.

multivalued indices measuring the quality of democratic institutions we use are the Polity Project combined polity score and the Freedom House index of political rights (Marshall et al., 2014; Freedom House, 2014). We drop so-called interregnum years when according to the Polity Project there is no government controlling most of the territory. Former colonies are only included since independence and we require countries to have been independent for at least 25 years (about half our sample period). Start and end dates of the different democratization measures for countries with agricultural GDP shares in the top quintile of the distribution are in Table 2.1. Our sixth measure of democratization is based on the dichotomous political regime classification that Gründler and Krieger (2021) derive using machine learning.

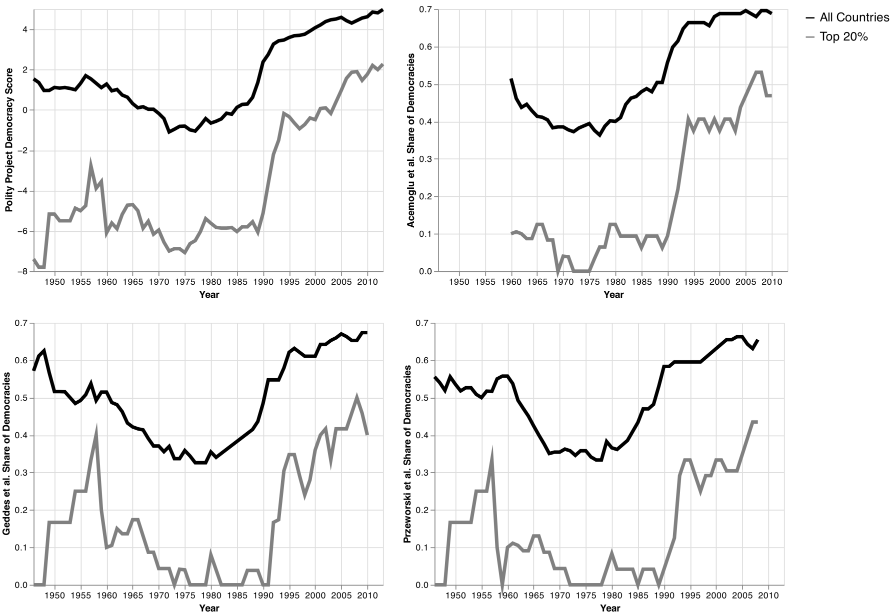
The measures of democratization we use differ in definitions, as explained in detail in the papers cited in the previous paragraph. For example, Geddes et al. (2014) code a competitive election for the executive as democratization only if a person other than the previous authoritarian incumbent or someone allied with the incumbent wins the election. They also use a different timing rule when coding the start date of democratic government (more about this further below). As a result, the different measures of democratization we use indicate a somewhat different timing for democratic change. Figure 2.1 illustrates these trends for the world's most agricultural countries and, for comparison, all countries. Trends are similar, but there are more ups and downs for the world's most agricultural countries.

2.2.2 *Empirical Framework*

The estimating equation for the effect of rainfall on real agricultural output follows the agricultural economics literature, see Schlenker and Lobell (2010), Lobell, Schlenker, and Costa-Roberts (2011), and Maertens (2021) for example. The literature finds that the within-country

(2013).

Figure 2.1: Democratization Trends in the World's Most Agricultural Countries



Note: Democratization trends illustrated clockwise starting with the upper-left panel: the Polity Project combined polity score; the share of democracies according to Acemoglu *et al.* (2019); the share of democracies according to Przeworski *et al.* (2000) as updated by Cheibub *et al.* (2010) and Bjørnskov and Rode (2020); and the share of democracies according to Geddes *et al.* (2014). The black line is for all countries and the grey line for countries with average 1970–2013 agricultural GDP shares in the top quintile of the distribution.

relationship between rainfall and agricultural output is quadratic and has an inverted U-shape. The effect of rainfall on agricultural output in country c and year t is

$$
\begin{aligned}
&RealAgriculturalOutputIndex_{c,t} = Controls_{c,t} \\
&+ \left(a_0 R_{c,t} + b_0 R^2_{c,t}\right) + \left(a_1 R_{c,t-1} + b_1 R^2_{c,t-1}\right) + \left(a_2 R_{c,t-2} + b_2 R^2_{c,t-2}\right) + \varepsilon_t
\end{aligned} \tag{2.1}
$$

where the three terms $aR + bR^2$ capture the (quadratic) within-country effect of rainfall at different lags and $Controls_{c,t}$ always include (i) country fixed effects; (ii) year fixed effects; (iii) country-specific linear time trends; and (iv) linear-quadratic terms for temperature that match the lag structure of the rainfall variable. The quadratic specification allows the relationship between rainfall and agricultural output to have an inverted U-shape. In this case, additional rainfall would increase agricultural output for rainfall levels to the left of the peak of the inverted U, but additional rainfall would decrease agricultural output for rainfall levels to the right of the peak of the inverted U. That is, there could be too little or too much rain as far as agricultural productivity is concerned. The method of estimation is least squares with HAC standard errors that are robust to both arbitrary heteroskedasticity and serial correlation.[7]

The estimating equation for the effect of rainfall on democratization outcomes in country c between years $t-1$ and T mirrors the equation for agricultural output

$$
\begin{aligned}
&Democratization^T_{c,t-1} = Controls_{c,t} \\
&+ \left(a_0 R_{c,t} + b_0 R^2_{c,t}\right) + \left(a_1 R_{c,t-1} + b_1 R^2_{c,t-1}\right) + \left(a_2 R_{c,t-2} + b_2 R^2_{c,t-2}\right) + \varepsilon_t
\end{aligned} \tag{2.2}
$$

where the three terms $aR + bR^2$ capture the (quadratic) within-country effect of rainfall at different lags and $Controls_{c,t}$ always include (i) country fixed effects; (ii) year fixed effects; (iii) country-specific linear

[7] We also estimate the equation using (log-)GDP per capita from the Penn World Tables on the left-hand side but never find any significant effects, probably because of the quite extreme noise in PWT GDP for low-income countries, see Johnson et al. (2013).

time trends; and (iv) linear-quadratic terms for temperature that match the lag structure of the rainfall variable. Year fixed effects play an important role as they capture global factors driving the probability of democratization; for example, the dissolution of the Soviet Union in 1991. The estimation method is the same as for equation (1).

For $T = 1$, the specification in (2) allows us to examine the short-run relationship between rainfall and democratization, as in Brückner and Ciccone (2011). By varying T, we can examine the effect of rainfall on short-run and longer-run democratization, see Acemoglu et al. (2008) for a similar approach.

Democratization between years $t-1$ and T in (2) is measured in two main ways. The first measure is a democratization indicator based on dichotomous political regime classifications. The democratization indicator takes the value of 1 if the country is classified as a nondemocracy in year $t-1$ but a democracy in year T. If the country is a nondemocracy in year $t-1$ and a nondemocracy in year T, the democratization indicator takes the value of 0. The democratization indicator between years $t-1$ and T is not defined if the country is a democracy in year $t-1$. The second measure of democratization is based on the change in multivalued indices measuring the quality of democratic institutions.

The model in (1) and (2) has two interesting implications. First, if the relationship between rainfall and agricultural output in (1) is inverted U-shaped and if the effect of rainfall on democratization is through agricultural output, the relationship between rainfall and democratization in (2) should be U-shaped. Second, the maximum of the inverted-U-shaped relationship between rainfall and agricultural output should be at the same level of rainfall as the minimum of the U-shaped relationship between rainfall and democratization.

2.3 EMPIRICAL RESULTS

We first examine the effect of rainfall on agricultural output in countries grouped by agricultural GDP shares. Then we examine the effect of rainfall on different measures of democratic change in the world's most agricultural countries.

2.3.1 *Rainfall and Agricultural Output*

Table 2.2 summarizes our results on the effect of rainfall on agricultural output using equation (1). Columns (1)-(4) contain results for different subgroups of countries. These subgroups are based on average agricultural GDP shares over the 1970–2013 period (agricultural GDP shares are only available since 1970). Column (5) contains the results for all countries with agricultural output data.

Table 2.2: Rainfall and Agricultural Output since 1960: Effect by Share of Agriculture in Gross Domestic Product

	Top Quintile Agricultural Countries	All Countries Except Top Quintile Agricultural Countries	Top Quarter Agricultural Countries	Top Tercile Agricultural Countries	All Countries
	(1)	(2)	(3)	(4)	(5)
Rainfall t	2.221***	0.021	1.033*	-0.123	0.302
	(0.636)	(0.392)	(0.534)	(0.429)	(0.367)
Quadratic Rainfall t	-0.059***	-0.004	-0.031***	-0.001	-0.010
	(0.014)	(0.009)	(0.012)	(0.010)	(0.009)
Rainfall $t-1$	0.134	-0.045	-0.389	-0.577	0.026
	(0.638)	(0.397)	(0.516)	(0.362)	(0.367)
Quadratic Rainfall $t-1$	-0.010	-0.004	0.003	0.009	-0.007
	(0.015)	(0.010)	(0.012)	(0.008)	(0.009)
Rainfall $t-2$	0.264	-0.426	-0.294	-0.363	-0.208
	(0.626)	(0.404)	(0.496)	(0.365)	(0.374)
Quadratic Rainfall $t-2$	-0.002	0.006	0.008	0.008	0.001
	(0.014)	(0.010)	(0.012)	(0.008)	(0.009)
Countries	32	129	41	53	161
Observations	1,515	5,936	1,934	2,444	7,451
R Squared	0.065	0.009	0.041	0.013	0.013

Note: The left-hand side variable is an index of real agricultural output. Countries are assigned to subsamples by the average share of agriculture in GDP over the 1970–2013 period. The specification includes country fixed effects, year fixed effects, and linear quadratic contemporaneous and lagged temperature effects. The numbers in parentheses are heteroskedastic and autocorrelation-consistent (HAC) standard errors that are robust to both arbitrary heteroskedasticity and serial correlation. * denotes significance at the 10% level; ** significance at the 5% level; and *** significance at the 1% level.

Column (1) shows the results for the 32 countries with an average GDP share of agriculture in the top quintile of the distribution, or equivalently, all countries with agricultural GDP shares above the Sub-Saharan-African median. This is the sample of the world's most agricultural countries we will focus on. As already mentioned, this sample is a logical starting point as it uses the same cutoff for the agricultural GDP share as Brückner and Ciccone's (2011) analysis for Sub-Saharan Africa. But while Brückner and Ciccone only include Sub-Saharan African countries in their analysis, we include all countries in the world with agricultural GDP shares above this cutoff. The relationship between rainfall in year t and agricultural output in year t in column (1) is statistically significant and inverted U-shaped. The effect of lagged rainfall is statistically insignificant.[8] Approximately 15% of the rainfall observations are to the right of the peak in agricultural output. To get a sense for the strength of the contemporaneous effect, we calculate the percentage decrease in agricultural output caused by the median year-on-year drop in rainfall between year $t-1$ and t starting at the median level of rainfall in year $t-1$. We refer to this shock as the median year-on-year negative rainfall shock. The implied decrease in agricultural output is around one percentage point. As the average share of agriculture in GDP in countries in the top quintile of the distribution is 40%, the implied effect on GDP of the median year-on-year negative rainfall shock is around -0.4%.

Column (2) considers countries whose average share of agriculture in GDP is outside of the top quintile of the distribution (the complement of countries in column (1)). Now the contemporaneous effect of rainfall on agricultural output is also statistically insignificant.

Columns (3) and (4) consider countries with shares of agriculture in GDP in the top quarter and the top tercile of the distribution respect-

[8] Maertens (2021) also finds the effect of lagged rain on agricultural output to be statistically insignificant in a very similar empirical specification estimated for Sub-Saharan African countries only. This remains true when he controls for rainfall over agricultural land during the growing season. Schlenker and Lobell (2010) and Lobell, Schlenker, and Costa-Roberts (2011) assume a contemporaneous effect only in their empirical specifications.

ively. For countries in the top quarter of the distribution in column (3), the relationship between rainfall in year t and agricultural output in year t is statistically significant and inverted U-shaped. The implied contemporaneous effect of the median year-on-year negative rainfall shock on agricultural output is around -0.3%, less than one-third of the effect that we estimated in countries with agricultural GDP shares in the top quintile of the distribution. When combined with the average GDP share of agriculture in the top quarter of the distribution, this yields an effect of the median year-on-year negative rainfall shock on GDP of -0.1%. This effect is substantially weaker than the -0.4% GDP effect we estimate in countries with agricultural GDP shares in the top quintile of the distribution. For countries in the top tercile of the distribution in column (4), the contemporaneous effect of rainfall on agricultural output becomes statistically insignificant and the implied effect of a median negative rainfall shock on agricultural output is close to zero.

There are two explanations for the drop off in the effect of rainfall on agricultural output as one moves outside the group of countries with agricultural GDP shares in the top quintile. First, a greater use of irrigation systems. There is very little irrigation in countries in the top quintile of the distribution of agricultural GDP shares. According to the World Development Indicators (2016), the median share of irrigated agricultural land in these countries over the 2001–2010 period was around 0.7% (very little data is available for earlier years). Outside of the group of countries in the top quintile of the distribution of agricultural GDP shares, the share of irrigated agricultural land is much higher. For example, the median share of irrigated agricultural land in countries with agricultural GDP shares in the top tercile but not the top quintile of the distribution was around 9%. A second factor likely to play a role is that rainfall is measured over a country's entire territory. In less agricultural countries, more of the measured rainfall is not over agricultural land and hence will not have an effect on agricultural output.

Finally, column (5) shows that the effect of rainfall on agricultural output is statistically insignificant when all countries are included in the empirical analysis.

2.3.2 *Rainfall and Persistent Democratization in the World's Most Agricultural Countries*

We start with our results using measures of democratization based on dichotomous political regime classifications and then turn to the results using multivalued indices of democratic quality.

2.3.2.1 Democratization Based on Dichotomous Political Regime Classifications

Table 2.3 summarizes the short-run and longer-run effects of rainfall on democratization in countries with agricultural GDP shares in the top quintile of the distribution. We use three main indicators of democratization based on dichotomous political regime classifications: (i) Acemoglu et al. (2019); (ii) Przeworski et al. (2000) as updated by Cheibub et al. (2010) and Bjørnskov and Rode (2020); and (iii) Geddes et al. (2014). The number of countries and observations per country depends on the measure of democratization as data sets differ in terms of countries and time periods covered, see Table 2.1.

Table 2.3: Rainfall and Democratization since 1960: From Short to Long Term

	Panel A: Acemoglu *et al.* (2019) Data				Panel B: Przeworski *et al.* (2000) Data				Panel C: Geddes *et al.* (2014) Data			
	Acemoglu *et al.* Democratization between $t-1$ and				Przeworski *et al.* Democratization between $t-1$ and				Geddes *et al.* Democratization between $t-1$ and			
	t (1-Year)	$t+2$ (3-Year)	$t+4$ (5-Year)	$t+9$ (10-Year)	t (1-Year)	$t+2$ (3-Year)	$t+4$ (5-Year)	$t+9$ (10-Year)	t (1-Year)	$t+2$ (3-Year)	$t+4$ (5-Year)	$t+9$ (10-Year)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Rainfall t	-0.031**	-0.044**	-0.065***	-0.056**	-0.031**	-0.043**	-0.057***	-0.042**	-0.012	-0.011	-0.035*	-0.041*
	(0.016)	(0.020)	(0.022)	(0.022)	(0.015)	(0.020)	(0.021)	(0.021)	(0.011)	(0.017)	(0.019)	(0.021)
Quadratic Rainfall t	0.001*	0.001*	0.001**	0.001**	0.001*	0.001*	0.001**	0.001	0.000	0.000	0.000	0.001
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)
Rainfall $t-1$	0.015	-0.008	-0.035*	-0.051**	0.011	-0.020	-0.025	-0.044**	-0.033**	-0.032*	-0.045**	-0.037*
	(0.011)	(0.021)	(0.019)	(0.024)	(0.012)	(0.018)	(0.018)	(0.020)	(0.015)	(0.019)	(0.019)	(0.019)
Quadratic Rainfall $t-1$	-0.000	-0.000	0.001	0.001**	-0.000	0.000	0.000	0.001*	0.001*	0.001*	0.001**	0.001*
	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)
Rainfall $t-2$	-0.016	-0.060***	-0.044**	-0.051**	-0.021	-0.034*	-0.019	-0.029	0.014	-0.018	-0.028*	-0.033*
	(0.015)	(0.019)	(0.018)	(0.024)	(0.013)	(0.017)	(0.016)	(0.021)	(0.012)	(0.017)	(0.016)	(0.020)
Quadratic Rainfall $t-2$	0.000	0.001**	0.001*	0.001**	0.000	0.001	0.000	0.000	-0.000	0.000	0.000	0.001
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Countries	31	31	31	30	26	26	26	26	26	26	26	26
Observations	1,132	1,100	1,069	975	1,054	1,016	981	895	1,049	1,012	978	899
R Squared	0.016	0.043	0.051	0.029	0.029	0.031	0.032	0.020	0.017	0.024	0.051	0.037

Note: The left-hand-side variable in columns (1), (5), and (9) is a democratization indicator that takes the value of 1 if a country that is a nondemocracy at $t-1$ is a democracy at t (one year later) and the value of 0 otherwise. The left-hand-side variable in columns (2), (6), and (10) is an indicator variable that takes the value of 1 if a country that is a nondemocracy at $t-1$ is a democracy at $t+2$ (three years later) and the value of 0 otherwise. The left-hand-side variable in columns (3), (7), and (11) is an indicator variable that takes the value of 1 if a country that is a nondemocracy at $t-1$ is a democracy at $t+4$ (five years later) and the value of 0 otherwise. The left-hand-side variable in columns (4), (8), and (12) is an indicator variable that takes the value of 1 if a country that is a nondemocracy at $t-1$ is a democracy at $t+9$ (ten years later) and the value of 0 otherwise. The classification of democratic and nondemocratic regimes in columns (1)-(4) is based on Acemoglu, Naidu, Restrepo, and Robinson (2019). The classification of democratic and nondemocratic regimes in columns (5)-(8) is based on Bjørnskov and Rode (2017) who extend the dataset of Cheibub, Gandhi, and Vreeland (2010) and Przeworski, Alvarez, Cheibub, and Limongi (2000). The classification of democratic and nondemocratic regimes in columns (9)-(12) is based on Geddes, Wright, and Frantz (2014). The included countries are with an average share of agriculture in GDP over the 1970–2013 period in the top quintile of the distribution. The specification includes country fixed effects, year fixed effects, and linear and quadratic contemporaneous and lagged temperature effects. The specification also includes a linear and quadratic term for rainfall lagged by three years but these terms are generally statistically insignificant and not reported for brevity. The numbers in parentheses are heteroskedastic and autocorrelation-consistent (HAC) standard errors that are robust to both arbitrary heteroskedasticity and serial correlation. * denotes significance at the 10% level; ** significance at the 5% level; and *** significance at the 1% level.

Acemoglu *et al.* Democratization Table 2.3, Panel A summarizes the short-run and longer-run effects of rainfall on democratization when the measure of democratization in estimating equation (2) is based on the political regime classification of Acemoglu et al. (2019), which we refer to as Acemoglu *et al.* in short. The democratization indicator between years $t-1$ and T is only defined if the country is classified as a nondemocracy in year $t-1$. The indicator takes the value 1 if the country is a democracy in year T and the value 0 if the country is a nondemocracy in T. The panel contains results for the effect of rainfall on the probability that a nondemocracy in year $t-1$ is a democracy in year t (one year later); in year $t+2$ (three years later); in year $t+4$ (five years later); and in year $t+9$ (ten years later).

The main finding is that the relationship between rainfall in year t and the probability of democratization is U-shaped and statistically significant for democratization one, three, five, and ten years later. Hence, the effect of within-country rainfall variation on democratization is persistent.[9] To get a sense for the magnitude of the effect of rainfall on democratization, consider a negative rainfall shock in year t equal to the median year-on-year drop in rainfall in the world's most agricultural countries. Suppose this shock affects a country following a year where the rainfall level was equal to the median. Our estimates in Table 2.3, Panel A imply that this negative shock increases the probability that a nondemocracy at $t-1$ is a democracy one year later by around 1.5 percentage points. The probability that a nondemocracy at $t-1$ is a democracy three, five, and ten years later increases by between two and three percentage points.

Online Appendix Tables 1 and 2 contain a robustness analysis of the results using the Acemoglu *et al.* democratization indicator. Online

[9] We also find a statistically significant and U-shaped effect of rainfall on democratization between year $t-1$ and year $t+14$ (15 years later). We focus on democratization periods of up to 10 years because the number of observations decreases with the length of the democratization period.

Appendix Table 1 shows that results are robust when we exclude years where according to Geddes *et al.* (2014) the country is controlled by foreign nations or there is no government controlling most of the country's territory. Online Appendix Table 2, Panels A-C show results when we drop or add countries one by one depending on their agricultural GDP share. Results are robust, especially for longer-term democratization.

Przeworski *et al.* Democratization Table 2.3, Panel B summarizes the short-run and longer-run effects of rainfall on democratization when the measure of democratization in equation (2) is based on the political regime classification of Przeworski et al. (2000) as updated by Cheibub et al. (2010) and Bjørnskov and Rode (2020), which we refer to as Przeworski *et al.* in short. The main finding is that the relationship between rainfall in year t and the probability of democratization is U-shaped and statistically significant for democratization between year $t{-}1$ and year t (one year later); year $t{+}2$ (three years later); year $t{+}4$ (five years later); and year $t{+}9$ (ten years later).[10] Hence, the Przeworski *et al.* democratization indicator also points to a persistent effect of within-country rainfall variation on democratization. The estimates in Table 2.3, Panel B imply that the median year-t negative rainfall shock increases the probability that a nondemocracy at $t{-}1$ is a democracy one year later by around 1.5 percentage points. The median negative rainfall shock continues to be defined as the median year-on-year drop in rainfall starting at the median level of rainfall. The median year-t negative rainfall shock increases the probability that a nondemocracy at $t{-}1$ is a democracy three, five, and ten years later by between two and three percentage points.

Online Appendix Table 3, Panels A-C show that results are robust when we drop or add countries one by one depending on their agricultural GDP share.

[10] We also find a statistically significant and U-shaped effect of rainfall on democratization between year $t{-}1$ and year $t + 14$ (15 years later).

Geddes *et al.* Democratization Table 2.3, Panel C summarizes the short-run and longer-run effects of rainfall on democratization when the democratization indicator in equation (2) is based on the political regime classifications of Geddes et al. (2014). The results again indicate a statistically significant, U-shaped relationship between rainfall and the probability of democratization over different time periods. The timing of the rainfall effect is somewhat different than for Przeworski *et al.* democratizations. In particular, it is rainfall in year $t-1$ that is statistically significant over all time periods. Differences in timing are not particularly surprising as different political regime classifications use different definitions and measurement criteria. The difference between the Geddes *et al.* and the Przeworski *et al.* regime classifications that matters most for the difference in the timing of the rainfall effect in Table 2.3, Panel C is that Geddes *et al.* do not follow "the convention" (their words) in coding the start date of democratic regimes. If a democratic regime becomes established in year t, the convention is to code December 31 as the start date. This is the rule used by Przeworski *et al.* for example. Geddes *et al.* use January 1 of the subsequent year instead. To see how these rules can affect the results imagine that a negative year-t rainfall shock causes democratization in year t. With the December 31 rule for regime start dates, this democratization event is recorded in year t and researchers would observe that negative year-t rainfall shocks lead to democratization in year t. With the January 1 rule for start dates, the democratization event is recorded in year $t+1$ and researchers would observe that negative year-t rainfall shocks lead to democratization in year $t+1$ (or put differently, that year-t democratizations are related to negative rainfall shocks in year $t-1$).

Because of the unconventional rule for the start dates of democratic regimes used by Geddes *et al.*, we illustrate the strength of the effect of the median year-on-year negative rainfall shock on the probability of Geddes at al. democratizations in two different ways. Our first approach is based on the estimates of the rainfall effect in $t-1$ in Table 2.3, Panel C. They yield that the median negative rainfall shock in year $t-1$ increases the probability that a nondemocracy at $t-1$ is a democracy one year later by around 0.8 percentage points. The increase in the probability that a nondemocracy at $t-1$ is a democracy three years later is around one percentage point, and the increase in the probability that a nondemocracy at $t-1$ is a democracy five and ten years later is around two percentage points. Our second approach recodes the start dates of democratic regimes in the Geddes *et al.* data set according to the convention, reestimates the specification in Table 2.3, Panel C using this recoded data, and then uses these estimates in our calculations. This yields that the median negative rainfall shock in year t increases the probability that a nondemocracy at $t-1$ is a democracy one and three years later by around 1.5 percentage points. The increase in the probability that a nondemocracy at $t-1$ is a democracy five and ten years later is around two percentage points.

Online Appendix Table 4, Panels A-C show results when we drop or add countries depending on their agricultural GDP share. Results are robust, especially for longer-term democratization.

Gründler and Krieger Democratization Table 2.4 summarizes the relationship between rainfall and short-run and longer-run democratization when the democratization indicator in equation (2) is based on the dichotomous political regime classification that Gründler and Krieger (2021) derive using machine learning. In addition to the effects on democratization in years t, $t+2$, $t+4$, and $t+9$ in Table 2.3, we also show the effect in $t+1$. It can be seen that the Gründler and Krieger democratization indicator, like the democratization indicators in Table 2.3, also yields a relationship between rainfall and the probability of democratization in year t that is U-shaped and statistically significant. However, the timing differs compared to Table 2.3 as it is rainfall in year $t-2$ that is statistically significant. This discrepancy disappears for democratization in year $t+1$ and thereafter.

Table 2.4: Rainfall and Democratization since 1960: From Short to Longer Term, Gründler and Krieger (2021) Data

	Gründler and Krieger (2021) Data				
	Gründler and Krieger Democratization between $t-1$ and				
	t (1-Year)	$t+1$ (2-Year)	$t+2$ (3-Year)	$t+4$ (5-Year)	$t+9$ (10-Year)
	(1)	(2)	(3)	(4)	(5)
Rainfall t	-0.019	-0.029**	-0.053***	-0.034*	-0.040*
	(0.012)	(0.014)	(0.017)	(0.020)	(0.021)
Quadratic Rainfall t	0.000	0.001*	0.001**	0.001	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Rainfall $t-1$	-0.002	-0.033**	-0.022	-0.043**	-0.027
	(0.010)	(0.015)	(0.016)	(0.018)	(0.021)
Quadratic Rainfall $t-1$	0.000	0.001**	0.000	0.001**	0.001
	(0.000)	(0.003)	(0.000)	(0.000)	(0.001)
Rainfall $t-2$	-0.030**	-0.019	-0.024	-0.032*	-0.011
	(0.013)	(0.015)	(0.018)	(0.018)	(0.020)
Quadratic Rainfall $t-2$	0.001**	0.000	0.000	0.001	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Countries	30	30	30	30	30
Observations	1,265	1,264	1,246	1,213	1,139
R Squared	0.021	0.025	0.033	0.029	0.009

Note: The left-hand-side variables in all columns are democratization indicators based on the classification of democratic and nondemocratic regimes of Gründler and Krieger (2021). The left-hand-side democratization indicator in column (1) takes the value of 1 if a country that is an autocracy at $t-1$ is a democracy at t (one year later) and the value of 0 otherwise. The left-hand-side democratization indicator in column (2) takes the value of 1 if a country that is an autocracy at $t-1$ is a democracy at $t+1$ (three years later) and the value of 0 otherwise. The left-hand-side democratization indicator in column (3) takes the value of 1 if a country that is an autocracy at $t-1$ is a democracy at $t+2$ (three years later) and the value of 0 otherwise. The left-hand-side democratization indicator in column (4) is an indicator variable that takes the value of 1 if a country that is an autocracy at $t-1$ is a democracy at $t+4$ (five years later) and the value of 0 otherwise. The left-hand-side democratization indicator in column (5) takes the value of 1 if a country that is an autocracy at $t-1$ is a democracy at $t+9$ (10 years later) and the value of 0 otherwise. The included countries are all countries with an average share of agriculture in GDP over the 1970–2013 period in the top quintile of the distribution. The specification includes country fixed effects, year fixed effects, and linear quadratic contemporaneous and lagged temperature effects. The specification also includes a linear and quadratic term for rainfall lagged by three years but these terms are generally statistically insignificant and not reported for brevity. The numbers in parentheses are heteroskedastic and autocorrelation-consistent (HAC) standard errors that are robust to both arbitrary heteroskedasticity and serial correlation. * denotes significance at the 10% level; ** significance at the 5% level; and *** significance at the 1% level.

Table 2.5: Rainfall and Democratization in Sub-Saharan Africa since 1980: From Short to Long Term

	Panel A: Acemoglu *et al.* (2019) Data					Panel B: Przeworski *et al.* (2000) Data				
	Acemoglu *et al.* Democratization between $t-1$ and					Przeworski *et al.* Democratization between $t-1$ and				
	t (1-Year)	$t+1$ (2-Year)	$t+2$ (3-Year)	$t+4$ (5-Year)	$t+9$ (10-Year)	t (1-Year)	$t+1$ (2-Year)	$t+2$ (3-Year)	$t+4$ (5-Year)	$t+9$ (10-Year)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Rainfall t	-0.016	0.003	-0.038	-0.104***	-0.111***	-0.094***	-0.071**	-0.053	-0.098***	-0.065*
	(0.024)	(0.026)	(0.030)	(0.036)	(0.031)	(0.031)	(0.032)	(0.036)	(0.035)	(0.039)
Quadratic Rainfall t	0.000	0.000	0.001	0.002***	0.003***	0.003***	0.002**	0.002	0.003***	0.002*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Rainfall $t-1$	0.024	-0.010	-0.048	-0.069*	-0.126***	0.028	0.032	-0.010	-0.058	-0.102**
	(0.022)	(0.030)	(0.034)	(0.037)	(0.032)	(0.020)	(0.033)	(0.035)	(0.042)	(0.042)
Quadratic Rainfall $t-1$	-0.001	0.000	0.001	0.002*	0.003***	-0.001	-0.001	0.000	0.001	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Rainfall $t-2$	-0.024	-0.063*	-0.113***	-0.089**	-0.100***	-0.008	-0.052	-0.068	-0.091**	-0.036
	(0.025)	(0.034)	(0.035)	(0.035)	(0.038)	(0.025)	(0.036)	(0.044)	(0.039)	(0.046)
Quadratic Rainfall $t-2$	0.001	0.001	0.003***	0.002**	0.003***	-0.000	0.002	0.002	0.003**	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Countries	22	22	22	22	22	20	20	20	20	20
Observations	478	466	456	437	381	435	422	411	388	323
R Squared	0.022	0.024	0.047	0.048	0.098	0.083	0.044	0.043	0.069	0.072

Note: The left-hand-side variable in columns (1) and (6) is a democratization indicator that takes the value of 1 if a country that is an nondemocracy at $t-1$ is a democracy at t (one year later) and the value of 0 otherwise. The left-hand-side variable in columns (2) and (7) is an indicator variable that takes the value of 1 if a country that is an nondemocracy at $t-1$ is a democracy at $t+1$ (two years later) and the value of 0 otherwise. The left-hand-side variable in columns (3) and (8) is an indicator variable that takes the value of 1 if a country that is an nondemocracy at $t-1$ is a democracy at $t+2$ (three years later) and the value of 0 otherwise. The left-hand-side variable in columns (4) and (9) is an indicator variable that takes the value of 1 if a country that is an nondemocracy at $t-1$ is a democracy at $t+4$ (five years later) and the value of 0 otherwise. The left-hand-side variable in columns (5) and (10) is an indicator variable that takes the value of 1 if a country that is an nondemocracy at $t-1$ is a democracy at $t+9$ (ten years later) and the value of 0 otherwise. The classification of democratic and nondemocratic regimes in columns (1)-(5) is based on Acemoglu, Naidu, Restrepo, and Robinson (2019). The classification of democratic and nondemocratic regimes in columns (5)-(10) is based on Bjørnskov and Rode (2017) who extend the dataset of Cheibub, Gandhi, and Vreeland (2010) and Przeworski, Alvarez, Cheibub, and Limongi (2000). The countries included in the analysis are all Sub-Saharan African countries with an average share of agriculture in GDP over the 1970–2013 period in the top quintile of the distribution or, equivalenty, with an average share of agriculture in GDP above the Sub-Saharn African median. The specification includes country fixed effects, year fixed effects, and linear quadratic contemporaneous and lagged temperature effects. The specification also includes a linear and quadratic term for rainfall lagged by three years but these terms are generally statistically insignificant and not reported for brevity. The numbers in parentheses are heteroskedastic and autocorrelation-consistent (HAC) standard errors that are robust to both arbitrary heteroskedasticity and serial correlation. * denotes significance at the 10% level; ** significance at the 5% level; and *** significance at the 1% level.

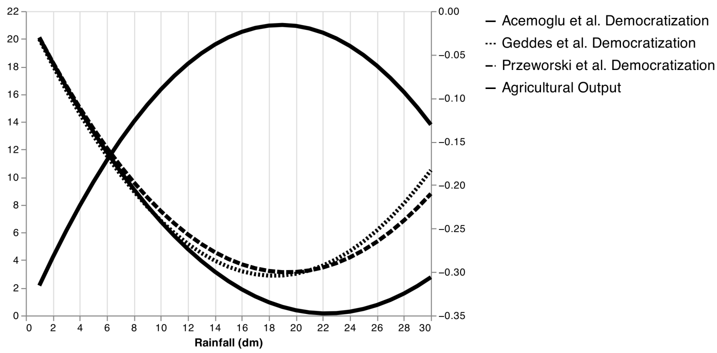
Agricultural Output and Democratization If the effect of rainfall on democratization is through agricultural output, the inverted-U-shaped relationship between rainfall and agricultural output should translate into a U-shaped relationship between rainfall and democratization. Put differently, the relationship between rainfall and the probability of democratization should be the flipped image of the relationship between rainfall and agricultural output.[11] Moreover, the minimum of the U-shaped relationship between rainfall and the probability of democratization should be at a similar rainfall level as the maximum of the inverted-U-shaped relationship between rainfall and agricultural output. That is, the rainfall level that maximizes agricultural output should be similar to the rainfall level that minimizes the probability of democratization.

Figure 2.2 examines whether this is the case. The inverted-U-shaped solid black curve shows the relationship between rainfall in year t and agricultural output in year t (measured on the left scale). This effect is calculated using the estimates in column (1) of Table 2.2. The peak of the inverted U is at a level of rainfall equal to the 85th percentile of the rainfall distribution. The maximum variation in agricultural output associated with rainfall variation is around 20 percentage points.

The U-shaped curves show the relationship between rainfall and the probability of democratization between years $t-1$ and t (measured on the right scale). The solid black curve is based on the Acemoglu *et al.* democratization indicator. The estimates used to obtain the effect of rainfall in year t on democratization in year t are those in column (1) of Table 2.3. The U-shaped relationship between rainfall and democratization is consistent with the effect of rainfall working through its inverted-U-shaped effect on agricultural output. Moreover, the rainfall level where the inverted-U-shaped relationship between rainfall and agri-

[11] Online Appendix Table 5 shows results when, following Brückner and Ciccone (2011), we assume that the probability of democratization depends on the log-level of rainfall. Results point in the same direction but indicate a somewhat different timing than when using the quadratic specification we focus on.

Figure 2.2: Effect of Rainfall on Real Agricultural Output and on the Probability of Democratization



Note: The inverted-U-shaped line is the effect of rainfall in year t on agricultural output in year t and is measured on the left axis. The three U-shaped lines are the effect of rainfall on the probability of democratization between years $t-1$ and t (one year later) for the three main dichotomous classifications of democratic and nondemocratic regimes used: Acemoglu *et al.* (2019); Przeworski *et al.* (2000) as updated by Cheibub *et al.* (2010) and Bjørnskov and Rode (2020); and Geddes *et al.* (2014).

cultural output reaches its maximum is similar to the rainfall level where the U-shaped relationship between rainfall and the probability of democratization reaches its minimum. A formal hypothesis test cannot reject that the two levels of rainfall are the same at any standard confidence level. The maximum variation in the probability of democratization associated with rainfall variation is around 35 percentage points.

The U-shaped dashed curve in Figure 2.2 illustrates the relationship between rainfall in year t and the probability of a Przeworski *et al.* democratization between years $t-1$ and t. This effect is calculated using the estimates in columns (5) of Table 2.3. It can be seen that the effect of rainfall on Przeworski *et al.* democratizations is also consistent with rainfall affecting democratization through its inverted-U-shaped effect on agricultural output. The rainfall level where the inverted-U-shaped relationship between rainfall and agricultural output reaches its maximum continues to be similar to the rainfall level where the U-shaped relationship between rainfall and the probability of democratization reaches its minimum. A formal hypothesis test cannot reject that these rainfall levels are the same at any standard confidence level. The maximum variation in the probability of democratization associated with rainfall variation is around 30 percentage points.

The U-shaped dotted curve in Figure 2.2 shows the relationship between rainfall and the probability of a Geddes *et al.* democratization between years $t-1$ and t. Because of the unconventional rule for start dates of different regimes used by Geddes *et al.*, the figure shows the probability of a Geddes *et al.* democratization as a function of rainfall in year $t-1$. This effect is calculated using the estimates in column (9) of Table 2.3. The effect of rainfall on democratization is again consistent with rainfall affecting democratization through its inverted-U-shaped effect on agricultural output. The maximum variation in the probability of democratization associated with rainfall variation is around 30 percentage points.

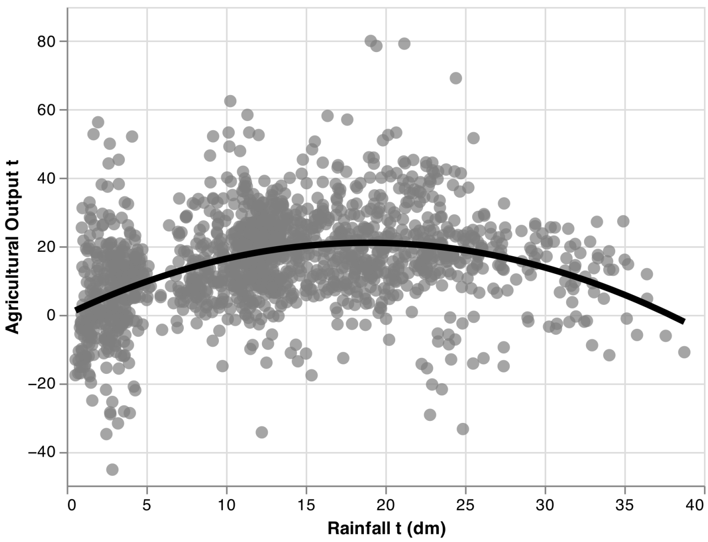
Hence, as would be expected if the effect of rainfall on democratization is through agricultural output, the inverted-U-shaped relationship between rainfall and agricultural output translates into U-shaped relationships between rainfall and the probability of democratization. The maximum variation associated with rainfall is around 20 percentage points for agricultural output and around 30–35 percentage points for the probability of democratization.

Figure 2.3 illustrates the empirical fit of the inverted-U-shaped relationship between rainfall and agricultural output for the world's most agricultural countries in Table 2.2 using an augmented-component-plus-residual plot. These plots are useful for checking on quadratic and other non-linear relationships, see Ashraf and Galor (2013), Duranton et al. (2014), Ashraf and Michalopoulos (2015), and Maertens (2021) for example. The horizontal axis measures rainfall and the vertical axis agricultural output. The inverted-U-shaped curve is agricultural output predicted by rainfall and rainfall squared. The grey dots are predicted agricultural output plus the residuals from the regression of agricultural output on all the right-hand-side variables in Table 2.2. The plot indicates that the quadratic (inverted-U-shaped) relationship describes the data well. Figures 2.4, 2.5, and 2.6 show augmented-component-plus-residual plots of the relationship between rainfall and the probability of democratization for our three main dichotomous political regime classifications. The horizontal axis measures rainfall and the vertical axis the probability of democratization. The U-shaped curves are the probability of democratization predicted by rainfall and rainfall squared. The grey dots are the predicted probability of democratization plus the residuals from the regression of the three different democratization indicators on all the right-hand-side variables in Table 2.3. These plots also indicate that the quadratic (U-shaped) relationship describes the data well.

Online Appendix Figure 1 illustrates the fit of the quadratic (inverted-U-shaped) relationship between rainfall and agricultural output and the quadratic (U-shaped) relationship between rainfall and democratization using separate binned scatter plots for the linear and quadratic terms. These plots also indicate that the quadratic relationship describes the data well.

Democratization in Sub-Saharan Africa since 1980 Table 2.5 summarizes the short-run and longer-run effects of rainfall on democratization for the sub-sample of Sub-Saharan African countries in Table 2.3 focusing on the period since 1980. This allows examining whether rainfall has a persistent effect on democratization in the region and during the more recent time period considered by Brückner and Ciccone (2011). The sample has somewhat less than half of the observations of the longest possible sample with all countries with agricultural GDP shares in the top quintile of the distribution. In addition to the effects on democratization in years t, $t+2$, $t+4$, and $t+9$, we also show the

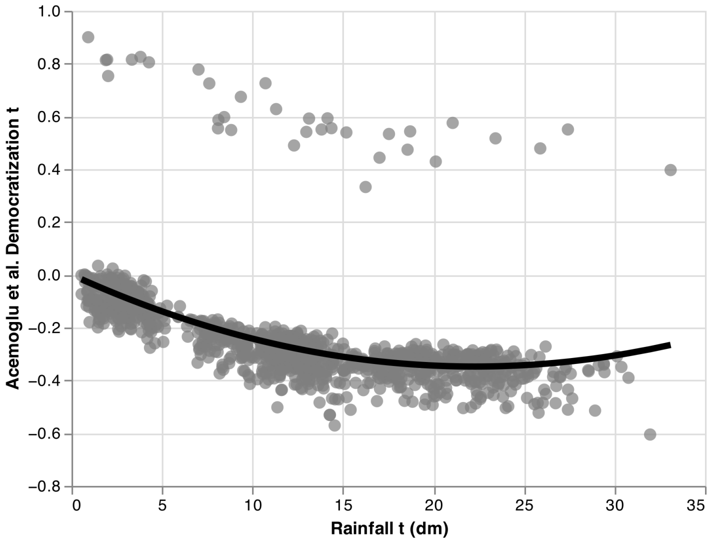
Figure 2.3: Agricultural Output: Augmented Component Plus Residual Plots



Note: Empirical fit of the inverted-U-shaped effect of rainfall in year t on agricultural output in year t using an augmented-component-plus-residual plot. The vertical axis represents agricultural output explained by rainfall and its square plus the residuals from the (full) regression of agricultural output on all the right-hand-side variables in Table 2.2.

effect in year $t+1$. The results are for the Acemoglu *et al.* and the Przeworski *et al.* democratization indicators. Results for the Geddes *et al.* democratization indicator are similar. The main finding is that rainfall continues to have a U-shaped effect on democratization in the short run and the longer run. A difference with the results in Table 2.3 is the timing of the rainfall effect for the Acemoglu *et al.* democratization indicator and that the effect only sets in after two years.

Figure 2.4: Augmented Component Plus Residual Plots for Acemoglu *et al.* Democratization



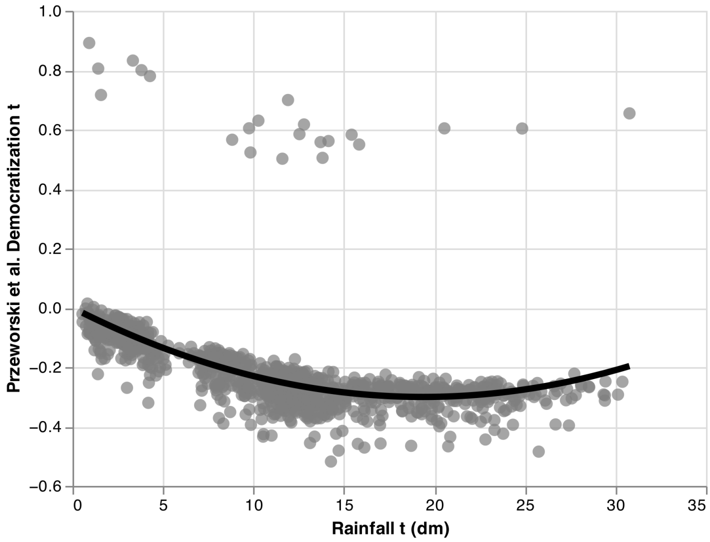
Note: Empirical fit of the U-shaped effect of rainfall in year t on the probability of democratization between years $t-1$ and t based on the classification of democratic and nondemocratic regimes of Acemoglu *et al.* (2019). The vertical axis represents the probability of democratization explained by rainfall and its square plus the residuals from the (full) regression of the democratization indicator on all the right-hand-side variables in Table 2.3.

2.3.2.2 Democratization Based on Multivalued Indices

Table 2.6 summarizes the short-run and longer-run effects of rainfall on democratic change using the multivalued Polity Project combined polity score and the Freedom House index of political rights (Marshall et al., 2014; Freedom House, 2014).

Combined Polity Project Score Table 2.6, Panel A summarizes our findings on the effects of rainfall on short-run and longer-run democratic change when the left-hand side of estimating equation (2) is democratic

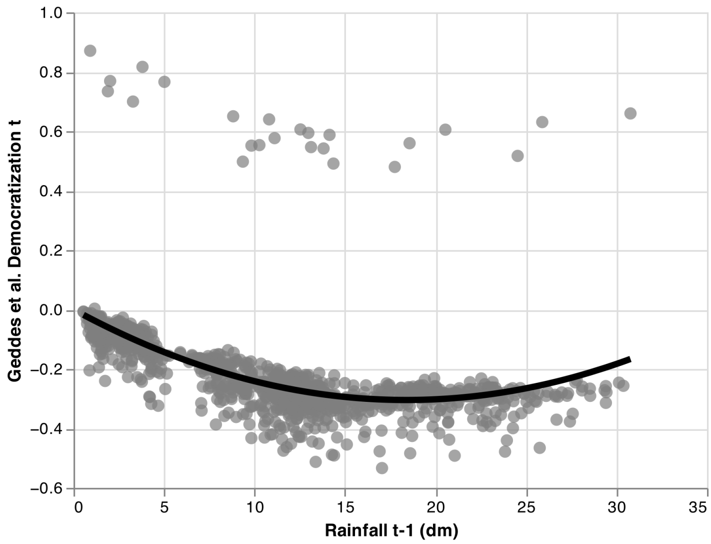
Figure 2.5: Augmented Component Plus Residual Plots for Przeworski *et al.* Democratization



Note: Empirical fit of the U-shaped effect of rainfall in year t on the probability of democratization between years $t-1$ and t based on the classification of democratic and nondemocratic regimes of Przeworski *et al.* (2000) as updated by Cheibub *et al.* (2010) and Bjørnskov and Rode (2020). The vertical axis represents the probability of democratization explained by rainfall and its square plus the residuals from the (full) regression of the democratization indicator on all the right-hand-side variables in Table 2.3.

improvement as measured by the change in the Polity Project combined polity score towards more democratic institutions. This score ranges from -10 to 10, with higher values indicating more democratic institutions. The Polity Project convention is that countries with a score smaller or equal to -1 are classified as nondemocracies and countries with a score greater or equal to 1 are classified as democracies. A zero polity score denotes a so-called interregnum where according to the Polity Project there is no government controlling most of the territory. As we are interested in improvements in democratic institutions in nondemocracies,

Figure 2.6: Augmented Component Plus Residual Plots Geddes *et al.* Democratization



Note: Empirical fit of the U-shaped effect of rainfall in year t on the probability of democratization between years $t-2$ and $t-1$ based on the classification of democratic and nondemocratic regimes of Geddes *et al.* (2014). The vertical axis represents the probability of democratization explained by rainfall and its square plus the residuals from the (full) regression of the democratization indicator on all the right-hand-side variables in Table 2.3. See page 10 for details on the convention used by Geddes, Wright, and Frantz to date the start of regime transitions.

we use the positive change between years $t-1$ and T in the polity score in nondemocracies at $t-1$ to measure democratic improvement. Negative changes, which correspond to democratic setbacks, are dropped from the analysis (results including negative changes are similar, see Online Appendix Table 6). By focusing on democratic improvements in nondemocracies, we are staying as close as possible to the analysis of democratization in Tables 2.3 and 2.4.

Table 2.6: Rainfall and Democratic Improvements since 1960: From Short to Long Term

	Panel A: Polity Project Democratic Improvement				Panel B: Polity Project Democratization				Panel C: Freedom House Political Rights			
	Polity Improvement between $t-1$ and				Polity Democratization between $t-1$ and				Political Rights Improvement between $t-1$ and			
	t (1-Year)	$t+2$ (3-Year)	$t+4$ (5-Year)	$t+9$ (10-Year)	t (1-Year)	$t+2$ (3-Year)	$t+4$ (5-Year)	$t+9$ (10-Year)	t (1-Year)	$t+2$ (3-Year)	$t+4$ (5-Year)	$t+9$ (10-Year)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Rainfall t	-0.225*	-0.654***	-0.667***	-0.619**	-0.013	-0.051**	-0.052**	-0.035	-0.063**	-0.160***	-0.272***	-0.238***
	(0.127)	(0.220)	(0.245)	(0.257)	(0.015)	(0.020)	(0.020)	(0.023)	(0.027)	(0.048)	(0.069)	(0.070)
Quadratic Rainfall t	0.005*	0.015***	0.013**	0.012**	0.000	0.001**	0.001*	0.001	0.001**	0.003***	0.006***	0.004**
	(0.003)	(0.005)	(0.006)	(0.006)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Rainfall $t-1$	-0.092	-0.129	-0.220	-0.198	-0.008	-0.023	-0.015	-0.005	0.000	-0.115**	-0.128**	-0.206***
	(0.106)	(0.193)	(0.211)	(0.276)	(0.011)	(0.017)	(0.018)	(0.023)	(0.022)	(0.045)	(0.061)	(0.078)
Quadratic Rainfall $t-1$	0.002	0.002	0.004	0.005	0.000	0.000	0.000	0.000	0.000	0.002**	0.002*	0.003**
	(0.003)	(0.004)	(0.005)	(0.007)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.002)
Rainfall $t-2$	-0.337**	-0.486***	-0.301	-0.120	-0.040**	-0.066***	-0.034**	-0.009	-0.076***	-0.158***	-0.068	-0.229***
	(0.137)	(0.186)	(0.198)	(0.269)	(0.017)	(0.018)	(0.017)	(0.024)	(0.028)	(0.048)	(0.061)	(0.077)
Quadratic Rainfall $t-2$	0.009**	0.011**	0.007	0.002	0.001**	0.001***	0.001	0.000	0.001**	0.003***	0.001	0.004**
	(0.003)	(0.004)	(0.005)	(0.006)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Countries	30	30	30	30	30	30	30	30	31	31	31	31
Observations	1,073	1,003	941	846	1,101	1,070	1,032	946	1,078	910	808	677
R Squared	0.033	0.069	0.075	0.050	0.024	0.073	0.078	0.053	0.034	0.072	0.073	0.059

Note: The left-hand-side variables in columns (1) to (4) are the improvements in the Polity IV Project combined polity score in nondemocracies over different time periods. The left-hand-side variable in column (1) is the improvement in the polity score between years t-1 and t; the left-hand-side variable in column (2) is the improvement in the polity score between years t-1 and t+2; the left-hand-side variable in column (3) is the improvement in the polity score between years t-1 and t+4; and the left-hand-side variable in column (4) is the improvement in the polity score between years t-1 and t+9. The left-hand-side variables in columns (5) to (8) are indicators for democratization over different time periods constructed as the democratization indicators in Table 3. The classification of democratic and autocratic regimes is based on Polity IV Project combined polity score. The left-hand-side variables in columns (9) to (12) are the improvements in political rights over different time periods measured by the Freedom House political rights index. The left-hand-side variable in column (9) is the improvement in political rights between years t-1 and t; the left-hand-side variable in column (10) is the improvement in political rights between years t-1 and t+2; the left-hand-side variable in column (11) is the improvement in political rights between years t-1 and t+4; and the left-hand-side variable in column (12) is the improvement in political rights between years t-1 and t+9. The included countries are all countries with an average share of agriculture in GDP over the 1970–2013 period in the top quintile of the distribution. The specification includes country fixed effects, year fixed effects, and linear quadratic contemporaneous and lagged temperature effects. The specification also includes a linear and quadratic term for rainfall lagged by three years but these terms are generally statistically insignificant and not reported for brevity. The numbers in parentheses are heteroskedastic and autocorrelation-consistent (HAC) standard errors that are robust to both arbitrary heteroskedasticity and serial correlation. * denotes significance at the 10% level; ** significance at the 5% level; and *** significance at the 1% level.

Table 2.6, Panel A shows our results for the effect of rainfall on democratic improvement in nondemocracies as measured by the Polity Project between year $t-1$ and year t (one year later); year $t+2$ (three years later); year $t+4$ (five years later); and year $t+9$ (ten years later). The effect of rainfall in year t is statistically significant and implies a U-shaped relationship over all time periods. The implied effect of the median negative rainfall shock in year t on the improvement in the polity score between year $t-1$ and t is around 0.12 polity points after one year and around 0.3 points after three years. Over five-year and ten-year periods, the improvement in the polity score is around 0.35 points.[12]

Table 2.6, Panel B contains our results for the effect of rainfall on a democratization indicator based on the dichotomized Polity Project combined polity score. We follow the convention and classify countries with a polity score smaller or equal to -1 as nondemocracies and countries with a polity score greater or equal to 1 as democracies. The results indicate a statistically significant, U-shaped relationship between rainfall and the probability of democratization over different time periods. The rainfall effect is a bit weaker than for the improvement in the polity score in Panel A (a more granular measure), but overall, results are similar.

Freedom House Political Rights Table 2.6, Panel C summarizes our findings on the effects of rainfall on short-run and longer-run democratic change when the left-hand side of estimating equation (2) is democratic improvement as measured by the Freedom House index of political rights. This index ranges from 1 to 7, with higher values indicating less political rights. Put differently, an improvement in political rights corresponds to a drop in the political rights index. To make results more directly comparable with those using the Polity Project combined polity score, where higher values indicate more democratic institutions, we use the

[12] We also find a statistically significant and U-shaped relationship between rainfall and democratic improvement between year $t-1$ and year $t+14$ (15 years later). Online Appendix Figure 2, Panel A illustrates the empirical fit of the U-shaped effect using augmented-component-plus-residual plots.

negative of the Freedom House political rights index as the basis of our empirical work. This leaves the range of the index unchanged but ensures that positive changes over time correspond to improvements in political rights. As in the case of the combined polity score, we focus on improvements in political rights and drop years where political rights deteriorate (results including negative changes are similar, see Online Appendix Table 6).[13]

Table 2.6, Panel C shows our results for the effect of rainfall on improvements in the Freedom House index of political rights. The effect of rainfall in year t on improvements in political rights is statistically significant and implies a U-shaped relationship over all time periods.[14] The implied effect of the median negative rainfall shock in year t is an improvement in political rights of around 0.03 points over one year. Over a three-year period, the increase in political rights is around 0.08 points. Over five-year and ten-year periods, the improvement in political rights implied by the median negative rainfall shock rises to around 0.12 points.

Democratization in Sub-Saharan Africa since 1980 Table 2.7 summarizes the short-run and longer-run effects of rainfall on democratic change as measured in Table 2.6 for the sub-sample of Sub-Saharan African countries since 1980. Rainfall continues to have a U-shaped short-run and longer-run effect on democratization despite the large drop in sample size. The main difference with the results in Table 2.6 is the timing of the rainfall effects.

[13]We are not looking at results in nondemocracies only as the Freedom House political rights index is not used to classify countries into democracies and nondemocracies.

[14]Online Appendix Figure 2, Panel B illustrates the empirical fit of the U-shaped relationship between rainfall and improvements in political rights using augmented-component-plus-residual plots.

Table 2.7: Rainfall and Democratic Improvements in Sub-Saharan Africa since 1980: From Short to Long Term

	Panel A: Polity Project Democratic Improvement				Panel B: Polity Project Democratization				Panel C: Freedom House Political Rights			
	Polity Improvement between $t-1$ and				Polity Democratization between $t-1$ and				Political Rights Improvement between $t-1$ and			
	t (1-Year)	$t+2$ (3-Year)	$t+4$ (5-Year)	$t+9$ (10-Year)	t (1-Year)	$t+2$ (3-Year)	$t+4$ (5-Year)	$t+9$ (10-Year)	t (1-Year)	$t+2$ (3-Year)	$t+4$ (5-Year)	$t+9$ (10-Year)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Rainfall t	-0.116	-0.567*	-0.979**	-0.656*	0.007	-0.044*	-0.100***	-0.080**	-0.044	-0.140*	-0.386***	-0.225*
	(0.200)	(0.309)	(0.400)	(0.353)	(0.021)	(0.027)	(0.035)	(0.035)	(0.042)	(0.078)	(0.098)	(0.126)
Quadratic Rainfall t	0.003	0.017**	0.020**	0.016	0.000	0.001	0.002**	0.002**	0.001	0.003*	0.009***	0.004
	(0.004)	(0.008)	(0.010)	(0.010)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)
Rainfall $t-1$	-0.027	-0.055	-0.333	-0.154	-0.006	-0.047*	-0.059*	-0.048	0.073***	-0.086	-0.062	-0.192
	(0.180)	(0.293)	(0.392)	(0.370)	(0.021)	(0.026)	(0.033)	(0.034)	(0.028)	(0.074)	(0.093)	(0.136)
Quadratic Rainfall $t-1$	0.001	-0.002	0.007	0.006	0.000	0.001	0.001	0.001	-0.001**	0.002	0.000	0.003
	(0.005)	(0.007)	(0.009)	(0.011)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.004)
Rainfall $t-2$	-0.330	-0.857**	-0.444	0.207	-0.052**	-0.131***	-0.085***	-0.047	-0.110**	-0.248***	-0.166*	-0.423***
	(0.233)	(0.344)	(0.384)	(0.359)	(0.024)	(0.031)	(0.033)	(0.034)	(0.043)	(0.072)	(0.088)	(0.135)
Quadratic Rainfall $t-2$	0.011*	0.020**	0.013	-0.006	0.001**	0.003***	0.002**	0.001	0.002**	0.005***	0.004*	0.010***
	(0.006)	(0.008)	(0.009)	(0.009)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)
Countries	22	22	22	21	22	22	22	21	22	22	22	21
Observations	454	424	392	349	463	444	416	357	616	508	445	362
R Squared	0.039	0.055	0.062	0.057	0.028	0.087	0.069	0.044	0.045	0.103	0.109	0.087

Note: The left-hand-side variables in columns (1) to (4) are the improvements in the Polity IV Project combined polity score in nondemocracies over different time periods. The left-hand-side variable in column (1) is the improvement in the polity score between years $t-1$ and t; the left-hand-side variable in column (2) is the improvement in the polity score between years $t-1$ and $t+2$; the left-hand-side variable in column (3) is the improvement in the polity score between years $t-1$ and $t+4$; and the left-hand-side variable in column (4) is the improvement in the polity score between years $t-1$ and $t+9$. The left-hand-side variables in columns (5) to (8) are indicators for democratization over different time periods constructed as the democratization indicators in Table 3. The classification of democratic and autocratic regimes is based on Polity IV Project combined polity score. The left-hand-side variables in columns (9) to (12) are the improvements in political rights over different time periods measured by the Freedom House political rights index. The left-hand-side variable in column (9) is the improvement in political rights between years $t-1$ and t; the left-hand-side variable in column (10) is the improvement in political rights between years $t-1$ and $t+2$; the left-hand-side variable in column (11) is the improvement in political rights between years $t-1$ and $t+4$; and the left-hand-side variable in column (12) is the improvement in political rights between years $t-1$ and $t+9$. The countries included in the analysis are all Sub-Saharan African countries with an average share of agriculture in GDP over the 1970–2013 period in the top quintile of the distribution or, equivalently, with an average share of agriculture in GDP above the Sub-Saharan African median. The specification includes country fixed effects, year fixed effects, and linear quadratic contemporaneous and lagged temperature effects. The specification also includes a linear and quadratic term for rainfall lagged by three years but these terms are generally statistically insignificant and not reported for brevity. The numbers in parentheses are heteroskedastic and autocorrelation-consistent (HAC) standard errors that are robust to both arbitrary heteroskedasticity and serial correlation. * denotes significance at the 10% level; ** significance at the 5% level; and *** significance at the 1% level.

2.4 Conclusion

As agriculture is harmed by both droughts and very wet conditions, the effect of rainfall on agricultural output is inverted-U-shaped (e.g., Schlenker and Lobell, 2010; Lobell et al., 2011). We confirm this inverted-U-shaped relationship for the world's most agricultural countries and also show that the effect of rainfall on agricultural output is transitory. The relationship between rainfall and democratization is U-shaped, which is consistent with rainfall affecting democratization through its inverted-U-shaped effect on agricultural output. Moreover, the U-shaped relationship between rainfall and democratization persists in the long run. Hence, as hypothesized by Acemoglu and Robinson (2001, 2006) and Besley and Persson (2019), democratic transitions can outlast the (transitory) shocks that started the democratization process.

To get a sense for the magnitude of the longer-run effect of rainfall on democratization, consider an adverse rainfall shock equal to the median year-on-year drop in rainfall in the world's most agricultural countries. Suppose this shock affects a country following a year where the rainfall level was equal to the median. Our estimates of the effect of rainfall on agricultural output imply that this shock lowers contemporaneous agricultural output by around one percentage point, but does not affect agricultural output in the longer run. Our estimates of the effect of rainfall on democratization imply that the adverse rainfall shock makes it around two percentage points more likely that the country will be democratic ten years later.

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