FINANCIAL CRISES AND RISK PREMIA[*]

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I analyze the behavior of risk premia in financial crises, wars, and recessions in an international panel spanning over 140 years and 14 countries. I document that expected returns, or risk premia, increase substantially in financial crises, but not in the other episodes. Asset prices decline in all episodes, but the decline in financial crises is substantially larger than the decline in fundamentals so that expected returns going forward are large. However, drops in consumption and consumption volatility are fairly similar across financial crises and recessions and are largest during wars, so asset pricing models based on aggregate consumption have trouble matching these facts. Comparing crises to "deep" recessions strengthens these findings further. By disentangling financial crises from other bad macroeconomic times, the results suggest that financial crises are particularly important to understanding why risk premia vary. I discuss implications for theory more broadly and discuss both rational and behavioral models that are consistent with the facts. Theories where asset prices are related to the health of the financial sector appear particularly promising. *JEL Codes:* G01, G12, E44.

I. INTRODUCTION

Why do risk premia (equivalently, expected returns) vary over time?[1] This article explores the behavior of risk premia across financial crises, recessions, and wars and uses variation in the data to help understand the economic forces behind time-varying risk premia. I use data on consumption, dividend yields, stock returns, and credit spreads for over 140 years and 14 developed countries yielding more than 60 financial crises and 200 recessions. The

1. A large body of literature documents that risk premia vary by substantial amounts (Shiller 1981; Campbell and Shiller 1988b) and understanding this variation has been called "the central organizing question of current asset-pricing research" (Cochrane 2011).

central finding is that risk premia spike dramatically in financial crises—defined specifically as a banking panic or banking crisis—but rise only modestly in recessions or wars. After documenting this fact, I examine the ability of leading macro asset pricing models to explain these facts. I find the overall drop in consumption and increase in consumption volatility is fairly similar across financial crises and recessions and is largest during wars, meaning the variation in risk premia is difficult to reconcile with standard consumption-based asset pricing models.

Figure I plots the increase or decrease of variables of interest across each of the events and provides the main results of the article. The details, data description, and formal statistical analysis are all left to the main text. Panel A documents large increases in dividend yields and credit spreads, both typical measures of risk premia, during financial crises. On average, stock prices fall by around 30% more than dividends during financial crises, representing a large increase in risk premia. Throughout the article I use the term "high risk premia" to mean an asset with high expected return, high discount rate, or low price relative to fundamentals or expected cash flows. While these measures spike in financial crises, other episodes show relatively modest movements in risk premia. Later sections give these same results via regressions and trace out the path of risk premia and consumption moments across these episodes. Panel B shows that the key state variables implied by leading macro asset pricing models—the drop in consumption and the conditional volatility of consumption—have difficulty accounting for the behavior of risk premia across episodes. These variables do not vary dramatically across financial crises and typical recessions, and each changes the most during wars. I show this further by comparing financial crises to "deep recessions"—defined as the worst third of recessions in terms of declines in consumption. While consumption falls by more and consumption volatility increases by more than during financial crises, these deep recessions do not have higher risk premia. The challenge posed to the standard consumption-based models cannot likely be overcome by changing parameters in their calibrations because the relationship between state variables and risk premia is either relatively small (across financial crises and recessions) or has the wrong sign (across financial crises and wars or financial crises and deep recessions). This poses a challenge for consumption-based asset pricing models, and in later sections I show that the calibrated versions of these models (Campbell and
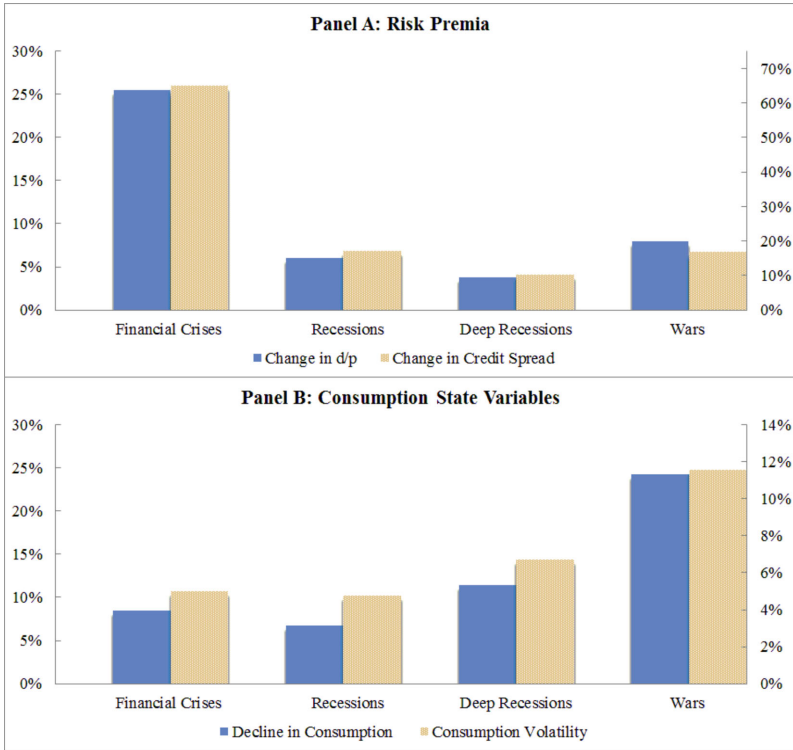
FIGURE I

Main Results

This figure computes changes in risk premia, as measured by dividend yields (left axis) and credit spreads (right axis), in Panel A across financial crises, recessions, deep recessions, and wars. Panel B plots consumption state variables argued to capture variation in risk premia: the peak to trough decline in consumption (left axis) and consumption volatility (right axis).

Cochrane 1999; Bansal and Yaron 2004; Barro 2006) have difficulty matching the behavior of risk premia in financial crises. The facts instead appear more consistent with the idea that risk premia are correlated with credit conditions and the health of the financial sector.

There are several potential objections to the results that financial crises have much higher risk premia than other events. The first objection is that the conclusions are tautological. Of course prices fall and risk premia rise during financial crises if

a "crisis" is defined ex post as a large decline in asset values. In fact, the financial crisis dates are defined as a systemic event—a major bank run or bank failure—and are not defined based on what happened to stock prices. Therefore, they are not defined ex post based on drops in asset values. However, this objection partly misses the point. Regardless of the dating convention, the fact remains that models should explain these episodes. For example, if the habits model of Campbell and Cochrane (1999) is a fairly complete description of the world, we can define changes in prices ex post, but it must still be the case that consumption (or "surplus consumption," which is consumption relative to habit) drops substantially whenever prices drop substantially. Worries that low market returns simply coincide with the crisis are also misguided. Market crashes do occur around recessions and wars as well, but these drops in market values are accompanied by drops in fundamentals, so that valuation ratios do not move drastically. The key feature of financial crises relative to recessions is the change in the discount rate, rather than the change in cash flows. One should think of the exercise of comparing crises to recessions and wars as a "difference-in-difference" type approach where in both cases the economy faces similar drops in cash flows and fundamentals, but in one episode the financial sector is particularly affected.

The second main objection is that dividend yields and credit spreads are poor measures of risk premia. Maybe the spike in dividend yields in crises is really about expected dividend growth in those episodes, even though unconditionally we don't see dividend yields strongly forecast future dividend growth. I show that dividend yields are valid measures of expected returns because they strongly forecast returns both conditional on financial crises as well as unconditionally. The results show that the standard result that dividend yields are mostly related to expected returns and less related to dividend growth hold during crises and recessions as well. These results are shown in the Online Appendix, which conducts a number of other robustness checks. Moreover, and more directly, I show that realized abnormal returns after financial crises are very large at around 20%, which is a nonparametric way of showing that risk premia in these episodes are indeed large. During crises asset prices display a V-shape pattern of collapse and recovery: cumulative returns fall by around 40%, but about half of this decline is reversed within a few years. Further, I find that the episodes with larger price drops are followed

by stronger price rebounds and that my results are not driven by influential outliers. Taken together, these results confirm that the key feature of financial crises relative to typical recessions is the discount rate effect or spike in risk premia. In contrast, in the other events subsequent realized returns are not abnormally high. In fact, the extra drop in return in financial crises versus recessions is completely reversed several years out, so there is very little difference in long-term prices or cash flows but a substantial difference in discount rates across these episodes. The main results are robust to multiple dating conventions for financial crises. They also hold for a broader set of countries that includes developing economies, they hold when only looking at U.S. data, and they hold for the postwar period.

Finally, one may worry about the long-lasting effects of financial crises and whether they are indeed far worse than the other events in the long run. Although these effects are difficult to estimate, I find the long-term effects of financial crises on consumption to be slightly worse than recessions, slightly better than deep recessions, and much better than war-related disasters. The difference in long-term effects compared to recessions remains modest in comparison to the difference in risk premia across the events. I find long-term effects of financial crises on consumption to be on the order of 2% lower than those in typical recessions. Although not trivial, from the perspective of standard models this difference is again relatively small to explain the much larger differences in risk premia across the events. Next, if one is still worried about this difference as explaining the results, I compare crises to "deep recessions" where the drops in consumption are larger—in both the short and long term—than those around financial crises. Even in these deep recessions, risk premia do not move substantially.

It is important to note that this article says nothing about the causality of the macroeconomic outcomes during recessions, crises, and wars. In particular, I do not take a stand as to whether the drop in consumption around financial crises is caused by a collapse in lending in the financial sector or whether it reflects, say, shocks to total factor productivity. In fact, this is not essential to my analysis. The underlying shocks in any episode are difficult to observe, and consumption is the endogenous outcome of these shocks along with potential amplification from the financial sector. The broader point in this article is that for the standard asset pricing models, the causes of the drop in consumption are

irrelevant and can be taken as exogenous. Similar changes in aggregate consumption—regardless of their cause—should produce similar changes in risk premia based on equilibrium relationships in those models.

These facts have implications for the type of model necessary to fit the empirical regularities documented about variation in risk premia. I can characterize various models as being inconsistent with the facts, directly consistent with the facts, or inconclusive. "Inconclusive" simply means the theory does not explicitly generate the patterns in the data, yet the data do not present direct evidence against the model. The models most directly inconsistent with the facts are standard representative agent asset pricing models where risk premia are purely a function of aggregate consumption and expectations about aggregate consumption. The models most directly consistent with the facts are models that explicitly feature financial intermediaries and/or credit conditions as being important for asset prices. This includes both behavioral and rational models.[2] These models naturally deliver the results in this article because the health of the financial sector is adversely affected by more in a financial crisis than during a normal recession. An older working paper version of this article shows that a calibrated version of an intermediary based model (as in He and Krishnamurthy 2013 or Brunnermeier and Sannikov 2014) gets into the right quantitative ballpark to match the facts documented here. The more inconclusive models include heterogeneous agent models such as limited participation and also models with idiosyncratic consumption or income risk (Constantinides and Duffie 1996), though I do show several dimensions on which these models appear promising.

While I cannot distinguish whether the high risk premia during financial crises are rational or irrational, my findings still speak to behavioral theories of asset pricing. In particular, if one believes that sentiment is the key driver of risk premia, then the facts in this article suggest that financial crises are uniquely important as being episodes with large negative shocks to sentiment. Behavioral theories would have to explain why recessions and wars do not feature equally large changes in sentiment despite the fact that investors would have many reasons to be pessimistic in

2. For example, Geanakoplos (2009), Gennaioli, Shleifer, and Vishny (2012), He and Krishnamurthy (2013), Brunnermeier and Sannikov (2014), Jin (2015), among others.

these episodes based on their negative macroeconomic outcomes. Finally, much recent work in behavioral finance has focused on investors forming incorrect expectations based on overweighting past returns or experiences (Malmendier and Nagel 2011; Barberis et al. 2015). In these models drops in asset prices are exacerbated by investors forming pessimistic forecasts of future returns and cause prices to fall below fundamentals and therefore measured risk premia to rise. These theories would have to explain why risk premia do not rise during all market downturns but only the ones associated with financial crises. In other words, there must be a drop in sentiment only during financial crises for reasons beyond poor past returns. Recent work is starting to explore the link between financial cycles and risk premia in a behavioral context (see Shleifer and Vishny 2010; Gennaioli, Shleifer, and Vishny 2012, 2013; Baron and Xiong 2017; Jin 2015). This literature appears particularly promising from the standpoint of the data presented here.

The main takeaway of this article is that, compared with other bad economic times, financial crises are uniquely characterized by large increases in risk premia or drops in asset prices relative to fundamentals. In recessions, which do not involve financial crises, I find much smaller movements in risk premia even when these recessions are particularly deep. This adds to previous work that focuses on the increase in expected returns over the business cycle.[3] This article adds to these findings by splitting bad economic times (i.e., recessions or disasters) into those involving a banking panic and those that do not. This is the first article to systematically document the behavior of risk premia during financial crises and is the first to compare this behavior to other episodes. In a related and subsequent paper, Baron and Xiong (2017) find consistent evidence that risk premia are low before financial crises when credit growth is high. Consistent with the facts presented here, their paper also suggests that asset prices are strongly related to the credit cycle. My findings have implications for asset pricing theory. At the most basic level, the results are difficult to reconcile with standard representative agent consumption based asset pricing models because those theories generally say that high expected returns simply depend on bad economic times for the average consumer. Thinking of recessions, deep recessions,

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3. Fama and French (1989), Lustig and Verdelhan (2012); see also Cochrane (2011) for a comprehensive review of the behavior of expected returns.

and wars as control groups with similarly bad or worse macroeconomic outcomes for the average consumer, this article shows the additional large increase in risk premia during financial crises despite no additional increase in typical measures of consumption risk.[4] The article provides a suggestive link between aggregate risk premia and the financial sector that is related to findings by Adrian, Moench, and Shin (2015) and Adrian, Etula, and Muir (2014), who find intermediary balance sheets help explain the time-series and cross-section of asset returns.

II. DATA AND EMPIRICAL RESULTS

II.A. Data Description

The main data span from 1870 to 2009 across 14 countries and consists of the following: real per capita consumption, dividend yields, real stock returns, and credit spreads. The countries included in the main sample are the United States, Canada, Australia, Denmark, France, Germany, Italy, Japan, the Netherlands, Norway, Spain, Sweden, Switzerland, and the United Kingdom. Consumption and GDP data are from Barro and Ursua (2008). Dividend yield, stock price, and return data are from Global Financial Data (these can also be used to construct a dividend growth series) and are converted to real U.S. dollars. I use one-year real interest rates in the United States from Robert Shiller when constructing excess returns. Credit spreads are from Investor's Monthly Manual, which published bond prices from 1869 to 1929.[5] I also add the Moody's BaaAaa default spread from 1930 onward for U.S. data as well as other additional credit spreads from Global Financial Data in the more recent post-1930 period. See the Online Appendix for more details on credit spread data. Spreads for each country are normalized by dividing by their mean for each series. Krishnamurthy and Muir (2015) show why this is a good normalization when comparing spreads of different credit quality. Thus, my spread variable represents percentage deviations of spreads from their means. Credit spreads are mainly used to provide supporting evidence for the claims in this article.

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4. See also Greenwald, Lettau, and Ludvigson (2014) who show that changes in risk premia or risk aversion explain a large fraction of asset price fluctuations in U.S. data, but these changes are uncorrelated to consumption and fundamentals.

5. See Krishnamurthy and Muir (2015) for more on the details of this data set.

TABLE I

DATA COVERAGE

Country	Consumption	Dividend yield	Return	Spread
Panel A: Main sample				
Australia	1902	1882	1882	1870
Belgium	1914	1927	1927	1960
Canada	1872	1934	1934	1872
Denmark	1845	1969	1969	1897
France	1825	1870	1896	1870
Germany	1852	1870	1870	1927
Italy	1862	1925	1925	1870
Japan	1875	1886	1886	1905
Netherlands	1808	1969	1951	1880
Norway	1831	1984	1984	1901
Spain	1851	1875	1875	1871
Sweden	1801	1915	1915	1870
United Kingdom	1831	1923	1870	1870
United States	1835	1870	1870	1870
Panel B: Additional countries				
Argentina	1876	1988	1988	NA
Austria	1914	1925	1970	NA
Colombia	1926	1988	1988	NA
Finland	1861	1962	1921	NA
Greece	1939	1977	1977	2003
Iceland	1946	2002	2003	NA
Indonesia	1961	1990	1988	NA
Korea	1912	1963	1963	1995
Malaysia	1901	1973	1973	NA
Philippines	1947	1982	1982	NA

Notes. I report the first date available for each variable in my sample from 1870 to 2011. Some variables may have missing values throughout the sample (e.g., during wars). Panel A is the main sample in the article. Panel B contains data for additional countries when using the broader set of crisis dates from Reinhart and Rogoff.

Stock market data (dividend yield data and return data) begin at various times across these countries, but are typically continuous once they begin (the main exception is a few countries during major world wars; I return to this issue later). Table I, Panel A gives the list of countries and the first date at which each series is available. Panel B contains additional countries that are used later to extend the analysis and provide robustness checks. The data are described in greater detail in the Online Appendix.

Crisis and event dates come from several sources. My main source uses business cycle dates from Jorda, Schularick, and

Taylor (2010), henceforth JST,[6] Table 1 who document business cycle peaks for these 14 countries and document whether each was associated with a financial crisis or not. Jorda, Schularick, and Taylor (2010) define a financial crisis as "events during which a country's banking sector experiences bank runs, sharp increases in default rates accompanied by large losses of capital that result in public intervention, bankruptcy, or forced merger of financial institutions." Their financial crisis dates are largely based on, and hence similar to, others in the literature.[7] The occurrence of a financial crisis is due to a major bank run or bank failure—therefore "financial crisis" and "banking panic" are used synonymously. The crises are not dated ex post by aggregate stock market declines, and in fact there are many large such declines where no financial crisis occurs.[8] Table II reproduces main dates used in this article.

I use the term "recessions" generally to mean "nonfinancial recessions" meaning recessions that do not coincide with a financial crisis, though sometimes I will use the term "nonfinancial recession" if it is unclear from the context. Finally, I define "deep recessions" as nonfinancial recessions for which the initial drop in consumption exceeds 2%. This cutoff represents the lowest 30% of recessions in terms of the initial drop in the first year of the recession and gives me roughly the same number of deep recessions as I have financial crises. However, this criteria does not condition on anything beyond the first year of the recession, hence it does not imply a look-ahead bias. An alternative definition for a "deep recession" is to condition on the recessions with the lowest peak to trough decline in consumption, which thus includes forward-looking data beyond the first year of the recession. Using a cutoff for a peak to trough decline in consumption of more than 5% gives about the same number of deep recessions. This alternative definition is discussed more later, and it produces even stronger results in favor of the conclusions of the article.

Overall, my sample contains consumption data for 209 nonfinancial recessions, 63 of which are considered deep recessions, and 67 financial crises. These numbers reduce to 135 recessions

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6. See also Schularick and Taylor (2012), Jordà, Schularick, and Taylor (2013).

7. See Bordo et al. (2001), Laeven and Valencia (2008), Reinhart and Rogoff (2009), and Cecchetti, Kohler, and Upper (2009). Many of these studies are in turn based on previous work, the list of which is too long to be included here.

8. For example, there have been only two major U.S. financial crises since World War I but many stock market declines.

TABLE II

LIST OF FINANCIAL CRISIS DATES

Country	Date	Country	Date
Australia	1891	Netherlands	1939
Australia	1894	Netherlands	2008
Australia	1989	Norway	1897
Canada	1874	Norway	1920
Canada	1907	Norway	1930
Denmark	1872	Norway	1987
Denmark	1876	Spain	1883
Denmark	1883	Spain	1889
Denmark	1920	Spain	1913
Denmark	1931	Spain	1925
Denmark	2007	Spain	1929
France	1882	Spain	1978
France	1907	Spain	2007
France	1929	Sweden	1879
France	2007	Sweden	1907
Germany	1875	Sweden	1920
Germany	1890	Sweden	1930
Germany	1908	Sweden	1990
Germany	1928	Sweden	2007
Germany	2008	Switzerland	1871
Italy	1874	Switzerland	1929
Italy	1887	Switzerland	2008
Italy	1891	United Kingdom	1873
Italy	1929	United Kingdom	1889
Italy	2007	United Kingdom	1973
Japan	1882	United Kingdom	1990
Japan	1901	United Kingdom	2007
Japan	1907	United States	1873
Japan	1913	United States	1882
Japan	1925	United States	1892
Japan	1997	United States	1906
Netherlands	1892	United States	1929
Netherlands	1906	United States	2007
Netherlands	1937		

Notes. Dates from Jorda, Schularick, and Taylor (2010). The dates give business cycle peaks associated with financial crisis episodes. See text for more details.

(43 of which are deep recessions) and 43 financial crises when I consider events that have nonmissing dividend yield data. Missing data is primarily due to dividend yield series starting significantly later in many countries, although some countries have missing data during the major world wars as financial markets shut down (particularly Germany and France). These are my

primary dates because they allow me to compare the behavior of asset prices across recessions which contain a financial crisis and those which do not, for a balanced panel of countries. This allows me to estimate the differential response of each variable in a recession versus a financial crisis where the timing of each event is the same and based on the initial macroeconomic decline. One can loosely think of the financial crisis group as the treatment group and normal recessions as the control. I will show that both are hit with similar declines in consumption but have different responses in asset prices.

I also use dates from Reinhart and Rogoff (2009) (RR) which only contain dates for financial crises but not recessions in general and show robustness of the results to using these dates. The main difference with the RR dates are that they date the crisis when the major bank run or bank failure occurs, whereas JST use the business cycle peak associated with the banking panic. Typically the banking panic occurs several months to a year after the peak (Gorton 1988) so the RR dates are usually either in the same year or one year later. However, there is also disagreement on a few crises as well. For example, RR date 1984 as a crisis, whereas this is not included in the JST dates. Overall there are very few instances of this, but again, the goal here is to provide robustness to an alternative set of dates and alternative dating convention. The main disadvantage of the RR dates is that the timing is based off the crisis date and not the business cycle peak, so the comparison to normal recessions is not as clean as the ST dates. However, they are also more extensive and cover far more countries with data on many additional countries (both developed and emerging) starting as early as 1800. I therefore analyze the results using the RR dates as well for robustness and Table I, Panel B indicates the additional countries used.

The evidence that dividend yields and credit spreads measure expected returns or risk premiums is strong. In both cases, asset prices must signal something about future returns or future cash flows, and the overwhelming conclusion is that price fluctuations have much more to do with future returns than cash flows. A long literature documents the behavior of dividend yields and excess stock returns, most notably Shiller (1981) and Campbell and Shiller (1988a, 1988b). The central fact is that dividend yields appear to strongly predict future stock returns and only very weakly forecast future dividend growth, if at all. The idea that credit spreads are related to risk premia is based on similar

reasoning: fluctuations in credit spreads appear to largely predict excess returns and not default rates (cash flows). Perhaps most notably, using 150 years of U.S. data, Giesecke et al. (2011) find that credit spreads do not predict default rates at all, much as dividend yields only weakly forecast dividend growth (see also Collin-Dufresne, Goldstein, and Martin 2001; Gilchrist and Zakrajšek 2012). That implies that these fluctuations are instead due to risk premia. There is also strong evidence that these risk premia co-move, in that credit spreads are connected to equity risk premia (for example, Keim and Stambaugh 1986; Jagannathan and Wang 1996; Chen, Collin-Dufresne, and Goldstein 2009; Adrian, Moench, and Shin 2015). The Online Appendix confirms this well-known evidence that returns do appear strongly predictable by these variables.

Following the above sources, I take increases in dividend yields and credit spreads as measures of expected returns for risky assets (both stocks and corporate bonds). I also provide a decomposition of return movements into cash flows and expected returns to study both the cash flow and discount rate news during financial crises. To do this, I follow Campbell (1991) and run a standard vector autoregression (VAR) of returns and dividend yields to decompose unexpected returns into discount rate news and cash flow news. When doing so, I demean the dividend yield and return series within each country, before and after World War II, and run a single pooled VAR. One major caveat of this approach is that I only have dividend yields going back this far historically and no other predictor variables, hence I likely assign too little of the unexpected return variation to discount rate news. This allows me to distinguish return shocks based on changes in dividend yields and expected dividend growth where the latter is defined implicitly based on the behavior of realized returns and discount rate news. Unfortunately, I am not able to directly do this computation for credit spreads, the main reason being that I do not have default data that would allow me to decompose credit spreads into cash flow versus discount rate news. However, studies that have analyzed default data find that the vast majority of spread movements are related to risk premia and not default rates. For example, using 150 years of U.S. data, Giesecke et al. (2011) find that yield spreads do not predict default at all, meaning fluctuations in spreads are largely, if not completely, about discount rate information.

TABLE III

STATISTICS ACROSS EPISODES

Variable	Description	Fin crisis	Recess	Deep recess	War
Panel A: Risk premia					
Main sample					
Δdp	5 yr change in d/p	25%	6%	4%	8%
$\max(\Delta dp)$	Peak d/p relative to initial value*	43%	23%	17%	23%
N	#observations	42	135	43	15
$\Delta spreads$	5 yr change in credit spreads	66%	15%	10%	14%
N		39	91	35	5
Additional countries from RR					
Δdp	5 yr change in d/p	22%	3%	−5%	
N		75	219	89	
Panel B: Consumption moments					
Main sample					
c	Peak to trough consumption	8.0%	6.7%	11.5%	24.3%
σ_C	Consumption volatility	4.6%	4.8%	6.7%	11.6%
N		67	209	63	24
Nonmissing stock market data					
c	Peak to trough consumption	7.6%	7.6%	12.6%	25.3%
σ_C	Consumption volatility	4.1%	4.8%	7.0%	9.7%
N		45	135	43	15
Additional countries from RR					
c	Peak to trough consumption	7.1%	6.2%	8.4%	
σ_C	Consumption volatility	5%	5%	7%	
N		75	219	89	

Notes. Panel A documents movements in risk premia. I give the mean and median five-year change in dividend yield across episodes as well as the maximum dividend yield in the five-year window around the event minus the initial value at the start of the window. For reference, a completely random window would give a max change of 22%. I also show five-year changes in credit spreads around the event as another measure of risk premia increases. The main sample focuses on the 14 developed countries shown in Table I, but I also consider a larger set of countries. Panel B documents movements in consumption moments using the peak to trough decline in consumption around the event and the 10-year forward-looking volatility of consumption. I consider the full sample and the more limited sample for which I have nonmissing stock price data.

II.B. Empirical Results

1. Summary Statistics for Each Episode. I plot the raw data in Figure I and give corresponding numbers in Table III. I plot the five-year change in dividend yields and credit spreads one year after the start of the event because this is when risk premia typically peak for both financial crises and recessions. For wars, I compute the five-year change at the beginning of the event rather than one year after because this is when risk premia are typically highest. The log change in dividend yield represents the amount

that prices fall relative to fundamentals or cash flows. In crises, the change in dividend yield is 25% versus 6% in recessions, 4% in deep recessions, and 8% in wars.[9] Credit spreads increase by about 65% during crises compared to between 10% and 20% in the other episodes. However, credit spreads are almost entirely missing during wars, so the main comparison should be across the other episodes. Moreover, it is likely that government yields during the wars contain substantial risk, so the credit spread is far less useful there.

This basic conclusion is robust to using the highest possible dividend yield in a five-year window around each event as shown in Table III. Specifically, those results choose the maximum value for the dividend yield in the five-year window, rather than using the ending value, and compare this to the initial value at the beginning of the window. This allows more flexible timing of the events. It is especially important for models where news about a recession or disaster comes out before the onset of the event. The increase in risk premia during financial crises in this flexible window is 43%, whereas the other episodes are all 23% or less. Picking a completely random window in my sample gives a peak change of 22%, again suggesting that only financial crises have abnormally large increases in risk premia.

Table III also shows results for alternate dating conventions for crises using Reinhart and Rogoff's dates, and shows results for different samples of the data. The results using the RR dates are very similar but incorporate many additional crises and thus increase the number of observations. Overall these results strengthen and support the main result using the 14 developed countries and JST dates.

I next compute consumption volatility over a forward-looking 10-year window beginning at the start of the event to try to pick up persistent increases in consumption volatility. This window does not contain the current year so does not condition on the fact that consumption drops in the current year. It instead computes volatility going forward after the event has occurred. For crises and recessions, consumption volatility going forward is around 5% (only slightly higher than unconditional volatility), whereas it is around 7% in deep recessions and 12% in wars.

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9. Median changes are not much different than average changes, suggesting that the results are not due to a few influential outliers.

Finally, I compute the peak to trough change in real consumption over each event. Using peak to trough consumption is natural because consumption often responds with a lag to asset price changes in these events, and this captures the overall severity of the event in terms of consumption loss but avoids imposing perfect timing between consumption and asset price changes. In every event, I use the corresponding variable for the country experiencing the event in each case (i.e., if Spain experiences a financial crisis or a civil war, I look at changes in asset prices, consumption, etc. in Spain at that time). Consumption declines in crises are around 8% versus 7% in recessions, 12% in deep recessions, and 24% during wars.

One can easily see the main patterns in the data: financial crises are associated with large spikes in measures of risk premia, whereas wars and recessions are not. However, drops in consumption and consumption volatility are fairly close across financial crises and nonfinancial recessions and these comparisons are even stronger in deep recessions and wars. Therefore, we can almost immediately see that models based on aggregate consumption only will struggle to fit the behavior of risk premia. This basic point illustrates the main results of this article, and the rest of this section simply makes this point more carefully and rigorously.

2. Empirical Strategy. I run regressions of my outcome variables on dummies that indicate whether a recession, deep recession, financial crisis, or war-related disaster occurred. My outcome variables are (log) consumption growth, stock returns, log changes in dividend yields, the surplus consumption ratio, consumption volatility, credit spreads, and discount rate news, though other variables are considered later. Consumption volatility is computed using a GARCH(1,1) model but is robust to simply using rolling windows. It is also robust to using the square of the difference between consumption growth and its unconditional country-specific average as a measure of conditional consumption variance.[10] The recession dummy is equal to 1 if a recession occurs for any reason other than a financial crisis (i.e., it represents nonfinancial recessions). I include 10 lags of each dummy, though including more lags does not change the results. I also include country fixed effects in every regression to account for differences in cross-country

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10. Specifically, conditional variance could be approximated as $(\Delta c_{i,t} - \frac{1}{T} \sum_{t=1}^{T} \Delta c_{i,t})^2$.

averages, and I include the lag of each dependent variable as well.[11] Finally, I follow Nakamura et al. (2013) and include a dummy for postwar data interacted with the country fixed effect to account for a trend break in growth and volatility for the roughly 30 years following World War II.[12] Specifically, I run

$$
(1) \qquad y_{i,t} = \alpha_{0,i} + \sum_{j=0}^{J} a_j 1_{fin,i,t-j} + \sum_{j=0}^{J} b_j 1_{recess,i,t-j}
$$
$$
+ \sum_{j=0}^{J} c_j 1_{deep\ rec,i,t-j} + \sum_{j=0}^{J} d_j 1_{war,i,t-j}
$$
$$
+ \phi_k y_{i,t-1} + \alpha_{1,i} 1_{(t \geq 1946)} + \varepsilon_{i,t+1},
$$

where $j = 0$ corresponds to the business cycle peak associated with the event (e.g., for financial crises it is the year shown in Table II). Figure II takes the coefficients from these regressions and plots the cumulative response for each variable of interest to a financial crisis, recession, war, and deep recession given by equation (1).[13] These impulse responses are thus given by $E[y_{i,t}|1_{event,t-k} = 1] - E[y_{i,t}|1_{event,t-k} = 0]$ where $1_{event,t-k}$ represents the dummy for whether the event happened at time $t-k$. Note that when deep recessions are included in the regression, the response for regular nonfinancial recessions becomes more complicated. The recession response will include the recession dummy coefficient, which now represents nondeep recessions, plus the deep recession coefficient

11. One issue that comes up is potential bias when using fixed effects and lagged dependent variables because country fixed effects are correlated with the lagged dependent variables. First, it should be noted that the order of the bias is $\frac{1}{T}$ hence is small in this case when T is large (in other words, this is typically a "small T large N problem"). Second, using the Arellano and Bond (1991) estimator, which takes first differences to remove country fixed effects then uses lagged values of the dependent variable as instruments, gives results that are nearly identical to those presented in the main text. Therefore, I choose to stick to the basic OLS estimates with fixed effects.

12. Nakamura et al. (2013) argue for this both because of the structural break in growth after World War II and because of changes in data collection practices following this period. An additional break in 1974 as in Nakamura et al. (2013) does not affect results. An alternative way to account for changes in these variables over time is time fixed effects. Including time fixed effects does not substantially change results, but these seem inappropriate for my setting, especially for events like wars that tend to be global in nature.

13. I do not account for cross-correlation when sampling via bootstrap as I do not find strong cross-correlation in residuals. Further, a very conservative approach that groups all observations by year and bootstraps by year does not change the results.
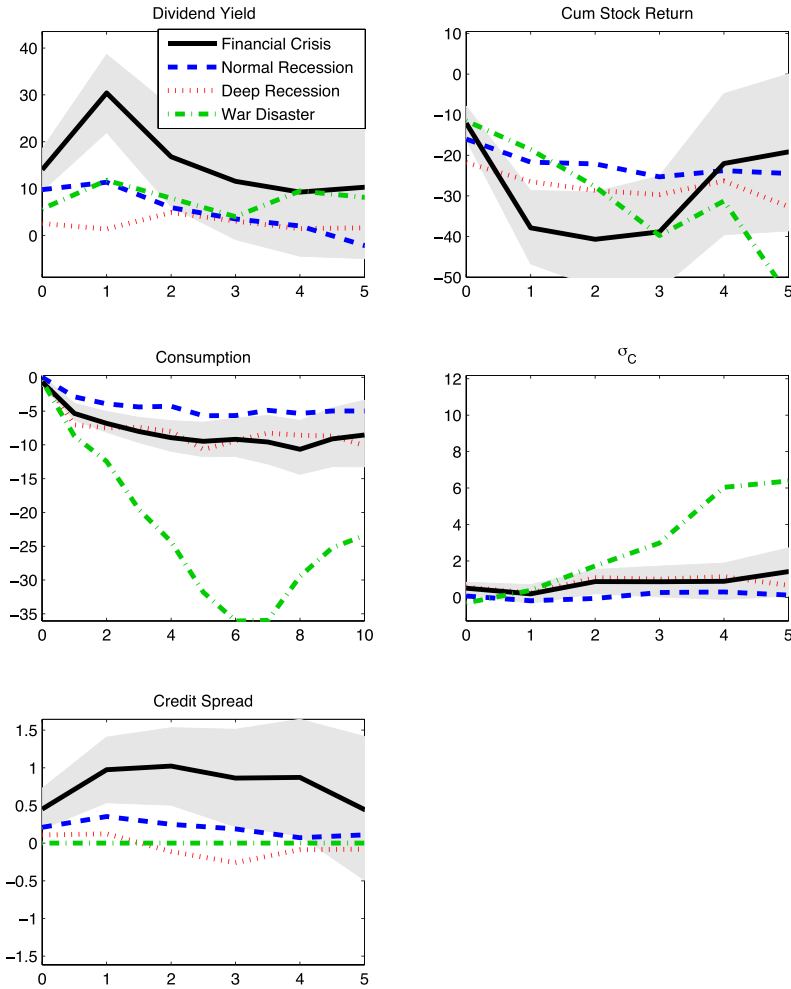
FIGURE II

Impulse Responses

This figure plots responses to each event. The *x*-axis is in years. War denotes "war-related disasters," Rec "recessions," Fin "financial crises," Deep "deep recessions" (defined as the worst 30% of recessions). I plot the dividend yield, cumulative log stock return, log consumption, and volatility of consumption, all relative to means. 90% confidence bands for financial crises in gray.

times the fraction of recessions which are deep. This is because $E[1_{deep}|1_{recession} = 1]$ is not 0 (deep recessions are a subset of regular recessions) but is equal to the probability that a recession is a deep recession. All inference regarding regular recessions holds if deep recessions are excluded and only the recession dummy is included. The plots for recessions thus represent the average nonfinancial recession and those for deep recessions represent the average deep recession.

I also plot bootstrapped 90% confidence bands for the financial crisis events. In the Online Appendix, I show results for each event separately and include bootstrapped 90% confidence bands for each event, rather than just for financial crises. Graphically, we can then evaluate whether the responses, which depend on all coefficients jointly, are statistically different from each other. This figure makes it easier to understand if the response in the financial crisis is statistically different from the response in (say) a recession at each individual horizon.

3. Financial Crises and Recessions. Starting with Figure II, we first see a large increase in dividend yields immediately after financial crises relative to typical recessions. Dividend yields increase by 15% and 31% after years 0 and 1, respectively. The 31% increase is highly statistically significant. In typical recessions, dividend yields increase by 9% on impact and then slowly decline. Dividend yields in crises are therefore significantly higher than in typical recessions, and much higher than in "normal" times. The cumulative increase in dividend yield in a financial crisis is around 30%. For reference, the standard deviation of log changes in dividend yields is around 20%. Credit spreads show similar patterns as well, again suggesting a change in risk premiums. In financial crises, credit spreads increase by around 65%. This is larger than a one standard deviation change (the standard deviation in credit spreads is about 50%). There is no meaningful change in credit spreads in recessions.

But are these changes in dividend yields actually risk premia or just changes in expected dividend growth? We know it must be one of the two based on the work of Campbell and Shiller (see e.g., Campbell 1991). To answer this, I study the behavior of returns and discount rate news. First, I show that future returns decrease dramatically but then increase. Contemporaneously, returns fall by around 20% in a financial crisis and fall by a total of around 40%. After three years, returns rebound by gaining around 20%

above their mean. The drop in returns is presumably a "shock" due to the fact that we are conditioning on the business cycle peak. The rebound, however, is forecastable by an event several years in the past. This shows that financial crises are associated with large price declines that are subsequently reversed, meaning the crisis is largely about a change in discount rates not in cash flows. Furthermore, we see a large increase and then decrease in discount rate news. The increase in discount rate news is around 20%, which is exactly commensurate with the part of returns that appear transitory. Thus, both series tell a consistent story of prices falling due to discount rate news and then rebounding.

This intuition is supported by Figure III, which shows large differences in discount rate news between the episodes. The size of the shock to discount rate news along with the changes in realized returns suggests no large differences in cash flow news across financial crises, recessions, and deep recessions. The Online Appendix confirms this intuition by running standard predictive regressions of returns and dividend growth on dividend yields both unconditionally and conditional on a financial crisis. I find that the standard relationship that dividend yields forecast returns also holds during financial crises. In my sample, dividend growth is somewhat predictable though the relationship between dividend yields and dividend growth is fairly weak. The Online Appendix discusses the approach to estimating discount rate news in more detail, following Campbell (1991).

Turning next to macro variables, we see the drop in consumption growth in recessions is around 1.1%, 3.7%, 2.0%, and 1.4% in years 0, 1, 2, and 3 after a recession, showing persistent declines in consumption. It is worth remembering that these are relative to each country's long-run average of around 1.5–2% as there are country fixed effects. For financial crises, there is no drop in consumption relative to recessions on impact, but an extra 1.5% and 1.1% drop one and two years out. This is the sense in which financial crises are deeper and longer than normal recessions. It seems unlikely that this relatively small extra drop in consumption alone would generate the large spike in risk premia. The cumulative loss in these recessions is around 5% depending on the horizon and the cumulative loss in financial crises is estimated around 7%. The response in recessions is not statistically significantly different from the response in a financial crisis, as can be seen from Figure II, which overlays these impulse responses.
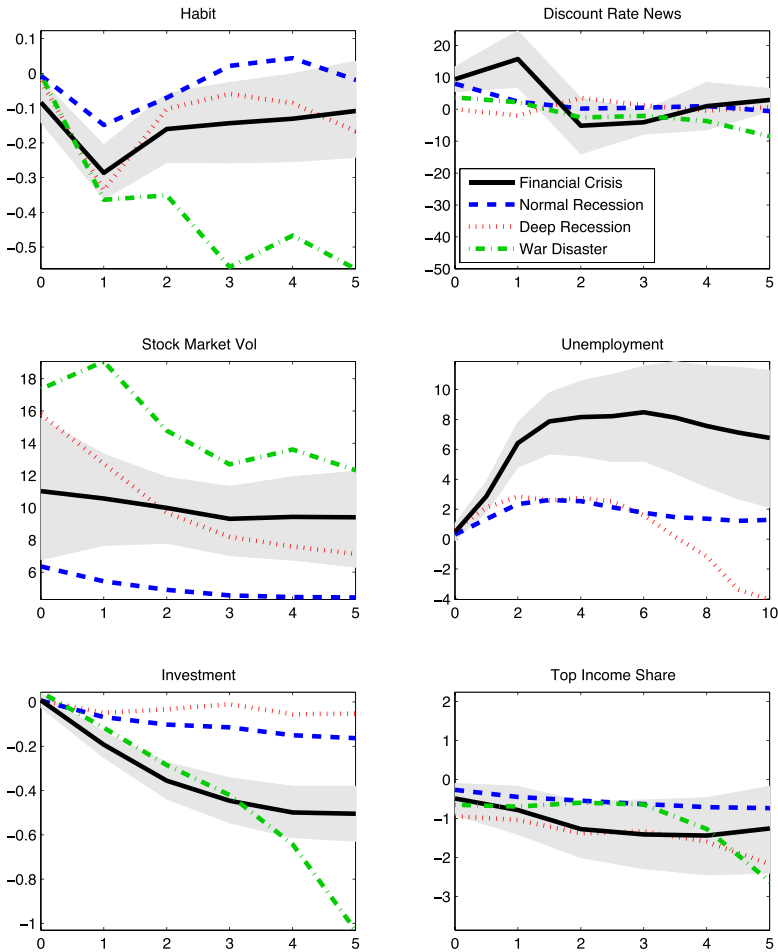
FIGURE III

Additional Impulse Responses

This figure plots the log habit or surplus consumption ratio, the normalized credit spread, discount rate news, unemployment, investment, and income share of the top 1% of earners, all relative to means. War panels are left blank when data are missing. 90% confidence bands for financial crises in gray.

Figure III looks at the habit level, or log surplus consumption ratio, of Campbell and Cochrane (1999) (CC) using their long-run calibration. Surplus consumption is essentially consumption relative to a slow-moving average of past consumption. The

patterns look fairly similar to consumption with one key difference. The habit function is nonlinear so that a bad shock in bad times decreases the log surplus consumption ratio more than in good times—as you get closer to habit you become more sensitive to drops in consumption. Does this mean the small extra drop in consumption in financial crises can generate the spike in risk premia because it comes when consumption has already fallen? No, for two reasons. First, I look at the log surplus consumption ratio, which has a roughly linear relationship with risk premia in their calibrated model. The log surplus consumption ratio declines by only around 10% extra in financial crises relative to recessions. In the CC calibration, that corresponds to an increase in risk premia of up to 6%, which cannot match the magnitude of the increase that we observe (around 20%). On top of this, the log surplus consumption ratio in the nonfinancial recession drops around 17%. The model would imply a spike in risk premia in recessions that would be much larger than what we observe in the data. Therefore, the model is not able to jointly match the lack of variation in risk premia in recessions and the large variation in risk premia in financial crises. In this case, the lack of variation in risk premia during recessions is nearly as important as the strong increase in risk premia during financial crises. Deep recession and war episodes confirm this further, where the change in habit in the deep recession is larger than during the financial crisis.

I next look at the volatility of consumption growth. This is the key state variable in Bansal and Yaron (2004) for movements in risk premia. Increases in the volatility of consumption growth increase the volatility of the stochastic discount factor. The recession dummies indicate that indeed consumption becomes more volatile following a recession, though the numbers are fairly small. Compared to recessions, financial crises have nearly identical consumption volatility with an initial increase of 0.5% relative to recessions but then a subsequent decline. Therefore, the difference in risk premia clearly cannot be accounted for by a change in consumption volatility. This is not a matter of calibration because the point estimate is not statistically significant and is economically small in magnitude. The long-run risks model would therefore predict a small increase in risk premia during recessions, but no additional increase in risk premia during financial crises. Moreover, it would predict much larger increases in risk premia in deep recessions and wars, which is counterfactual.

To summarize, purely consumption-based models are faced with the following challenge in matching these episodes: first, relying on changes in consumption volatility cannot work because volatility does not meaningfully change across episodes. The only potential way to match the data is using the extra decline in consumption in financial crises of 2%. But this has two complications. If drops in consumption lead to changes in risk premia, one has to explain why there is no substantial increase in risk premia during recessions despite the relatively large drop in consumption, and why there is no substantial increase in risk premia during wars when consumption drops can reach 30%. As I show next, deep recessions make this point even more clearly.

4. Deep Recessions. To strengthen the comparison of financial crises to recessions I also compare crises to "deep recessions," defined as those having an initial drop in consumption below -2% during the first year of decline which characterizes about 30% of the recessions in my sample. There are 63 such episodes, roughly comparable to the number of financial crises in my sample. I do not condition on any forward-looking information beyond the first year, similar to the definition of a recession, which conditions on an initial decline in growth though I consider alternative definitions of deep recessions later as a robustness check. Deep recessions make the point of this article even more clearly because relative to financial crises, consumption falls by larger amounts, consumption volatility increases by larger amounts, yet risk premia again do not increase. In deep recessions, consumption falls by more than it does during financial crises, at all horizons, and consumption volatility increases by more as well.

It is worth pointing out whether conditioning on the drop in consumption to define deep recessions leads to a bias. Most important, I do not define these episodes by the ultimate drop in consumption, simply on the drop in the first year of the recession, so there is no guarantee these episodes will ultimately be worse. Furthermore, for the habits model conditioning on ex post declines in consumption is a valid exercise. Risk premia in that model respond to realized declines in consumption, not expected ones. For long-run risk, where the key variable is the volatility of consumption, a measured bias would only potentially occur in the first year, where volatility would be measured as large because realized consumption fell by a large amount. However, I find a large persistent increase in consumption volatility many years out, far

beyond the initial year. The only model for which the dating convention here is potentially a problem is the rare disasters model where it is the probability of disaster, not the realization, that matters most, though these recessions are mostly still too mild to be considered "disasters."

5. Wars. So far I have shown that financial crises and recessions are fairly similar among consumption variables but vary greatly in terms of risk premia. I next compare financial crises and recessions to war-related disasters. These dates are from Barro and are post-1900. The results are given in the dashed and dotted lines in Figure II.

There are advantages and disadvantages to looking at the war-related disasters. The advantages are that they generate very large drops in consumption and increases in the volatility of consumption. This provides good testing grounds for models because it generates large changes in consumption moments. In other words, the increases in the consumption state variables are stark in these episodes. There are also downsides. First, there are relatively fewer of these episodes overall. But the biggest concern is that financial markets shut down during some of the major world wars preventing dividend yields from being measured. Therefore, data availability is a problem. The markets that shut down are also those that were typically hit the hardest. I therefore only use consumption data on the countries for which markets did not shut down so my data is balanced. Another approach is to measure dividend yields in a flexible window both before and after markets shut down. According to the rare disasters models (e.g., Gabaix 2012), the probability of a disaster should affect the dividend yield, and the probability is higher both before and after the realization of a disaster. However, even looking in a window around the event, I find no increase in dividend yields.

Looking at Figure II, we see no large increases in risk premia around wars as measured by dividend yields, while returns are large and negative. Consumption, however, falls dramatically by 1%, 17%, and 10% in years 1, 2, and 3, respectively, with a cumulative response eventually reaching around 35% in year 6. The surplus consumption ratio naturally collapses while the volatility of consumption increases dramatically. Notice the drops in magnitude in consumption, habits, and consumption variance are all drastically larger than in recessions by a factor of five or more. Again, the data strongly show that despite massive changes in

consumption, risk premia as measured by dividend yields do not change. As mentioned, there are of course data issues with wars, but one has to consider the economic magnitude here. Cumulative consumption losses of near 35% and an increase in consumption variance of several standard deviations produces no measurable increase in risk premia. Dividend yields must be related to expected returns or dividend growth. The only way risk premia could have been high in these wars is if expected dividend growth was equally high, so that price dividend ratios remained constant. Yet if anything we see (and would expect) negative cash flow news as returns fall dramatically but discount rate news does not change much, suggesting that cash flow news is substantially negative.

There is a possible defense of the rare disasters model: that these are just the realizations of the disaster, but maybe the probability of disaster did not increase and therefore the risk premium not increasing is natural. However, this explanation does not ultimately work. First, war-related disasters are very likely forecastable. There is usually a build-up period to wars in which events occur that substantially increase the probability of a disaster.[14] More generally, as shown in Table III, when using a flexible window five years before the disaster and looking for the maximum changes in risk premia, I still find very little evidence of any increase. It is hard to imagine that in the five-year window before these major war disasters there was no change in the likelihood of a major disaster at any point. Further, when a war actually starts consumption has not yet fallen, but clearly the probability that it will fall has gone up. In the disaster calibrations, even very small increases in the probability of the disaster occurring will have very large implications for risk premia. This is the entire foundation of these models. Even if the probability does not increase before the disaster, it should certainly increase during and after the disaster because these events tend to be clustered. Further, an agent who updates the probability of a disaster in a Bayesian way will typically expect a higher probability of disaster after one has occurred. But risk premia are not elevated during those times either. Finally, one might still argue that maybe the disasters are not forecastable, so that the probability is a

14. In unreported results, I also find that events that likely increased the probability of a large war related disaster for the United States (i.e., the Cuban missile crisis, Hitler invading Poland, the attack on Pearl Harbor) do not feature large increases in risk premia.

constant. But that would imply constant dividend yields, meaning these models won't be able to match time variation in risk premia. In any case, the standard consumption disaster model is not likely to fully explain these facts.

One may also worry that the data are poorly measured around wars. For some countries, markets completely shut down during the major world wars. First, there are enough war-related episodes without this issue that clearly show large declines in consumption and no change in dividend yield. Although there are definitely data issues, the magnitudes of the discrepancies are very large and appear difficult to attribute solely to poor data. However, as mentioned, risk premia in the rare disasters model should increase before the war begins and markets shut down as risk premia depend on the probability of the disaster, not its occurrence. These probabilities would increase leading up to wars as the wars became more and more likely. I see no change in risk premia in the lead up to wars or just after wars. Therefore, wars do not seem to be associated with large spikes in risk premia of the magnitude implied by standard disaster models.

Data issues aside, the war evidence is consistent with what we found in recessions and deep recessions: large drops in consumption and increases in consumption variance do not seem to be associated with risk premia in these episodes. Even with potential objections to the data and relatively few observations, the message seems to be clear given the large magnitudes and consistent behavior across these events.

II.C. Additional Evidence

Figure III studies the behavior of additional variables including surplus consumption, discount rate news, stock market volatility, unemployment, investment, and the top income share. Stock market volatility reacts in a similar fashion to consumption volatility, with financial crises featuring somewhat more volatility than regular recessions, but less volatility than deep recessions or wars. Hence this pattern of volatility seems to be a broader feature of the data. Surplus consumption, or "habit," appears to behave relatively similarly to consumption in terms of the differential response across episodes. The top income share evidence does not suggest that financial crises uniquely affect the wealthiest earners, suggesting that a limited participation story based on

wealthy individuals does not easily explain the results (see, e.g., Malloy, Moskowitz, and Vissing-Jørgensen 2009).

Turning to investment and unemployment, we see larger impacts of both variables in financial crises compared to the other episodes. These responses do not contradict a likely discount rate effect in financial crises. As a long literature has noted, increases in the cost of capital (or discount rates) would adversely affect investment and hiring decisions at the firm. Cochrane (1991) makes this link for investment, and recently Hall (2014) shows the same for employment.

Indeed, the data are consistent with these declines being driven by increases in discount rates. However, a major caveat is that the data are very limited for unemployment, generally being available only since 1980 for a select group of countries, meaning this evidence is based on only 10 crisis episodes, with a large influence coming from the 2008 crisis alone. In the Online Appendix, I regress future stock returns and future changes in credit spreads (a proxy for corporate bond returns) onto both investment and unemployment. Both variables have the expected sign: high unemployment signals high expected returns in the future, and low investment signals high expected returns in the future. This evidence provides further weight to the interpretation that discount rate movements in financial crises are abnormally large.

II.D. Robustness and Additional Tests

This section provides robustness checks to the main results. I first show that the results are not driven by outliers and provide more evidence that stock market losses in financial crises are driven by discount rate effects. I discuss robustness of the main results to alternative financial crisis dates, various subsamples (in particular postwar data), and to looking at only U.S. data. The main finding is reasonably robust to these alternative choices.

I start by looking not just at averages but at the distribution of returns and dividend yields across crises which are plotted in Figure IV. I begin by looking at the distribution of dividend yield increases in years 0 and 1 after a crisis when dividend yields increase by the most (e.g., see Figure II). The mean increase is around 25% with median around 20%. The fact that the mean and median are similar indicates that the increase in dividend yield is not driven by outliers. Similarly, the decline in dividend
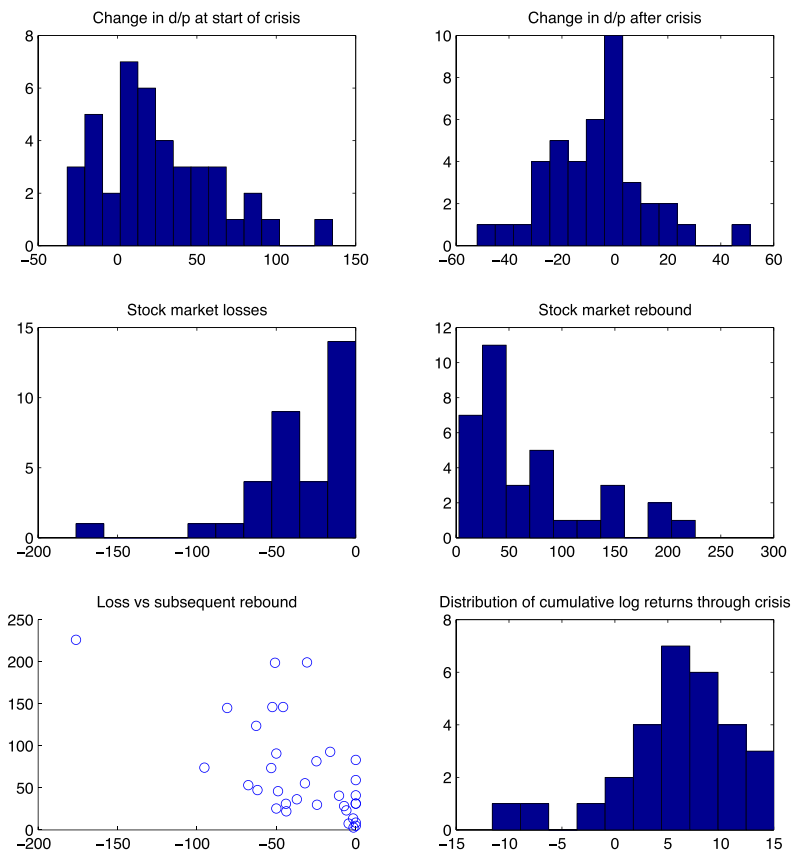
FIGURE IV

Distribution of Response

This figure gives a histogram of crisis outcomes for stock prices and provides more detail on the decline and rebound in prices during a crisis. Top left panel shows the two-year change in dividend yield from the year before the crisis $(t - 1)$ to the year after $(t + 1)$. Top right panel shows the subsequent two-year change in dividend yield from $(t + 1)$ until $(t + 3)$ to capture the reversal in dividend yields. Middle left panel shows the peak to trough decline in total log stock returns in the crisis. Middle right panel shows the subsequent rebound in returns after the initial trough is reached until the next peak. Bottom left panel gives a scatter plot of the experienced loss versus rebounds for each individual crisis/country. Bottom right panel plots the distribution of cumulative, annualized log stock returns from $(t - 1)$ to $(t + 5)$ in each crisis.

yields, which typically happens in years 2 and 3 after a crisis, is not driven outliers, as can be seen in the figure.

Next I look more comprehensively at stock market losses and rebounds around crises in the middle panels. To do so, I first define peaks and troughs in cumulative log returns using the Bry and Boschan (1971) algorithm. I search for a peak in either the year of the crisis or two years before. This constrains the peak to occur in a reasonable window near the beginning of the crisis and accords with the method in Jorda, Schularick, and Taylor (2010). Losses are defined as the cumulative return from this peak to the subsequent trough, with losses set to zero if no peak is found. Rebounds are defined as the cumulative return from this trough to the subsequent peak.

Average losses are around -35% (median -31%) over an average of two years and average rebounds are around 65% (median 48%) over an average of three to four years. Losses may not appear large relative to Figure II, but recall that Figure II shows returns relative to their country averages (stock returns average around 8% in real terms). Median values are not too far from means again, suggesting that one or two outliers do not drive results. The sum between losses and gains implies an average increase in returns of 30% cumulated over around five to six years, or a 5% annualized return compared to an unconditional annual return of 9%. This difference, compounded over five to six years, implies a total loss of around 20% relative to the mean, consistent with the earlier findings in this article that about 20% of the drop in returns during crises are "permanent" while the rest of the decline in prices is transitory and will reverse in the near future.

The lower panel of Figure IV compares losses to rebounds in each individual crisis, showing that when losses tend to be larger, rebounds also tend to be larger, consistent with a mean-reverting discount rate effect, or temporary fall in prices. The correlation between losses and future gains is -0.63 across crises and this decreases to -0.48 when the largest loss is removed. The lower right panel shows the distribution of annualized cumulative total log returns over the crisis, measured from $(t - 1)$ to $(t + 5)$. This distribution is fairly stable, with a mean and median around of around 6%. This again highlights that the large losses in each episode tend to be reversed over the longer term, so that longer term returns over the financial crisis episode are relatively stable. These results also indicate that it is not large losses during some crises and large gains in other crises that drives the overall

averages in this article; rather we see consistent evidence of temporary but large price declines that subsequently reverse within the given episode.

In the Online Appendix, I redo all of the main results with different cuts of the data. The general patterns in the results qualitatively hold when looking only at (i) postwar data (i.e., after 1950), (ii) when using the Reinhart Rogoff (RR) crisis dates instead of the crisis dates from Jorda, Schularick, and Taylor (2010), (iii) when looking only at U.S. data, and (iv) when using a balanced panel of macro and financial market data. I also find that my choice of the cutoff for deep recessions does not materially affect the results. Finally, the Online Appendix also confirms that movements in dividend yields correspond to changes in risk premia, both unconditionally and during financial crises, by running standard predictive regressions of returns on dividend yields with dummies for financial crises and recessions. The unconditional relationship that dividend yields predict returns is unaffected during these times. The Online Appendix also discusses data sources in more detail.

II.E. Causality and a Higher Frequency Approach

The results do not prove a causal relationship between the health of the financial sector and risk premia, and decisively showing this is beyond the scope of this article. However, I can still use the data to rule out certain stories. First, one may be concerned that a crisis is simply caused by a drop in asset values, which in turn causes banks to fail. In that case the causality runs from the fall in prices to the crisis. This doesn't accord particularly well with several aspects of crises. First, low stock returns occur fairly often in the data outside of financial crises. Wars, for example, feature large negative stock returns. So it must not be the case that there is a crisis any time stock prices fall. In contrast, it could be the case that movements in risk premia do cause the crisis. It is possible that investors panic and suddenly behave in a very risk-averse manner. This may lead them to run on bank debt as they become nervous and also results in high risk premia in asset markets. More generally, though, using annual data makes it difficult to assess how tightly linked the financial crisis or banking panic is with the behavior of stock prices.

To explore this issue, and to better understand the patterns of stock prices during financial crises, I use higher frequency dating
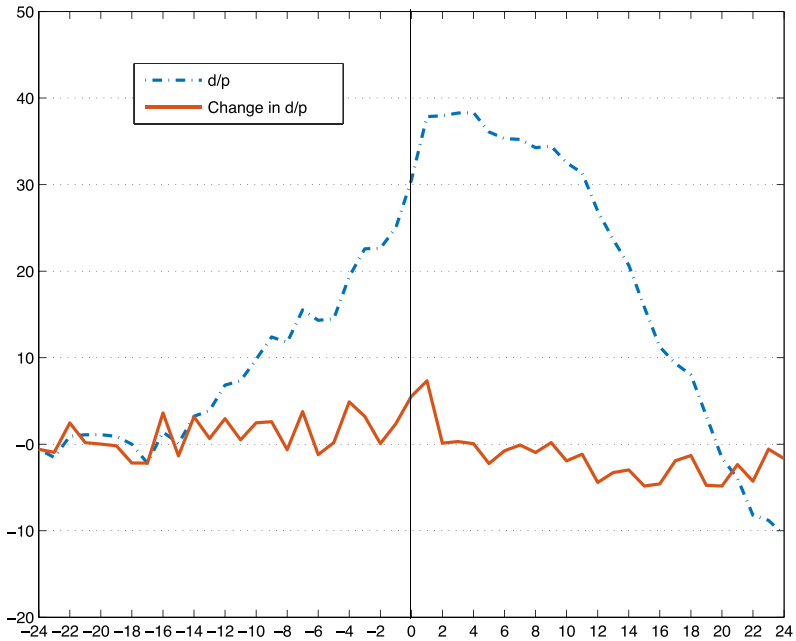
FIGURE V

Timing of Dividend Yield Spikes

This figure plots the timing of dividend yield movements surrounding financial crises at the monthly level. It uses a smaller subset of 22 financial crises which are dated to the exact month (as opposed to just the year) and for which I have monthly dividend yield data to match. The Online Appendix describes these dates more fully.

around banking panics and gauge stock market reactions. More specifically, I study a subset of 22 crises for which I have both a monthly dividend yield series and the exact month in which the banking panic occurs. These dates are from Reinhart and Rogoff, table A.4 for international countries and Gorton (1988) and Wicker (2000) for U.S. data. The dates are detailed further in the Online Appendix, and all results shown hold when studying only the U.S. banking panics. Figure V plots the average path of dividend yields and the change in dividend yields at this monthly frequency from 24 months before to 24 months after the crisis date.

Similar to the earlier results, I find nearly a 40% increase in the dividend yield surrounding these crises. This is slightly larger than the numbers reported earlier, most likely because of the exact

nature of the timing, which captures the most acute phase of the crisis as opposed to using the annual dividend yield (e.g., the crisis may occur in, say, June but the annual dividend yield is end of year and may have already receded). Of this increase, 6% occurs in the exact month of the crisis or banking panic, and 7% occurs in the month following the crisis. The monthly standard deviation of the change in dividend yields is 7%, highlighting that these are large significant increases and these two months are the largest monthly increases in the entire window surrounding the crisis. Moreover, we can reject the null that the average change in these months is zero. Thus, about a third of the increase in risk premia occurs exactly during, or even immediately after, the banking panic. A large part does increase before the crisis occurs. However, as the health of the banking sector likely deteriorates in the months prior to the crisis, it is not clear how to interpret the price decline in the run-up. It may be that a weak banking sector causes price declines or that price declines cause a weak banking sector, or both.

The other notable feature of Figure V is the rapid decline in dividend yields after the crisis, again consistent with a large price drop and large price rebound. In particular, there appears to be a fairly sharp fall in the dividend yield from a month after the crisis occurs to the following two years. Overall, the higher frequency approach confirms earlier results in the article that financial crises are associated with large changes in discount rates. It shows that these discount rate changes occur very close to the actual event of the banking panic, with the largest increases in dividend yields occurring in the month during and month after the financial panic.

III. MODELS

What type of model does one need to match the data? I first discuss models with a representative agent in more detail. As discussed at length, these models will generally say that asset prices depend only on aggregate consumption which will be problematic for the facts discussed thus far.

Then I consider intermediary models, where the role of the financial sector is explicitly considered and where asset prices depend on the health of the financial sector. These models will naturally generate the patterns in the data as the health of the

financial sector will be most affected during a financial crisis or banking panic.

Next I consider alternative models that may be able to match the facts here. Although the data suggest that a model based on aggregate consumption will generally have difficulty matching the data because the moments of aggregate consumption do not vary in the right way across financial crises, wars, and recessions, this is not necessarily inconsistent with all consumption-based models. Instead, if one takes the standard consumption-based view, then the data suggest heterogeneity is likely very important. I review heterogeneous agent models and describe the type of behavior they need to match the data.

Finally, I consider behavioral asset pricing models. A promising direction for behavioral theories are those that emphasize a link between financial cycles and asset prices (e.g., Shleifer and Vishny 2010; Gennaioli, Shleifer, and Vishny 2012, 2013; Jin 2015). This literature appears promising from the standpoint of the data presented here because it generally features asset prices being too high before a financial crisis as investors neglect risk and then has prices crashing suddenly during a crisis.

III.A. Unifying Framework

This section briefly reviews the state variables that drive risk premia in leading asset pricing models. Asset pricing models generally specify a stochastic discount factor (SDF) M. The main pricing equation is

$$(2) \qquad E_t \left[R_{t+1} \right] - R_f = -R_f cov_t \left(M_{t+1}, R_{t+1} \right).$$

The SDF, M, is typically a function of some state variables S. Therefore, the covariance and risk premia will also depend on S, and we end up with an equation of the form[15]

$$(3) \qquad E_t \left[R_{t+1} \right] - R_f = f \left(S_t \right),$$

where f is some monotonic function. Each model proposes a different state variable S that determines risk premia. I review how each model specifies S and discuss the calibrations of each model.

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15. The link is clearest in continuous time where the main pricing equation is $E_t \left[dR_{t+1} - rdt \right] = -\lambda (s_t) \sigma_{R,t}$, where $\lambda(s_t)$ is the volatility of the pricing kernel and $\sigma_{R, t}$ represents the factor loadings.

We have already seen variation in the left-hand side (risk premia) in the data between financial crises and other episodes. The key question is whether that variation can be reasonably explained by variation in the state variables.

III.B. Standard Consumption-Based Models with a Representative Agent

1. Habits. Habit models specify utility as $U(C) = (C - X)^{1-\gamma}$ where X is the habit level. I focus on the external habit model of Campbell and Cochrane (1999) where X depends on past consumption. Here the state variable is the surplus consumption ratio $H_t = (C_t - X_t)$.

$$(4) \qquad E_t\left[R_{t+1}\right] - r_f \approx \sigma_t\left(M_{t+1}\right)\sigma_t\left(R_{t+1}\right)$$

$$(5) \qquad E_t\left[R_{t+1}\right] - r_f \approx \sigma_t\left(\left(\frac{H_{t+1}}{H_t}\frac{C_{t+1}}{C_t}\right)^{-\gamma}\right)\sigma_t\left(R_{t+1}\right).$$

Consumption growth is i.i.d., and habit is based on past consumption, so the key state variable is H. Therefore high risk premia should be associated with drops in consumption (relative to habit). As noted in Campbell and Cochrane (1999), the dividend yield is nearly linear in the log surplus consumption ratio, which is the state variable I work with empirically.

To get a sense of magnitudes, I calibrate the habit model based on Campbell and Cochrane (1999). The calibration is difficult because different countries have different expected growth rates and different volatilities of consumption. Campbell and Cochrane (1999) have two calibrations—one based on postwar data and one over a longer sample. In the calibration geared to match the recent sample, habits play a larger role in the results because consumption volatility is much lower (1.2% versus 3%), and because the Sharpe ratio is larger in the recent sample. Therefore, habit must be "cranked up" dramatically to account for higher volatility of the discount factor (to match the higher Sharpe ratio) with lower consumption volatility. Therefore, in this calibration, the slope coefficient of expected return on log habit is about −3. In the model calibration, risk premia are nonlinear in the level of habit, but nearly linear in log habit level. In the long sample parameterization, the sensitivity of risk premia to log habit is only about −0.6. This would be further reduced if calibrated to international

data because international consumption volatility tends to be even higher. However, I use the long sample calibration because it best applies to the data I have and is taken directly from their original study. The estimated coefficient and estimated increase in habit in financial crises would imply an increase in expected return of around 5% in financial crises relative to recessions. Moreover, the estimated increase in risk premia during recessions imply that expected returns would rise by around 9%, whereas they would increase by around 35% during wars and around 20% in deep recessions. All in all, recessions and financial crises should not be dramatically different in terms of increases in risk premia according to the habit model, while risk premia in wars and deep recessions should be dramatically higher than both. Therefore the model has difficulty matching the data on this dimension.

2. Long-Run Risks. Long-run risks models (Bansal and Yaron 2004) feature Epstein-Zin-Weil utility and slow persistent movements in consumption and consumption volatility. Future consumption enters the SDF and hence

$$(6) \qquad E_t \left[R_{t+1} \right] - R_f = f \left(\sigma_{C,t} \right).$$

The model is log-normal, so more specifically we have

$$(7) \qquad E_t \left[r_{t+1} \right] - \frac{1}{2} \sigma^2 \left(r_{t+1} \right) - r_f = \alpha + \lambda \sigma_{C,t}^2$$

for constants α and λ. Therefore high risk premia should correspond to high consumption volatility. Return volatility also depends on consumption volatility, therefore expected excess returns are only a function of consumption volatility.

Empirically, I measure consumption volatility in two ways. I look at 10-year rolling estimates of consumption volatility and also estimate consumption volatility in each country as a GARCH(1,1) process. The estimated consumption volatility at time t uses the forward 10 years of annual data. I choose to use volatility instead of variance because units are more easily interpreted, but using variance produces similar results. In the data, consumption volatility is similar across recessions and financial crises and is much larger in both wars and deep recessions. Regardless of calibration, this model is not able to fully account for the variation in risk premia across episodes. Here the calibration is not correctly suited to the data because the original BY study focuses on

U.S. data after World War II when consumption volatility is small. Therefore, rather than using that calibration or recalibrating the model to past data, I simply point out that the model will struggle to match the facts under any parameters.

3. *Rare Disasters.* The rare disasters literature (Rietz 1988; Barro 2006; Gabaix 2012)[16] argues that asset prices and risk premia can be explained by rare disasters, which are defined as any large decline in consumption and/or GDP. Empirically, most of these disasters are major wars or financial crises. In these models the equity premium is only a function of the probability of the rare disaster, and a 1–2% probability of disaster can match the equity premium with low risk aversion. Gabaix (2012) shows that the expected no-disaster equity premium is approximately given by

$$(8) \qquad E_t\left[R_{t+1}\right] - r_{f,t} = p_t E_t\left[B_{t+1}^{-\gamma}\left(1 - R_{t+1}^{dis}\right)\right],$$

where p_t is the probability of disaster, B_{t+1} is the size or severity of the disaster (i.e., a 30% loss in output means $B_{t+1} = 0.7$), R_{t+1}^{dis} is the gross return conditional on disaster, and γ is risk aversion. Therefore the equity premium moves one-for-one with an increase in the probability of disaster, and increases exponentially with the size and potential severity of the disaster, where the sensitivity depends on the risk aversion parameter γ. Typically, the rare disasters literature exogenously specifies a process for p_t to generate both high unconditional risk premia and time-varying risk premia. In calibrated disaster models, a 2% increase in the probability of disaster would double the equity premium, so even small changes in p will have large changes in risk premia (in fact, this is the point of these models).

My findings indicate that consumption disasters cannot explain variation in risk premia because the most severe consumption disasters—wars—show little increases in risk premia while financial crises, which are comparably not nearly as severe, have much larger increases in risk premia. Consumption drops an average of 25% in a war-related disaster, compared to about 8% in a financial crisis. Therefore, B_{t+1} is clearly highest in wars.

16. Also see Liu, Pan, and Wang (2005), Kim (2013), and Wachter (2013).

Moreover, the measured B_{t+1} is similar in financial crises and deep recessions, yet there is strong variation in risk premia.

Of course measuring the probability of a consumption disaster is difficult, and my analysis so far relies on the idea that the probability of a disaster increases right before the beginning of a disaster. I think this is a reasonable assumption for wars because usually at the start of a war, or just before a major war is lost, it seems reasonable that there is an increased likelihood of consumption falling. My results show that in years right before consumption is about to fall drastically, there is no substantial increase in risk premia, so one would have to believe that the probability of a disaster is constant over that period. However, I also look at the data from several other angles and reach the same conclusion. I find that the results are robust to using the peak dividend yield in a five-year window before the disaster, which only assumes that the probability of the disaster increased at some point leading up to the event. These results were given in Table III.

III.C. Intermediary-Based Models

In intermediary-based theories the pricing kernel depends on the health of the financial sector. This is typically related to how constrained the financial sector is in raising debt and/or equity financing. The constraints affect the risk bearing capacity of the financial sector and therefore affect risk premia. Naturally, these theories imply risk premia will be highest in financial crises because these are the episodes where the financial sector is hardest hit.

Generically,

$$(9) \qquad E_t\left[R_{t+1}\right] - R_f = f\left(n_t\right),$$

where one can think of n_t as the health of the financial sector often measured by net worth. Times when n are low constitute financial crises when the risk-bearing capacity of the financial sector is particularly low and when their balance sheets are weakened. Therefore the central prediction of these models are that risk premia are highest in financial crises, consistent with what we see

in the data. Examples of these models include He and Krishnamurthy (2013), Brunnermeier and Sannikov (2014), Danielsson, Shin, and Zigrand (2011), and Adrian and Boyarchenko (2015).[17]

A key feature of these models is how financial sector leverage affects the response of prices to a shock. Specifically, a shock to asset prices when leverage is high means a large erosion in net worth or equity capital of banks. There are two potential scenarios. Either these institutions must hold the same assets with less capital, or if their situation is particularly bad they may have to sell assets in a fire sale scenario. In the former, they will typically demand a premium to bear the increased risk that comes from their capital scarcity (He and Krishnamurthy 2013; Brunnermeier and Sannikov 2014). In the latter, these fire sales will generally result in a large price discount as investors with lower valuations are left to purchase the assets (Kiyotaki and Moore 1997; Geanakoplos 2009; Danielsson, Shin, and Zigrand 2011; Adrian and Boyarchenko 2015). These lower valuations could be due to outside investors being less knowledgeable, less sophisticated, more pessimistic, or simply less willing to bear risk. There is ample evidence that financial crises are episodes where credit collapses after previously undergoing a boom (Jorda, Schularick, and Taylor 2010). This also explains why the same shock to fundamentals during a regular recession, when leverage is not high and credit growth is at a normal level, does not generate such a dramatic fall in prices because it does not significantly erode intermediary capital. Thus, these theories fit the patterns in the data well.

III.D. Heterogeneous Agent Models

This section discusses models with heterogenous agents and their implications for risk premia. The literature on limited participation implies that first-order conditions should be measured not for a representative agent but for stockholders. This can help potentially to resolve the equity premium puzzle (Mehra and Prescott 1985) with lower risk aversion because consumption of stockholders is more volatile and correlated with the stock market than aggregate consumption (see Malloy, Moskowitz, and Vissing-Jørgensen 2009 and early work by Mankiw and Zeldes

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17. See also related work by Bernanke, Gertler, and Gilchrist (1996, 1999), Kiyotaki and Moore (1997), Shleifer and Vishny (1997), Gromb and Vayanos (2002), Brunnermeier and Pedersen (2009), Moreira and Savov (2016).

1991). Guvenen (2009) provides a model with limited participation that can generate the equity premium and return predictability. For early contributions to this literature, see Basak and Cuoco (1998). Moreover, it is likely the case that there is heterogeneity in the frequency with which agents trade and optimize, so that some agents may be rationally inattentive for a period of time (Jagannathan and Wang 2007; Abel, Eberly, and Panageas 2013). Chan and Kogan (2002) show how a model with heterogeneous agents with habit formation can generate movements in risk premiums with the key state variables being the distribution of wealth between agents.

Typically, in heterogeneous agent models, the distribution of wealth matters. Without imposing additional structure it is difficult to say whether these models can match the stylized facts in this article unless we know how the distribution of wealth changes during financial crises compared to the other events.

Since precise data on the distribution of wealth is not available, I study the distribution of income as measured by the income accruing to the top 1% as a fraction of the total (see Alvaredo et al. 2014). Because the wealthy tend to participate to a much larger degree in the stock market, this provides one way to capture the heterogeneity much of these papers have in mind. Indeed Malloy, Moskowitz, and Vissing-Jørgensen (2009) show that consumption of wealthy households is more volatile, more correlated with stock returns, and can match the equity premium for lower levels of risk aversion. Although the top income data are not available for the entire sample of countries, they still provide some suggestive evidence as to whether the distribution of income behaves differently in financial crises and recessions, so helps get at this question. Figure III plots the behavior of the income distribution across events. The evidence shows a decline in the income accruing to the top 1% during crises, but this decline is nearly identical to typical recessions. It appears lower in deep recessions and wars. It is important to note that these results are highly suggestive but provide some evidence that the distribution of wealth does not go in the right direction to explain the behavior of risk premia in financial crises compared to the other events. It is important to keep in mind the limitations here that income and wealth are not equivalent. Moreover, heterogeneous models are more general than heterogeneity in wealth. I therefore consider these models essentially inconclusive, as I have no direct evidence supporting them, but neither can I say with any certainty that they are unable to fit the data.

Other promising consumption based models are models with idiosyncratic income or consumption risk (e.g., Constantinides and Duffie 1996; Schmidt 2015). In these models aggregate consumption is not the relevant variable, which is instead the cross-sectional standard deviation or skewness of consumption. This is because idiosyncratic risks are uninsurable in these models. One promising link from these models is the evidence that unemployment is particularly high during financial crises as shown in Figure III, potentially making idiosyncratic household risk increase.[18] More generally, models based on limited risk sharing appear promising if the financial sector in some way helps facilitate risk sharing so that a collapse in the financial sector leads to an increase in idiosyncratic risk.

III.E. Behavioral Theories

This section discusses behavioral theories and their implications for the facts in this article. While I cannot distinguish between whether the high risk premia during financial crises are rational or irrational, my findings do speak to behavioral theories of asset pricing as well. In particular, if one believes that sentiment is the key driver of risk premia, then the facts in this article suggest that financial crises are uniquely important as being episodes with large negative shocks to sentiment. Behavioral theories would have to explain why recessions and wars do not feature equally large changes in sentiment despite the fact that investors would have many reasons to be pessimistic in these episodes based on their negative macroeconomic outcomes. Much recent work in behavioral finance has focused on investors forming incorrect expectations based on over weighting past returns or experiences (Malmendier and Nagel 2011; Barberis et al. 2015). In these models drops in asset prices are exacerbated by investors forming pessimistic forecasts of future returns and cause prices to fall below fundamentals and therefore measured risk premia to rise. These theories would have to explain why risk premia do not rise

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18. It is also worth pointing out that models that feature a link between unemployment and asset prices will not automatically fit the data. For example, in Petrosky-Nadeau, Zhang, and Kuehn (2013) unemployment causes a consumption disaster, so aggregate consumption is still the only relevant state variable, but it endogenously falls through unemployment being high. In other words, this model reverts back to a standard rare disasters framework but is meant to endogenize disasters. It therefore still has the implication that wars and deep recessions should have equally high risk premia.

during all market downturns, but only the ones associated with financial crises. In other words, there must be a drop in sentiment only during financial crises for reasons beyond poor past returns.

Another more recent strand of behavioral finance is starting to explore the link between financial cycles and asset prices (see Shleifer and Vishny 2010; Gennaioli, Shleifer, and Vishny 2012, 2013; Baron and Xiong 2017; Jin 2015). This literature appears promising from the standpoint of the data presented here because it generally features asset prices being too high during an era of loose credit and financial innovation before a crisis and then prices crashing suddenly during a crisis. The behavior during crises is consistent with models where the crisis comes as a "surprise" or a sudden shift in beliefs. A prominent story is one where the risks of a tail event are neglected. The banking system responds to this by creating securities that are otherwise safe but exposed to this tail risk. When negative events happen and investors update their beliefs about tail risk, there is a crisis and a large decline in risky asset prices beyond the drop in fundamentals. These patterns accord well with the facts of expected returns rising dramatically during financial crises compared to the years preceding them and that this rise in expected returns is dramatic and sharp. It is also worth noting that these behavioral theories are not necessarily exclusive from intermediary theories.

IV. Conclusion

This article argues that financial crises are important for understanding asset price fluctuations and risk premia. I split bad economic events into financial crises, recessions, and wars and analyze data on consumption, dividend yields, stock returns, and credit spreads in these events for over 140 years and 14 countries. First, I document that risk premia spike dramatically in financial crises—defined specifically as a banking panic or banking crisis—but rise only slightly in recessions or wars. The financial crisis episodes feature average stock price declines in excess of fundamentals of nearly 30% and increases in credit spreads of 65% relative to average levels. The large increase in risk premia during financial crises means these are important episodes to understand from an asset pricing perspective, but equally interesting is the lack of variation in risk premia across the other episodes. These facts add substantially to the question of why risk premia vary over time and show that the behavior of risk premia during financial crises is unique. To my knowledge, this is the first paper

to study and characterize the behavior of risk premia across financial crises, and is the first to compare financial crises to other bad macroeconomic events. I examined the ability of leading asset pricing models to explain these facts. The behavior of consumption moments across financial crises, recessions, deep recessions, and wars is either roughly flat or has the wrong sign, meaning the variation in risk premia is difficult to explain using standard consumption based macro asset pricing models. Thus financial crises are different than the other events for reasons beyond the decline in aggregate consumption.

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SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

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