Asset pricing with data revisions*

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Abstract

This paper documents the asset pricing implications of the data release process of National Income and Product Accounts (NIPA) consumption expenditure. We find that early consumption data releases are more suitable for asset pricing than final revised releases. This is due to first revisions capturing genuinely novel information, whereas the remaining revisions serve to mitigate measurement error and functions as a filtering process which distorts return-consumption covariances. We then consider a new consumption-based model, the Revised CCAPM, which incorporates this release process using NIPA vintage data. It explains a striking 75% of the cross-sectional variation in average returns on the 25 size-value portfolios when featuring the first data release and its state-dependent uncertainty coming from first data revisions. We show that first revision uncertainty is strongly linked to ambiguity about consumption growth and is priced in the financial market as such. As as result, the strong performance of our Revised CCAPM is explained by state-dependent ambiguity attitudes.

Keywords: Asset pricing, data revisions, vintage data, consumption-based capital asset pricing model, NIPA personal consumption expenditures.

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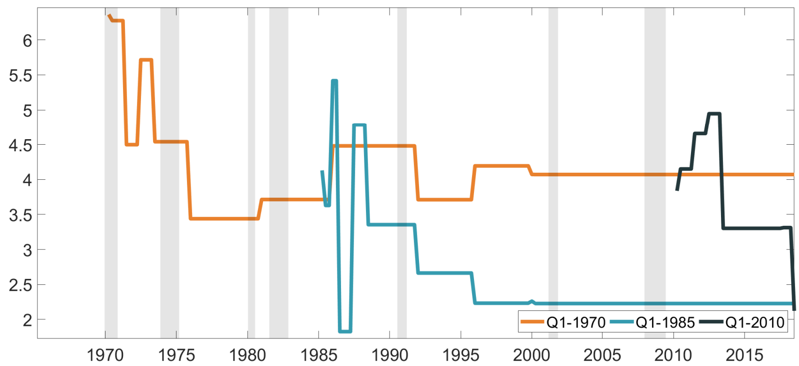
I. Introduction

Despite the structural foundation and intuitive appeal of the consumption-based capital asset pricing model (CCAPM), its empirical support is generally weak. In response, modifications to the consumption data input (e.g., Da and Yun (2010), Julliard and Parker (2005), Jagannathan and Wang (2007), Savov (2011), and Kroencke (2017)) or the inclusion of additional risk factors (e.g., Yogo (2006), Da (2009), and Boguth and Kuehn (2013)) have been proposed in order to improve the empirical asset pricing ability of the consumption-based framework. Both approaches share the commonality of using fully revised consumption data as part of their input. However, the Bureau of Economic Analysis (BEA) updates their estimates of consumption, as reported in the National Income and Product Accounts (NIPA) tables, frequently and the resulting revisions are non-negligible. To appreciate the magnitude of these revisions, we depict in Figure 1 nondurable real consumption growth for the time points 1970:Q1, 1985:Q1, and 2010:Q1 as seen through all vintages until 2018:Q2.

Figure 1: Consumption growth through vintages



This figure depicts the annualized value of aggregate consumption growth in Q1 of 1970 (1985) {2010} as reported in vintages through 1970 (1985) {2010} until Q2 of 2018 by the orange (blue) {black} solid line. Grey shaded areas indicate NBER recessions.

Revisions are large relative to the size of the first or final release and consumption data undergoes a series of revisions that continue even decades after the initial release. Moreover, first and final releases often tell a very different story about the economic state faced by the investor at time point t such that inference about the state of the economy may differ a great deal.

In this paper we document the implications of the consumption data release process for asset pricing. As argued by Aruoba (2008), Croushore (2011), and Gilbert (2011), users of this data understand the release process and recognize the inherent uncertainty surrounding the initial announcement. The relevance of this in an asset pricing context depends on the informational content of revisions and whether investors care about revisions enough to influence their portfolio allocation decisions and, as a result, asset prices today. To investigate this, we first propose a new consumption-based asset pricing model, referred to as Revised CCAPM, which incorporates the data release process using vintage NIPA data and allows us to test relevant empirical hypotheses. It features two components. First, the model uses the initial NIPA release of consumption growth as pricing factor instead of the fully revised final NIPA release. BEA (2019) explains that revisions serve to reduce measurement error of early data releases. However, Kroencke (2017) documents that final NIPA consumption is overly smooth, arguably due to mitigating measurement errors, and that unfiltering results in improved pricing performance of the CCAPM. As such, for the specific purpose of asset pricing measurement errors are less an issue relative to smoothing, and our hypothesis is that the use of first releases largely avoids the filtering process of NIPA and leads to better asset pricing performance.[1]

While the representative agent knows with certainty the amount of consumption she had in the current period, future consumption remains unknown to her, such that consumption growth is subject to uncertainty (Boguth and Kuehn, 2013). As such, the investor is likely not interested in future revisions to learn about past consumption. Rather, she might be concerned about revisions to consumption growth if they represent uncertainty today about immediate consumption growth. To capture this, the Revised CCAPM also features the interaction between the initial release and the absolute value of revisions, the latter capturing uncertainty surrounding consumption growth. This arguably better reflects the way investors interpret consumption data, see also Croushore (2011), and our empirical analyses strongly support this. The Revised CCAPM is able to explain a striking 75% (adjusted R^2 of 74%) of the cross-sectional variation in average returns of the conventional 25 size and book-to-market

[1] A related argument is that final releases include so-called annual and benchmark revisions that involve changes in methodology, e.g. seasonal adjustments or base years. These have no new fundamental information of true consumption growth, yet incorporate information not available to individuals at the relevant time, even several decades after. This in turn generates measurement errors in the final releases.

sorted portfolios. This high level of explanatory power is on par with the Fama and French (1993) three-factor model, which is directly built to explain the size and value anomaly, and it surpasses popular consumption-based benchmark models by a large margin.[2] These findings are robust to a variety of misspecification and placebo tests and hold across a large set of test assets, addressing the critique in Lewellen, Nagel, and Shanken (2010). The remainder of the paper serves to understand why asset pricing with revisions, our Revised CCAPM, is this successful.

We find that the pricing ability of the model is highest when we use initial release consumption data together with the uncertainty coming from the first revision. We also find that pricing performance decreases monotonically in the horizon over which revisions are measured. The most likely explanation for this stems from the informational content in revisions. Specifically, using the framework of Mankiw, Runkle, and Shapiro (1984) and Mankiw and Shapiro (1986) together with an analysis of revision predictability, we demonstrate that revisions can either be a source of novel information or an attempt to smooth out measurement error. We provide compelling evidence in favor of the first revisions containing genuine news and contributes with novel information while the remaining of the revisions are predictable innovations that serve primarily as devices to mitigate measurement error, consistent with the methodology described in BEA (2019). This view of longer horizon revisions as a filtering process of data is in line with the findings in Kroencke (2017), but it contradicts the typical presumption in macroeconomic-based asset pricing studies that final releases best match the information set of the representative agent (Lettau and Ludvigson, 2010). Additionally, the variances of the early releases are significantly larger than that of the final release, supporting the idea of later revisions having a filtering function. The implication is that early consumption data releases are more suitable for asset pricing than final releases. Supporting this, the consumption growth risk premia for final and first release risk without the presence of revision uncertainty are 1.29% and 2.95%, respectively. That is, the first release more than doubles the risk premia. Simply replacing the final release with the first release, excluding the revision uncertainty component, leads to an improvement in adjusted cross-sectional R^2 from 9% to 19%, further suppoting this hypothesis. As such, this also provides an

[2]Our benchmark models are the Ultimate CCAPM of Julliard and Parker (2005), a quarterly version of the Q4-Q4 CCAPM of Jagannathan and Wang (2007), and the Cay CCAPM of Lettau and Ludvigson (2001).

explanation as to why other consumption measures like garbage (Savov, 2011) and electricity (Da and Yun, 2010) yield better asset pricing results, since they are less subject to revisions and the associated mitigation of measurement errors.

We find that the first release enters the stochastic discount factor (SDF) positively and the revision uncertainty component negatively, both with t-statistics exceeding the multiple testing threshold proposed by Harvey, Liu, and Zhu (2016). The implication being that investors interpret increasing first release consumption growth as high marginal utility states and require a premium for holding assets with positively covarying returns. This reflects the conventional logic of the consumption-based asset pricing framework. On the other hand, they dislike positive covariances with the revision uncertainty component. To understand why this is the case, we investigate to what extend revisions represent either of three plausible candidates; consumption growth shocks, risk, or ambiguity.

To partly answer this question, we derive an alternative CCAPM in which revisions enter the model in levels. We simply decompose consumption growth data into the first release and subsequent revisions. Since this model is essentially a decomposition of the Standard CCAPM, revisions should enter the SDF with a positive sign for them to be consistent with consumption growth shocks. However, we find that the opposite is true, since investors associate large positive revisions with low marginal utility states and, as a result, reject the growth shocks hypothesis. We then consider a model where the additional risk factor is the absolute value of revisions. The implication of this model is that since investors do not necessarily care about the direction of revisions but they care about its magnitude, revisions are capturing some sort of risk or ambiguity. Although the two terms are often used interchangeably with ambiguity typically mentioned as uncertainty, the distinction between risk and ambiguity is both behaviorally, empirically, and theoretically important. Risk refers to situations where the distribution of random outcomes, e.g. consumption growth, is known to the decision maker, while uncertainty or ambiguity (sometimes called Knightian uncertainty (Knight, 1921)) refers to the situation where the decision maker is uncertain about the distribution of these outcomes. The pricing performance of this model is solid, but weaker than both the model that includes revisions in levels and our proposed Revised CCAPM. Yet, the price is negative and weakly significant. A natural question then arises. How come the Revised CCAPM provides so large improvements by interacting absolute revisions with the first release, when using the absolute revision alone does not? In a nutshell, why does the interaction term matter? To guide our answer, we examine the relationship between absolute revisions and empirical proxies classified as either risk or ambiguity.[3]

Using a regression-based analysis, we find a strong positive relation between absolute revisions and ambiguity proxies identified in the literature. On the other hand, we find that revisions are unrelated to risk proxies. We conclude that revision uncertainty is strongly related to the ambiguity surrounding immediate consumption growth and may be interpreted as such. Aversion to ambiguity has been documented in the literature, dating back to e.g. Keynes (1937) and Ellsberg (1961) and recently in Dew-Becker et al. (2019). Moreover, recent psychological experiments show that ambiguity aversion exists even after having seen a particular data sample (Smithson, Priest, Shou, and Newell, 2019). In our context, this means that investors might be uncertain about the underlying distribution of consumption even if they have observed all past releases.

The strong pricing performance of our Revised CCAPM can be understood in this context. Simply interacting the consumption growth signal, captured by the first release, with absolute revisions allows for a state-dependent interpretation of revision uncertainty (or ambiguity). A recent contribution by Brenner and Izhakian (2018) finds evidence of *state-dependent ambiguity attitudes* in relation to stock market returns.[4] The logic is that with a high probability of a favourable outcome, ambiguity aversion is high as investors prefer the certainty around the outcome. On the other hand, with a high probability of an unfavourable outcome, ambiguity aversion is low as investors like that this is in fact uncertain. Adopting the logic of Brenner and Izhakian (2018), our asset pricing results show that increasing ambiguity in

[3]We proxy risk as conditional or realized volatility of consumption growth as in e.g. Boguth and Kuehn (2013) and Dew-Becker, Giglio, and Kelly (2019). As measure of ambiguity we use the cross-sectional dispersion in the Survey of Professional Forecasters' (SPF) nowcast on consumption growth, adopting Anderson, Ghysels, and Juergens (2009) and Drechsler (2013), along with several non-consumption related measures such as implied stock market volatility (Berger, Dew-Becker, and Giglio, 2019b) and economic policy uncertainty (Baker, Bloom, and Davis, 2016).

[4]There is also strong evidence from the behavioral and experimental economics literature that attitudes towards ambiguity are state dependent, with ambiguity aversion for positive states (high probability of gain) and ambiguity seeking for negative states (high probability of a loss), see, e.g., Mangelsdorff and Weber (1994); Di Mauro and Maffioletti (1996); Du and Budescu (2005); Chakravarty and Roy (2009); Kothiyal (2012).

good states of high consumption growth (possibly relative to a time-varying or fixed reference point) is disliked, yet increasing ambiguity in bad states is preferred by the investor. As such, the investor requires a premium for holding assets that have returns covarying negatively with the revision uncertainty component. We estimate the total premium on consumption growth to 5.73% per year, of which 5.41% attributes to the state-dependent ambiguity. This is significantly larger than the risk premia obtained using either final or first release risk in isolation.

Our paper contributes to three broad strands of the literature. First, there is an extensive literature that seeks to improve the asset pricing abilities of the consumption-based framework. Those improvements can broadly be categorized into a group that addresses the data input, e.g., using cumulative growth rates (Julliard and Parker, 2005), Q4-Q4 growth rates (Jagannathan and Wang, 2007), garbage (Savov, 2011), unfiltered consumption (Kroencke, 2017), or fuel (Dittmar, Schlag, and Thimme, 2018), and a group that proposes additional risk factors, e.g., durable consumption (Yogo, 2006), cash flow risk (Da, 2009), or consumption volatility (Boguth and Kuehn, 2013). Our Revised CCAPM contributes to both groups. It contributes to the first by capturing the data release process of its input, showing that early data releases are more suitable for asset pricing than final releases, and demonstrates how to use and understand revisions. It contributes to the second group by incorporating revision uncertainty, i.e. consumption growth ambiguity, as an additional risk factor. As a result, our paper's second main contribution can be linked to the growing literature that examines the asset pricing implications of investor attitudes to ambiguity or uncertainty in general. Ju and Miao (2012) develop a structural consumption-based asset pricing model that resolves many of the empirical puzzles faced in the Standard CCAPM, and Thimme and Völkert (2015) and Lee, Min, and Kim (2019) find supporting evidence of the relevance of ambiguity risk from the cross-section of stock returns. Bali, Brown, and Tang (2017) document that economic uncertainty in general is a priced risk factor, carrying a negative risk premium. Our findings support the idea of ambiguity or uncertainty being priced in the financial markets. This is, however, due to state-dependent ambiguity attitudes as in Brenner and Izhakian (2018). Our findings also suggest that revisions are a novel and natural proxy for ambiguity. Finally, our paper contributes to the large and active literature on vintage data and revisions (Mankiw et al., 1984; Mankiw and Shapiro, 1986; Aruoba, 2008; Croushore, 2011). Apart from understanding their informational content, this literature mainly focuses on the implications in a forecasting context, e.g., Ghysels, Horan, and Moench (2017) and Clements (2019), or for policy rules (Orphanides, 2001). To our knowledge, Christoffersen, Ghysels, and Swanson (2002) is a lonely contribution that addresses the data release process in a classical cross-sectional asset pricing context. They focus, however, on the publication lag and not on consumption nor its revisions or first release. Ferson and Harvey (1992) consider the impact of seasonality adjustments in an consumption-based framework, broadly related to the present paper, and Bell and Wilcox (1993) examine the impact of measurement error in retail surveys on economic implications in general. Gilbert (2011) documents the relevance of revision announcements on S&P 500 Index daily returns and find that revisions matter with a positive relationship in good states and vice versa. None has, to our knowledge, analysed the implications of data revisions nor their economic interpretation for asset pricing, which is our main contribution.

The rest of the paper is laid out as follows. Section II introduces the theoretical foundation for asset pricing and motivates the consumption-based framework. It documents the relevance of consumption growth revisions and presents our main model, the Revised CCAPM, that features the data release process. Section III shows baseline results for the asset pricing performance of the Revised CCAPM and a number of misspecification, robustness, and placebo tests, as well as a discussion of consumption growth risk premia. We analyze the informational content of revisions in relation to the news versus noise hypothesis and their economic interpretation as to whether they constitute consumption growth shocks, risk, or ambiguity. In response, we address the implication for interpretation of the pricing results for the Revised CCAPM in this context. In Section IV, we explicitly address our choice of tests assets and conduct a thorough robustness check on both the pricing performance of the Revised CCAPM as well as associated economic interpretation. Section V concludes and discuss implications for past and future research.

II. Asset pricing with data revisions

In this section, we briefly review the theoretical foundation for asset pricing and the consumption-based framework. This facilitates the presentation of our Revised CCAPM that conducts asset pricing with data revisions, motivated from an analysis of their empirical characteristics.

A. Asset pricing and the consumption-based framework

Absence of arbitrage ensures the existence of a stochastic discount factor (SDF) denoted by m_{t+1} that prices all real asset returns according to

$$\mathbb{E}\left[m_{t+1}r_{i,t+1}|\mathcal{F}_t\right] = 0, \tag{1}$$

where $r_{i,t+1}$ is the real excess return of asset i relative to the risk-free rate and $\mathcal{F}_t$ is the information set pertaining to time period t. The SDF captures states of marginal utility growth and prices stocks according to their returns within those states. Under a linear factor model framework,assuming without loss of generality that factors are de-meaned, m_{t+1} is linear in risk factors f_{t+1}

$$m_{t+1} = 1 - \lambda' f_{t+1}, \tag{2}$$

where λ are SDF loadings. These loadings determine how marginal utility growth is affected through the dynamics of f_{t+1} and, as such, how a factor influences the pricing of a given stock. Together, (1)–(2) imply expected excess returns given by

$$\mathbb{E}[r_{i,t+1}] = \lambda' \text{cov}[f_{t+1}, r_{i,t+1}], \tag{3}$$

which in turn yields the unconditional beta representation of the form

$$\mathbb{E}[r_{i,t+1}] = \gamma' \beta, \tag{4}$$

where γ are the factor risk premia and β the corresponding factor exposures defined as the regression coefficients from a multiple regression of returns on the factors. As such, the risk premia can be inferred directly from the SDF loadings in (1) or (3) via $\gamma = \Sigma_f \lambda$, where Σ_f is the covariance matrix of the risk factors.

Regardless of choice of utility function, the standard log-linearized CCAPM (Lucas, 1978; Breeden, 1979) posits that

$$m_{t+1} \approx 1 - \lambda c_{t+1}, \tag{5}$$

where c_{t+1} is the logarithmic growth rate of perishable consumption. In other words, aggregate marginal utility growth of consumption, $u'(C_{t+1})/u(C_t)$, C denoting aggregate consumption, is determined by a linear function in consumption growth. Due to (4) the implied beta representation is, ignoring the approximation in (5),

$$\mathbb{E}[r_{i,t+1}] = \gamma^c \beta_i^c, \tag{6}$$

where

$$\beta_i^c = \frac{\text{cov}[r_{i,t+1}, c_{t+1}]}{\text{var}[c_{t+1}]}. \tag{7}$$

The theoretical foundation of the standard CCAPM imposes that $\lambda^c, \gamma^c > 0$, reflecting the intuition that assets with increasing returns in good states characterized by high consumption growth and low marginal utility growth are considered risky, causing the investor to command a compensation for holding the assets.

Application of this Standard CCAPM requires data on consumption growth c_{t+1}. The common approach is to gather most recent data as reported in the NIPA tables maintained by BEA. Most macroeconomic variables, including consumption growth, are, however, substantially revised by statistical agencies in the periods following their initial releases, cf. Figure 1. As argued by e.g. Aruoba (2008), Croushore (2011), and Gilbert (2011), users of this data understand the release process and the inherent uncertainty surrounding the initial announcement. The relevance of this in an asset pricing context depends on the informational content of revisions and whether investors care about revisions enough to influence their portfolio allocation decisions (consumption versus investment in the context of CCAPM) and, as a consequence, asset prices today. We will investigate this below, starting with summary of the characteristic of consumption growth revisions.

B. Characterizing consumption revisions

An accounting-like identity dictates that a given data release pertaining to time period t and released k periods later can be decomposed into the first release $c_{t|t}$ and subsequent revisions $v_{t|t+k}$ as

$$c_{t|t+k} = c_{t|t} + v_{t|t+k}, \tag{8}$$

for $k = 0, 1, \ldots, T - t$ and $t = 1, \ldots, T$.[5] If $k = 0$ we obtain the initial announcement and $v_{t|t+k} = 0$. If $k = T$ we obtain the most revised, up-to-date release of c_t of consumption growth pertaining to time period t (i.e. the last point of the three lines in Figure 1). In the literature, applications of the Standard CCAPM framework have exclusively relied on $c_{t|T}$ as consumption data.[6] Statistical agencies make methodological changes, changes in base year or seasonal corrections. These typically occur at about every five years for NIPA variables on top of annual revisions (Aruoba, 2008; Croushore, 2011; Gilbert, 2011).[7] We refer to this type of revision as benchmark revisions. The informational content of such revisions may not be of great interest compared to revisions that provide genuine news about consumption growth. For this reason, we consider a wide range of k to examine whether and how investors interpret specific types of revisions both in this section and in the asset pricing section below.

We obtain quarterly vintage data on real nondurables consumption expenditure growth, henceforth referred to as consumption growth, as reported in the National Income and Production Accounts (NIPA) over the period 1965:Q1 to 2018:Q2 from the Archival Federal Reserve Economic Database (ALFRED) at the Federal Reserve Bank of St. Louis.[8] We then define the k-ahead revision as

$$v_{t|t+k} = c_{t|t+k} - c_{t|t}. \tag{9}$$

Table 1 reports several statistics associated with the vintage data of consumption growth across the full period. We also report results conditional on being in an NBER expansion or recession. Panel A, which looks at the full sample, indicates that the mean of revisions at all horizons are positive and economically large, but only statistically different from zero for $k = 4$. Conditioning on the business cycle, revisions during expansions are large, positive, and statistically significant at conventional

[5] A given data release can be further decomposed into a component that captures the publication lag, typically by one or two months. This is particularly relevant in a forecast context, as documented in Ghysels et al. (2017).

[6] As an exception and using the variables in Chen, Roll, and Ross (1986), which does not include consumption growth Christoffersen et al. (2002) use the lagged first data release and not the final data release in an application to the 25 size-value portfolios of Fama and French (1993).

[7] For further details, see `http://www.bea.gov/help/faq?faq_id=126`.

[8] To avoid continamination from revised data, we do not adjust for population growth since it is substantially revised but only available in vintage data form from 1999 and onwards. Moreover, we focus on nondurable goods only, as in e.g. Julliard and Parker (2005). This follows recommendations by Wilcox (1992) who advocate treating nondurable goods separately from services in empirical studies.

levels for all k. It is interesting to note that the most significant results are obtained for revisions announced closely to the initial release. During recessions, revisions are large and negative, but only significant for $k = 1$. This suggests revisions to nondurable consumption are biased, in the sense of being non-zero, and follow the economic cycle. In fact, the initial release is weakly positive in recessions, with revisions causing the final series to being substantially negative on average. In summary, the initial announcements appear biased in a systematic manner. The third column, which shows the noise to signal ratio, $\text{var}[v_{t|t+k}]/\text{var}[c_{t|T}]$, are all above 0.4, documenting substantial variance in revisions relative to the final release. Interestingly, the initial announcement has a higher variance than the final release with revisions reducing the variance quite substantially over time. This has a strong implication for the informational content of revisions and their pricing ability, which we will examine further below.

C. The Revised consumption-based CAPM

We have now established that revisions are both large in magnitude and exhibit substantial variance relative to the size of initial (or final) release data. To see the implication for the consumption process in an asset pricing context, we will return to the fundamental linear SDF pricing expression in (2). We modify the Standard CCAPM model in (5) to the following form

$$m_{t+1} = 1 - \lambda^{\text{first}} c_{t+1|t+1} - \lambda^{\text{rev},k} c_{t+1|t+1} \cdot |v_{t+1|t+1+k}|, \tag{10}$$

where we denote the terms associated with revision by k to indicate their dependence on choice of revision horizon. We refer to $c_{t+1|t+1}$ as the first release component (FRC) and $c_{t+1|t+1} \cdot |v_{t+1|t+1+k}|$ as the revision uncertainty component (RUC), denoting it by φ^k_{t+1} for expositional clarity.[9] Inserting into the fundamental pricing equation in (1) and taking unconditional expectations implies the following expression for expected

[9]We note that future revisions and the first release are not available at time t to the investor. This is of no concern in this asset pricing context. The reason is that as long as investors know revisions will happen, they will position themselves in the assets in the market today according to a preference of hedging or taking the associated RUC risk. Moreover, this is fully consistent with the conventional use of final, revised data in the Standard CCAPM and other consumption-based models, e.g., the Ultimate (long-run) CCAPM of Julliard and Parker (2005) who uses future cumulative consumption growth, or Eiling, De Jong, Laeven, and Sperna Weiland (2019) who uses future labour income as risk factor.

returns

$$\mathbb{E}[r_{i,t+1}] = \lambda^{\text{first}}\text{cov}[c_{t+1|t+1}, r_{i,t+1}] + \lambda^{\text{rev},k}\text{cov}[\varphi^{k}_{t+1}, r_{i,t+1}]. \tag{11}$$

The model contains two risk components, where the first depends on the covariance between first release consumption growth and returns and the second depends on the covariance between the RUC and returns. Note that if $\lambda^{\text{rev},k} = 0$ (or alternatively setting $k = 0$), the model collapses to the Standard CCAPM using the first data release as risk factor instead of the final release which has been used so far in the literature. We will refer to this model as *First CCAPM*. Comparing this models' pricing abilities to that of the Standard CCAPM enable inference on the appropriates of using first versus final data releases. The presumption in the financial literature is that final releases best match the information of the representative agent and, as such, is the appropriate choice of measure to use, see e.g. Lettau and Ludvigson (2010). However, as argued by Kroencke (2017), in constructing this final release, NIPA statisticians aim at reducing measurement errors through a filtering process. If revisions represent this process by NIPA statisticians, they will be hurtful to the asset pricing ability of the consumption growth risk factor by removing possibly useful elements of the consumption signal without a need (in an asset pricing context) as measurement errors has no influence on return-consumption covariances, see Kroencke (2017). We investigate both empirically below. Moreover, as shown above, consumption growth is revised several decades after its first release due to benchmark revisions which adjust methodology such as seasonal corrections or base years. These adjustments have no new fundamental information of true consumption growth, yet incorporate information not available to individuals at the relevant time. This accumulates generates measurement errors in the final releases and a misrepresentation of the actual information set of the representative agent at time t.

Allowing for $\lambda^{\text{rev},k} \neq 0$, revisions may enter the SDF through the interaction of its absolute value and the first release. We will refer to this model as *Revised CCAPM*. We use the absolute value of revisions to capture the fact that revisions represent uncertainty and not contemporaneous consumption growth shocks, thus what matters is the size of revisions and not their sign. This distinction is something we document and discuss in great detail in Section E, showing that revision uncertainty from the BEA is indeed strongly related to the general uncertainty about consumption growth.[10] As such, this model maintains that investors care only about consumption growth for making portfolio decisions, as in the Standard CCAPM framework, yet incorporate the uncertainty and associated impact on marginal utility coming from the fact that immediate consumption growth is uncertain to the investor.

Several additional interesting hypotheses can be entertained. First, we are interested in whether consumption growth prices assets in the sense that the factor enters the SDF. This amounts to testing $\mathbb{H}_0 : \lambda^{\text{first}} = 0$, with the related hypothesis to test whether, from an asset pricing perspective, first release consumption data leads to a better measure of consumption than final release. Secondly, we want to test whether the uncertainty coming from revisions matter, i.e. whether the RUC belongs to the SDF. This amounts to testing $\mathbb{H}_0 : \lambda^{\text{rev},k} = 0$. Third, the sign of the SDF loadings are interesting since they inform about how the FRC and RUC enters the SDF, i.e. how they influence marginal utility growth.

It is also of interest to examine the risk premia investors' require in compensation for those two types of risk as captured by γ^{first} and $\gamma^{\text{rev},k}$ in the following implied beta representation

$$\mathbb{E}[r_{i,t+1}] = \beta_i^{\text{first}}\gamma^{\text{first}} + \beta_i^{\text{rev},k}\gamma^{\text{rev},k}. \tag{12}$$

We expect the risk premium on the first consumption growth release to be positive and in line with the general notion of immediate consumption growth risk in the Standard CCAPM. The second risk premium explicitly captures the fact that the interpretation of the first consumption growth release is dependent on its uncertainty. As such, this interaction captures the risk premia arising from uncertainty about immmediate consumption grwoth. For now, we will be agnostic about the sign of the risk premia of this second term and defer our discussion of a plausible explanation for the sign of the risk premia to Section E. In that way we follow the principle ideas of the approach in Dew-Becker et al. (2019) and extract market participants' evaluation of a given risk source, here the RUC, from their portfolio decisions. The total consumption growth risk premium is given by the sum of these two risk premia, adjusted for the sign according to the risk exposure.

[10] We show results and discuss the implication of revisions entering in levels as an additional risk factor in Section E.

Interestingly, this model is also implied by a conditional one-factor model with the first release risk as the only risk factor and $|v_{t|t+k}|$ the conditioning information in a linear specification of the dynamics of $\tilde{\beta}_{i,t}^{\text{first}}$ given by

$$\tilde{\beta}_{i,t}^{\text{first}} = \beta_i^{\text{first}} + \beta_i^{\text{rev},k} |v_{t|t+k}|. \tag{13}$$

Linear specifications of this kind have been used by, among others, Lettau and Ludvigson (2001) in a conditional CCAPM using the consumption-wealth variable as conditioning information, or by Cochrane (1996) in an investment-based asset pricing model. In our case, the uncertainty of the first release either dampens or propagates the risk associated with the consumption growth shock captured by the first release, depending on the sign of $\beta_i^{\text{rev},k}$. As such, the compensation for immediate consumption growth risk is dependent on the precision or uncertainty surrounding the signal on consumption.

III. Empirical results

In this section, we first introduce the econometric methodology for our pricing applications. We then present baseline results for the 25 size-value portfolios of Fama and French (1993) and an analysis of the main drivers of the empirical findings. We discuss the information content of revisions and examine whether revisions relate to consumption growth shocks, risk or ambiguity.

A. Econometric methodology

We follow the literature and estimate our asset pricing models using the conventional Fama and MacBeth (1973) two-pass methodology as in, e.g., Eiling (2013). In the first stage, we estimate unconditional risk exposures for each asset, i. This can either be done by a multivariate time series regression including simultaneously all risk factors as covariates or one-by-one for each factor, effectively computing single regression betas. If the risk factors are completely orthogonal, the outcome of the two approaches would be identical. As noted by Jagannathan and Wang (1998), Kan, Robotti, and Shanken (2013) and Feng, Giglio, and Xiu (2019) this distinction is important since determining whether a particular factor has additional pricing power can only be answered through the significance of SDF loadings arising from univariate beta regressions. In other words, if the question we are asking is whether or not a given factor belongs in the SDF we need to use univariate betas, as delineated by (3). On the other hand, if our interest is on understanding the risk premium of a given factor, we need to use multivariate betas, as delineated by (4).[11] Thus, to test the model, make inference on whether RUC risk belongs in the SDF and understand implications for marginal utility growth we use univariate betas on the first stage. Univariate betas are also the appropriate tool to use when testing model misspecification by augmenting the second-stage cross-sectional regression with firm characteristics. We then use multivariate betas to understand the pricing of RUC risk and investors' required premium for taking this risk. It is important to note that pricing errors, hence cross-sectional R^2 and model-implied returns, are unaffected by whether we use univariate or multivariate betas on the first stage.

On this basis, we compute conventional Fama-Macbeth t-statistics on parameters on the second stage regression. However, Kan et al. (2013) document that substantial standard error adjustments are needed when using non-traded factors due to errors-in-variables coming from the first-stage estimation of SDF loadings. Accordingly, we compute GMM standard errors that are robust to errors-in-variables as well as to heteroskedasticity and autocorrelation by Newey and West (1987) with a Bartlett kernel and data-driven lag selection based on Andrews (1991) as in, e.g., Dittmar et al. (2018).

B. Benchmark models

To compare the performance of our Revised CCAPM to existing approaches, we implement three widely acknowledged modifications to the Standard CCAPM that have shown strong empirical performance. The first model is the Ultimate (or long-run) CCAPM from Julliard and Parker (2005), which is based on the idea that an asset's covariance with ultimate consumption risk, as measured by the three-year future consumption growth rate, is a better measure of the riskiness of that asset. As noted by Julliard and Parker (2005) the performance of the ultimate CCAPM as a linear one-factor model approaches that of the Fama and French (1993) three-factor model and the Lettau and Ludvigson (2001) three-factor model. Moreover, this model is not rejected when explaining the value premium (Golubov and Konstantinidi,

[11] As noted by Feng et al. (2019) the distinction is important since a factor can command a nonzero risk premium even if it does not belong to the SDF just because it is correlated with one of the true factors that belong in the SDF.

2019). The second benchmark model is the Q4-Q4 CCAPM from Jagannathan and Wang (2007) that uses the fourth quarter year-over-year consumption growth as risk factor.[12] This model is also related to Da and Yun (2010), Da, Yang, and Yun (2015) and Møller and Rangvid (2015). The core tenet of these models is that investors are more likely to review their consumption-investment decision during the fourth quarter of the year. Jagannathan and Wang (2007) show that the Q4-Q4 model performs almost as well as the Fama and French (1993) three-factor model. The third benchmark model is the Cay CCAPM used in Lettau and Ludvigson (2001), which is a conditional specification of the original CCAPM where conditionality is modelled by the log consumption–aggregate wealth ratio.[13] We implement the models using final, revised data as in the original papers.

C. Asset pricing results for size and value portfolios

We follow custom by initially selecting the 25 Fama and French (1993) size and book-to-market sorted portfolios as test assets. Partly because they constitute an economically interesting cross-section, and partly because they form a common ground for the asset pricing models considered in the present paper and other studies. As such, for this present application, we also implement the three-factor Fama and French (1993) model which is explicitly designed to capture the return patterns observed in these 25 size-value test assets.

Table 2 reports the estimates of λ and associated t-statistics, adjusted cross-sectional R^2, and mean absolute pricing error (MAPE). We confirm the poor performance of the Standard CCAPM using final, revised data. The SDF loading is insignificant and the R^2 is 9%. A 45-degree pricing error plot in Figure A.1 illustrates that the model is incapable of capturing the large cross-sectional variation in realized average excess return by cross-sectional variations in beta. Employing the First CCAPM, one obtains a notable increase in pricing ability relative to the Standard CCAPM with an R^2 of almost 19% and more than double the size of the SDF loading. It is borderline insignificant, nevertheless. While the performance is not overwhelming in absolute terms, more than doubling the R^2 simply by replacing final, revised data with the

[12] Similarly to Boguth and Kuehn (2013), we use fourth quarter year-over-year consumption growth as risk factor for the entire year, until new data on fourth quarter is available.

[13] Data on the cay variable is from Martin Lettau's personal website, https://sites.google.com/view/martinlettau/data.

first release suggests that first release data is a better measure of the consumption growth that investors actually care about. That is, if consumption risk is priced in the financial markets it is most likely that associated with the first release and not the final one, strongly contradicting the typical presumption that final releases are the most appropriate representation of investors' information set. We examine this further in Section C.1 below.

Turning to the Revised CCAPM, the pricing performance is striking. With first revisions, $k = 1$, the adjusted R^2 is as high as 74% and MAPE about half of that of the Standard CCAPM and the First CCAPM. The SDF loading of the first release remains positive, but is now strongly significant. The SDF loading on the RUC is negative and is strongly significant as well.[14] Due to nestedness of the First CCAPM within the Revised CCAPM, we may evaluate the statistical significance of the increase in R^2 by testing $\mathbb{H}_0 : \lambda^{\text{rev},k} = 0$ (Kan et al., 2013). As such, the increase in R^2 is also statistically significant. The graphical illustration of this improvement in pricing performance can be found in Figure A.1. The deviations from the 45-degree line are small in magnitude. It is interesting to note that the pricing ability of the Revised CCAPM with revisions decreases monotonically as k, the revision horizon, increases. At $k = T$, using so-called final revisions, the pricing performance is close to that excluding revisions altogether, albeit using the first release. The SDF loading of first releases is insignificant for $k > 4$, with opposite and unintuitive sign. Taken together, this evidence suggest that early data revisions of the first release are important economically and statistically.[15] The implication is that investors associate revision uncertainty with risk. In other words, it appears as if investors base their portfolio decisions on the information contained in the first release, but care about inherent uncertainty about immediate consumption growth.[16]

The adjusted cross-sectional R^2 and MAPE of the benchmark models are reported

[14] Both t-statistics are greater than three and passess, as such, the multiple-testing corrected threshold proposed by Harvey et al. (2016).

[15] The notable shift in sign of SDF loadings and pricing performance at $k > 4$ aligns well with the fact that benchmark revisions occur at an annual frequency or lower.

[16] We have also conducted a similar asset pricing exercise using factor mimicking portfolios constructed on the first release and absolute revisions using least squares weights, see e.g. Jagannathan and Wang (2007), and 10 industry portfolios obtained from Kenneth French's website. Results are qualitatively identical as the Revised CCAPM delivers an adjusted cross-sectional R^2 of 53% and t-statistics on the first release and revision components of 3.3 and -2.9, improving upon an R^2 of 8.1% obtained by the First CAPM. The monotonic decrease in k is maintained as well.

in Panel B of Table 2. Among the benchmark models, the three-factor Fama-French possesses the best pricing abilities (R^2 of 70%), followed by the Ultimate CCAPM (R^2 of 57%) and the Cay-CCAPM (R^2 of 47%). Keeping in mind that the three-factor Fama-French model is explicitly designed to price the test assets in this application, its strong performance is not surprising. Nonetheless, it is notable that even though it has one less factor, our Revised CCAPM with $k = 1$ outperforms the Fama-French model slightly on the basis of R^2 and is just slightly inferior on the basis of MAPE. This is striking, considering that the Revised CCAPM contains non-traded factors as opposed to traded factors in the Fama-French model.

C.1. Revision horizon, predictability, and news versus noise

To get a further understanding of the striking increase in pricing performance coming from the addition of the RUC risk with $k = 1$ as well as the associated monotonic decrease in accuracy when the revision horizon increases, we first characterize the informational content of revisions using the principles in Mankiw et al. (1984) and Mankiw and Shapiro (1986). This involves assessing the variance profile of successive data releases. If variance increases in revision horizon, it lends support to the news hypothesis that revision are unpredictable and contribute with genuinely novel information.[17] On the other hand, if the variance decreases it supports the noise hypothesis that revisions are measurement errors which serve to improve the signal-to-noise ratio of the consumption process.

Figure 2 depicts the variance profile of consumption growth releases. The pattern suggests that first revisions, $k = 1$, are special in that their informational content is genuine news, increasing the variance from the first to second data release. On the contrary, all remaining revisions support the noise hypothesis, with the variance of each data release decreasing monotonically for $k = 2$ until $k = T$. The monotonic decrease in pricing performance of the Revised CCAPM in k is perfectly consistent with this pattern. Relatedly, earlier data releases appear less smooth than the final release, suggesting an underlying filtering process behind fully revised NIPA data.[18] This aligns well with the results in Kroencke (2017) who unwinds the filtering process

[17]The reasoning is that if a given data release is rational for the final release, it will be smoother than the forecast objective. Revisions then bring new, unexploited information, which allows the data release to become closer, thus with higher variance, to the true value.

[18]Supporting this hypothesis, the variance of the first and second release are statistically significantly larger than that of the final release based on a standard F-test (p-values less than 0.1%).

in consumption data that arguably serves to mitigate measurement error and finds stronger pricing performance of the CCAPM using unfiltered consumption growth as risk factor. He concludes that in contrast to measurement error, filtering is fatal to asset pricing. More importantly, this aligns well with the improvement in pricing performance using our First CCAPM, which suggests that using early data releases instead of final ones results in similar effects as Kroencke (2017) obtains through unfiltering.

If first revisions ($k = 1$) were strongly predictable, they would likely draw less attention and, as such, be characterized by noise according to the principles of Mankiw et al. (1984), Mankiw and Shapiro (1986), Aruoba (2008), and Croushore (2011). To elaborate on the results above, we consider a forecasting study aimed at illuminating the degree of predictability of revisions. We do this in the context of conventional predictive regressions. Specifically, we are interested in a model of the type

$$\nu_{t|t+k} = a + bc_{t|t} + dx_t + \varepsilon_{t|t+k}, \tag{14}$$

where x_t denote an exogenous predictor. When $d = 0$ and $k = T$ the regression in (14) reduces to a news hypothesis regression employed in e.g. Faust, Rogers, and Wright (2005) and Aruoba (2008). The null hypothesis of interest is rationality of first releases, i.e. unpredictability of revisions, $\mathbb{E}[\nu_{t|t+k}|\mathcal{F}_t] = 0$, defined by $\mathbb{H}_0 : a = 0, b = 0, d = 0$. That is, testing if the benchmark model

$$\nu_{t|t+k} = \varepsilon_{t|t+k} \tag{15}$$

suffices. This can be tested using a standard Wald test and appropriate standard errors. We consider both results under the restriction $d = 0$ together with the maximum predictability found using the best predictor in our set of exogenous predictors. This set of predictors is derived from the literature on consumption growth forecasting, containing various financial (e.g. stock market returns), macroeconomic (e.g. labour income) and survey-based (e.g. consumer confidence index) variables. Data sources and full results can be found in the Appendix. To generalize the findings, we conduct both an in-sample analysis where we estimate (14) over the full sample period and a proper out-of-sample analysis where (14) is estimated recursively with an expanding window and forecasts generated accordingly. We set the initial training window to

15 years (60 periods), and generate the first forecasts for 1980Q1. For out-of-sample forecast evaluation we use a modified version of the Campbell and Thompson (2008) out-of-sample R^2, R^2_{OoS}, where the benchmark model is zero instead of the recursive mean of the series, i.e,

$$R^2_{OoS} = 1 - \frac{\sum_{t=1}^{T} \left(\nu_{t|t+k} - \widehat{\nu_{t|t+k}}\right)^2}{\sum_{t=1}^{T} \left(\nu_{t|t+k}\right)^2}. \quad (16)$$

This modification is done to ensure that the benchmark model is consistent with (15). Conclusions are unaltered if we use the recursive mean of the series as a benchmark model, indicating the first release and the exogenous predictors predict revisions above just getting the mean right.

Table 3 reports in Panel A the results using the first release only and in Panel B minimum Wald p-value, maximum R^2, maximum R^2_{OoS}, and minimum Diebold-Mariano p-value across all predictive regressions, capturing the strongest evidence in favour of predictability at each revision horizon. Overall, both panels echo the conclusion from the informational analysis above using the principles of Mankiw et al. (1984) and Mankiw and Shapiro (1986). Using the first release only, first revisions are unpredictable both in-sample and out-of-sample. Predictability then strongly increases by increasing the revision horizon, rejecting the null hypothesis of no predictability on conventional levels at $k = 2$ and forward. Allowing for exogenous predictors, conclusions are the same, though in one instance (using the Michigan consumer confidence index) we reject the null of no in-sample predictability for $k = 1$ on 10% significance level. This is, however, not true out-of-sample. Moreover, correcting for evident multiple testing using a Bonferroni correction renders the in-sample predictability for $k = 1$ insignificant at all conventional levels, but retains significance for $k = 2$ at a 5% level and the remaining horizons at a 1% level. For out-of-sample predictability, we have run both the realitity check of White (2000) and the test of superior predictive ability in Hansen (2005) for addressing multiple testing issues.[19] Both test agree that predictability at $k = 1$ is non-existent, borderline significant at 10% for $k = 2$ and significant at the 1% level for the remaining revision horizons. These predictability results are consistent with the view that first revisions contain

[19]Consistent with the definition of R^2_{OoS} we used squared prediction errors and in the implementation of the test employed a stationary block bootstrap with 10,000 resamples and a studentized test statistic which generally leads to stronger power (Hansen, 2005).

news, whereas the remaining revisions generally serve to mitigate measurement errors. We will, in the following, primarily focus on the Revised CCAPM with $k = 1$ for above reasons and simply refer it as Revised CCAPM.

D. What drives this result?

The improvement in pricing of the 25 size and book-to-market sorted portfolios from using the initial consumption growth release and its revision uncertainty is striking. In this section, we conduct a set of analyses to examine the drivers of this results.

D.1. Model identification tests on betas

As discussed in Kan and Zhang (1999) and Burnside (2016), a natural diagnostic check that addresses concerns about weak model identification with non-tradable and possibly useless factors is to examine the joint significance and cross-sectional dispersion in (univariate) factor betas. We do this with a series of Wald tests, see e.g. Eiling (2013), Dittmar et al. (2018) and Delikouras and Kostakis (2019) for related approaches. We conduct the following five Wald tests: i) whether the 25 portfolio betas are jointly equal to 0 ($\mathbb{H}_0 : \beta_i = 0 \; \forall i$), ii) whether the 25 portfolio betas are jointly equal to one another ($\mathbb{H}_0 : \beta_i = \beta_j \; \forall i, j, i \neq j$), iii) whether the 25 portfolio betas are jointly equal to the average beta, $\bar{\beta}$, ($\mathbb{H}_0 : \beta_i = \bar{\beta} \; \forall i$), iv) whether the maximum portfolio beta is less than or equal to the minimum portfolio beta ($\mathbb{H}_0 : \beta_{\max} \leq \beta_{\min}$), and v) whether the beta of the portfolio with highest average excess return, $\beta_{\max(r)}$, (the small value portfolio in this sample) is less than or equal to the beta of the portfolio with smallest average excess return, $\beta_{\min(r)}$, (the small growth portfolio in this sample) ($\mathbb{H}_0 : \beta_{\max(r)} \leq \beta_{\min(r)}$).

The results reported in Table 4 indicate that we can reject each of the first four hypothesis for the first release betas on conventional significance levels, while the last hypothesis is not rejected. For the revision uncertainty component, we strongly reject the first four hypothesis on a 1% level and the fifth on a 5% level. We note, however, that half of the spreads in corner portfolio betas are significant on conventional levels. Taken together, these results alleviate the potential concern of weak identification of the Revised CCAPM and spurious results since betas are both jointly significant and show significant cross-sectional dispersion. In other words, we may conclude that the 25 size and book-to-market sorted portfolios covary significantly with the stochastic discount factor embedded in the Revised CCAPM and that these risk exposures are significantly dispersed cross-sectionally, reflecting the dispersion in portfolio excess returns.

D.2. Placebo tests

Despite the evidence in the former section, a reasonable concern may remain that revisions are simply white noise that happen to line up in a convenient manner and that the large improvements in adjusted R^2 and MAPE we observe are only due to chance. As noted by, e.g., Bryzgalova (2016) the cross-sectional R^2 in the second stage regression of the Fama-Macbeth approach can often be inflated by the presence of a useless factor, thus, we perform a set of Monte Carlo experiments to test this hypothesis as an additional robustness test. In a first test, we generate 10,000 bootstrap samples, of the same length as the original series, by row-wise re-sampling (with replacement) from the revision series. Since only the revision series is reshuffled, any possible dependence between the portfolio returns and the pricing factor is broken, thus the factor is said to be useless. Moreover, reshuffling revisions also breaks the link to the specific first release it pertains. For each bootstrap sample, we follow the econometric methodology outlined in Section A, focusing on the R^2 of the second stage regression and the MAPE. We can then determine the (empirical) probability of obtaining an R^2 measure as large as the one obtained with the revision series when the useless factor is used. A possible critique of this method is that single row re-sampling removes any possible serial correlation in the revision series. To accommodate this possibility we generate 10,000 placebo series that match the best ARMA(p,q) sample fit on the revision series, determined by the BIC.[20] Innovations for the placebo series are drawn from the normal distribution with variance matching the sample variance of the revision series. Finally, we also perform a test where the placebo series is pure white noise with variance given by the sample variance of revisions.

The results for these triad of tests are shown in histograms in Figure 3, which depicts the empirical distribution of second stage adjusted R^2's and MAPE from the Revised CCAPM but replacing revisions with placebo revisions. The black dotted line shows the R^2 and MAPE obtained by using the first release only while the blue line shows

[20]We find the that the revision series are best characterized by a ARMA(1,1) process.

the adjusted R^2 and MAPE obtained by adding the revisions uncertainty component.[21] In general, we find that it is highly unlikely that our results are spurious. The reason is that the probability of finding a fit as good as the one we find in the main results in Table 2 for the Revised CCAPM and $k = 1$ is between 0.57% and 0.72% for adjusted cross-sectional R^2 and 0.25% and 0.36% for MAPE, depending on our assumption about the data generating process for the useless factor. This is equivalent to saying that the improvements in R^2 and MAPE for the RUC are significant at the 1% level. In unreported results we find that the equivalent measure for $k = 2$ and $k = 4$ lie between 12% and 25% for the former and 19% and 41% for the latter, making them statistically insignificant at conventional levels.

D.3. Adding size and value characteristics

Kan and Zhang (1999) show that useless factors may appear statistically significant if the Fama-Macbeth methodology is applied to a misspecified model, and Jagannathan and Wang (1998) show that this misspecification can be tested for by including firm characteristics as additional explanatory variables in the cross-sectional asset pricing tests. They show that a useless factor cannot drive out firm characteristics in the second-stage regressions, whereas a large t-statistic on characteristics suggests that the model may be misspecified. That is, if the firm characteristics carry no significant SDF loading, it does not contradict the assertion of a correctly specified model. We report here results including stock characteristics as explanatory variables in the cross-sectional regression. Specifically, we augment the model similarly to Boguth and Kuehn (2013) as

$$\begin{aligned}\mathbb{E}[r_{i,t+1}] = \lambda^{\text{first}}\text{cov}[c_{t+1|t+1}, r_{i,t+1}] + \lambda^{\text{rev},k}\text{cov}[\varphi^k_{t+1}, r_{i,t+1}] \\ + \lambda^{ME}\overline{ME}_i + \lambda^{BM}\overline{BM}_i,\end{aligned} \tag{17}$$

where $\overline{X}_i$ is the time-series average of $X_{i,t}$ which is comprised by the market capitalization $ME_{i,t}$ and the book-to-market ratio $BM_{i,t}$ for asset i. Those are arguably the most relevant characteristics, given our present choice of test assets. Table 5 reports the results for the Standard CCAPM, the First CCAPM, and the Revised CCAPM. First, as expected, the two characteristics bring substantial explanatory power to the

[21] Note that since we use the adjusted R^2 measure it is possible for the model with both the first release and the RUC, using placebo series, to be less than that using the first release only.

Standard CCAPM and First CCAPM as indicated by their adjusted R^2 of 71-72%. In those models, the characteristics belong to the SDF statistically significantly, and consumption risk does not, consistent with the main results in Table 2. This may indicate misspecification of those models. On the contrary, firm characteristics are not included statistically in the SDF when added to the Revised CCAPM and the first release risk and RUC risk remain statistically significant on 5% and 1% levels, respectively, with same sign, yet somewhat smaller magnitude. The adjusted R^2 is only marginally increased compared to the model without characteristics, cf. Table 2, and correspondingly the MAPE only marginally reduced. As such, this test does not contradict the assertion that the Revised CCAPM is a correctly specified model without spurious factors.

D.4. Risk premia and factor exposures

While the analysis based on univariate betas is suitable for understanding whether a given factor belongs to the SDF (in the presence of other factors) and informs about implied utility states, multivariate betas are useful for understanding the success of the Revised CCAPM via factor exposures (multivariate betas) and their associated risk premia. It has no influence on pricing errors and cross-sectional R^2, yet matter for interpretation. Following Cochrane (2005) and Feng et al. (2019), we report in Table 6 multivariate betas and their differences among the smallest and largest size quintiles for all book-to-market quintiles and vice versa. We also report the average excess returns for each portfolio. Reading down the columns of Panel A, average returns generally decrease in size, confirming the size premium, except in the case for the low book-to-market ratio quintile. Reading across the rows, average returns generally increase in book-to-market ratio, confirming the value premium.

Panel B reports the betas from the Standard CCAPM. It is clear that the beta decreases in size for a given book-to-market ratio quintile, whereas it is not related to book-to-market ratios in any consistent way. The difference in betas between the high and low book-to-market ratio quintiles is also small compared to that of the small and big size quintiles. As such, the weak pricing performance of the Standard CCAPM is partly due to its inability to explain the value premium, see e.g. Yogo (2006) for a similar conclusion. On the contrary, first release betas, reported in Panel C, has large spreads in both the size and value dimension. On the size dimension, first release betas show a similar pattern as final release betas, but contrary to final release they also exhibit an increasing behavior going from low to high book-to-market ratio quintiles. This means that using the first release helps in explaining the value premium. Panel D reports betas associated with the RUC. It is clear that the spread in both size and value dimensions can be large, particularly for medium-to-high book-to-market ratio quintiles and for small-to-medium size quintiles. Consistent with a negative risk premium, betas are increasing in the size dimension and decreasing in the value dimension. In summary, the RUC helps explain both the size and value premium to a very large extent.[22]

Since multivariate and univariate betas have a one-to-one correspondence via a linear relationship through the covariance matrix of factors, the risk premia, γ^{first} and $\gamma^{\text{rev},1}$, can be obtained directly from the main results in Table 2. The resulting annualized risk premia for (first release) consumption growth risk totals 5.73% per unit risk exposure, of which first release risk only is small, equalling $\gamma^{\text{first}} = 0.32\%$. The risk premia for revision uncertainty risk is large, $\gamma^{\text{rev},1} = -5.41\%$. To illustrate the implication of this, note that the value premium for small stocks equals 10.07% out of which almost two-thirds, $\gamma^{\text{rev},1}(\beta_{11}^{\text{rev},1} - \beta_{15}^{\text{rev},1}) = 6.44\%$, is due to the difference in exposure to RUC risk. First release risk captures 0.42% points of the total value premium. Together with the cross-sectional dispersion in betas, this elaborates on our understanding of the substantial and significant increase in pricing performance for the Revised CCAPM versus the First CCAPM. It is primarily driven by investors' large compensation for the uncertainty associated with immediate consumption growth, captured by revision uncertainty, which is the main focus of the following section. For comparison, the risk premia for final or first release risk without the presence of revision uncertainty risk are relatively small at 1.29% and 2.95%, respectively. This lends strong support for the pricing of revision uncertainty. However, more than doubling the consumption growth risk premia simply by replacing the final release with the first release strongly supports our arguments above that the first release is a more suitable consumption growth risk factor than the final realease.

As shown in Section II the Revised CCAPM implies a conditional one-factor representation with the first release as risk factor and the absolute value of revisions as

[22] In the Appendix we depict the relation between realized average returns and first release and RUC betas. The figures show a clear linear relationship in both cases, with average excess return increasing in first release betas and decreasing in revision component betas.

conditioning information. This decomposition allows us to get a better understanding of the effect of revision uncertainty on cross-sectional factor exposure dispersion and thereby on risk premia. We start by rewriting the exposure spread between corner portfolios, i.e. small-big size and high-low book-to-market quintiles, denoted by i and j, as

$$\tilde{\beta}_{i,t}^{\text{first}} - \tilde{\beta}_{j,t}^{\text{first}} = \beta_i^{\text{first}} - \beta_j^{\text{first}} + (\beta_i^{\text{rev},k} - \beta_j^{\text{rev},k})|\nu_{t|t+k}|. \tag{18}$$

We then note that we empirically find that the first element of this decomposition (Panel C), $\beta_i^{\text{first}} - \beta_j^{\text{first}}$, is always positive while the second element (Panel D), $\beta_i^{\text{rev},k} - \beta_j^{\text{rev},k}$, is always negative. Since $|\nu_{t|t+k}| \geq 0$, revision uncertainty has a dampening effect on the spread between conditional exposures. In the absence of any revision uncertainty, $|\nu_{t|t+k}| = 0$, consumption exposure spreads are given by first release betas, but these differences become smaller the larger the revision uncertainty at time t. Overall, it appears that revision uncertainty has the effect of reducing the difference in consumption exposure across firms with different characteristics. Focusing on the value dimension where the dampening effect is largest, we note that this mechanism can be related to the impact of uncertainty on the value of investment opportunities and associated capital adjustment costs. Nishimura and Ozaki (2007) show theoretically that uncertainty about the economy reduces the value of investment opportunities and associated capital expenditure. These effects are confirmed empirically in Neamtiu, Shroff, White, and Williams (2014). This leads to a larger amount of capital adjustments for growth firms relative to value firms. Since good states of the economy occur more often and last longer than bad states, adjustment costs for growth firms are likely on par or even greater than that for value firms unconditionally (Zhang, 2005). Given those higher adjustment costs, the flexibility in growth firms are weakened in uncertain times which causes, in response, their returns to be more dependent on the current state. That is, it increases their exposure to consumption more relative to value firms. The story is consistent with the fact that factor exposures to the revision uncertainty component (Panel D) for high book-to-market firms, in particular within small-to-medium size quintiles, are close to zero, as opposed to large values for low book-to-market ratio quintiles.

E. Consumption growth shocks, risk, or ambiguity?

In this section, we examine the implications of our asset pricing results for the understanding of the risk associated with consumption growth revisions. We entertain three plausible interpretations. First, revisions may represent shocks to consumption growth which enter the SDF directly, in accordance with the logic in the Standard CCAPM. If this is the case, positive revisions (surprises) are associated with high marginal utility states. Secondly, revisions may capture risk or, thirdly, ambiguity surrounding immediate consumption growth. The distinction between risk and ambiguity is important and we will examine this further below. While the expected sign of the SDF loading on risk and ambiguity is not entirely clear, one might be tempted to associate the magnitude of revisions in those cases with low marginal utility states and, thus, a negative SDF loading.[23] We investigate this further in the following sections.

E.1. Consumption growth shocks?

To test whether revisions are perceived by investors as if they represent consumption growth shocks, we derive a version of the Standard CCAPM that incorporates the decomposition in (8) in the following Proposition.[24]

Proposition 1 (Standard CCAPM with revisions). *Under regular assumptions of the Standard CCAPM, the decomposition in* (8) *implies that the model admits a beta-representation of the form*

$$\mathbb{E}[r_{i,t+1}] = \beta_i^{\text{first}} \gamma^{\text{first}} + \beta_i^{\text{rev},k} \gamma^{\text{rev},k}, \tag{19}$$

where

$$\beta_i^{\text{first}} = \frac{\text{cov}[r_{i,t+1}, c_{t|t+1}]}{\text{var}[c_{t|t+1}]}, \quad \beta_i^{\text{rev},k} = \frac{\text{cov}[r_{i,t+1}, v_{t+1|t+1+k}]}{\text{var}[v_{t+1|t+1+k}]}, \tag{20}$$

and

$$\gamma^{\text{first}} = \gamma^c \eta^{\text{first}}, \quad \gamma^{\text{rev},k} = \gamma^c \eta^{\text{rev},k}, \tag{21}$$

with variance ratios $\eta^{\text{first}} = \text{var}[c_{t|t+1}]/\text{var}[c_{t+1|T}]$ *and* $\eta^{\text{rev},k} = \text{var}[v_{t+1|t+1+k}]/\text{var}[c_{t+1|T}]$.

[23]For example, Boguth and Kuehn (2013) find that conditional consumption growth volatility demands a negative price, whereas Dew-Becker et al. (2019) find that increasing ambiguity is linked to high marginal utility states but realized volatility, measuring risk, is linked to low marginal utility states. Segal, Shaliastovich, and Yaron (2015) find that risk enters the SDF positively in good states but negatively in bad states.

[24]The derivations behinds the Proposition can be found in the Appendix.

This result can be generalized to hold for any macroeconomic-based factor model that assumes $c_{t+1} = g(f_{t+1})$ for both linear and non-linear functions $g(\cdot)$. By corollary of Proposition 1, the associated representation using SDF loadings reads

$$\mathbb{E}[r_{i,t+1}] = \lambda^{\text{first}}\text{cov}[c_{t+1|t+1}, r_{i,t+1}] + \lambda^{\text{rev},k}\text{cov}[\nu_{t+1|t+1+k}, r_{i,t+1}]. \tag{22}$$

Note that $\lambda^{\text{rev},k}$ (and $\gamma^{\text{rev},k}$) is different from the the one in Revised CCAPM as it relates to the covariance of returns with revisions in levels and not the RUC, φ^{k}_{t+1}. If $k = T$, we get a straightforward decomposition of the final Standard CCAPM that is typically employed in the literature. In the Standard CCAPM, $\gamma^{c} \geq 0$. Since $\eta^{i} \geq 0$, $i \in \{\text{first}, \text{rev}\}$ it follows that $\gamma^{\text{first}}, \lambda^{\text{rev},k} \geq 0$. That is, according to Proposition 1, if the Standard CCAPM were to capture revisions, they should carry a positive price. The logic is similar to the conventional logic in that positive shocks to immediate consumption growth, i.e. positive revisions to consumption growth, are associated with good states with high marginal utility. Any positive covariance between asset i's return and revisions are considered risky and would demand a positive compensation. We estimate this model following similar procedures as in Section A and report the results for $k = 1, 2, 4, 12, T$ in Table 7. The results show that there is indeed information in revisions in levels, showing a considerable improvement relative to the Standard CCAPM and First CCAPM. However, the improvement is smaller when compared to the Revised CCAPM and it is only significant on the 5% level for $k = 1$. The decrease in pricing performance when increasing the revision horizon is present here as well. An important finding is that revisions in levels enter the SDF negatively. This is inconsistent with the Standard CCAPM using revisions as in Proposition 1. As such, we conclude that revisions may not be interpreted as simply consumption growth shocks and the improvement seen in this model require a different explanation. We examine this in the following section.

E.2. Consumption growth risk or ambiguity?

If we instead consider the absolute value of revisions, the implication is that investors do not necessarily care about the direction of revisions but rather its magnitude. This may be consistent with the idea of risk or ambiguity. Although the two terms are often used interchangeably with ambiguity typically mentioned as uncertainty, the distinction between risk and ambiguity is both behaviorally, empirically, and theoretically important. Risk refers to situations where the distribution of random outcomes, e.g. consumption growth, is known to the decision maker, while uncertainty or ambiguity (sometimes called Knightian uncertainty (Knight, 1921)) refers to the situation where the decision maker is uncertain about the distribution of these outcomes due to cognitive or informational constraints.[25] The distinction is particularly important in financial markets since risk and ambiguity can have completely opposing impacts on aspects that span from asset pricing (Epstein and Schneider, 2010; Guidolin and Rinaldi, 2013; Brenner and Izhakian, 2018) to payout policy (Dahya, Herron, and Izhakian, 2019) and even capital structure of the firm (Izhakian, Yermack, and Zender, 2019). Aversion to both risk and ambiguity has been documented in the literature, dating back to e.g. Keynes (1937) and Ellsberg (1961) and recently in Dew-Becker et al. (2019).[26]

Table 8 reports the results estimating the following model

$$\mathbb{E}[r_{i,t+1}] = \lambda^{\text{first}}\text{cov}[c_{t+1|t+1}, r_{i,t+1}] + \lambda^{\text{rev},k}\text{cov}[|v_{t+1|t+1+k}|, r_{i,t+1}], \quad (23)$$

using absolute revisions instead of levels. We refer to this model as |Standard CCAPM|. The performance is solid though not as impressive as including revisions in levels nor our proposed Revised CCAPM. Yet, the SDF loading is negative and significant at 10% for $k = 1$. The natural question arises; how come the Revised CCAPM provides so large improvements, simply by interacting absolute revision with the first release, when using the absolute revision alone does not? To guide our answer, we examine the relationship between absolute revisions and natural proxies classified as either risk or ambiguity.

Our first proxy for risk is adopted from Anderson et al. (2009) who empirically distinguish between (asset return) risk and uncertainty. We follow their approach and identify consumption growth risk as its conditional volatility. As such, we estimate a GARCH(1,1) model similarly to Tédongap (2015), on the first release series,[27] and

[25] In our context, risk refers to the situation where future consumption growth is random and not known today, yet comes from a known probability distribution. Ambiguity, on the other hand, refers to the situation where the probability distribution is also uncertain.

[26] Ellsberg's paradox, often taken to be evidence for ambiguity aversion, builds upon the idea that individuals prefer taking on risks with known probabilities of being better or worse off as opposed to risks where the probabilities are uncertain.

[27] Results are similar, however, using the final consumption growth series.

use the fitted values $\hat{\sigma}_{t+1}^{\text{GARCH}}$ as our first proxy for consumption growth risk. Our second measure of risk is the conditional volatility estimate from a markov-switching model from Boguth and Kuehn (2013) $\hat{\sigma}_{t+1}^{\text{MS}}$. Our final risk measure is inspired by Dew-Becker et al. (2019), who posit that a better measure for risk is realized volatility. To obtain such a measure in our context, we construct a monthly vintage data set of real nondurable consumption growth similarly to our quarterly data set obtained from ALFRED. We then compute quarterly realized volatility as per

$$RV_{t+1} = \sum_{j=1}^{3} (c_{t+i/3|t+i/3} - \bar{c})^2, \tag{24}$$

where $i = 1,2,3$ represents the first, second, and third month within the quarter $t+1$, $c_{t+i/3|t+i/3}$ the first release pertaining to month i, and $\bar{c}$ the average monthly logarithmic growth rate.[28] Our first ambiguity proxy follows Anderson et al. (2009), using the dispersion in individual forecasts' on consumption growth from the Survey of Professional Forecasters (SPF) provided by the Federal Reserve Bank of Philadelphia.[29] We obtain quarterly real consumption growth rate forecasts for the current quarter in which the survey is conducted. That is, we use the nowcast by all individual forecasters. We then construct our first ambiguity proxy as the cross-sectional standard deviation, consistent with e.g. Anderson et al. (2009) and Drechsler (2013), of those individual forecasts at all time points as per

$$SPF_{t+1} = \left(\frac{1}{P_{t+1} - 1} \sum_{p=1}^{P_{t+1}} \left(\hat{c}_{t+1,p} - \frac{1}{P_{t+1}} \sum_{p=1}^{P_{t+1}} \hat{c}_{t+1,p} \right)^2 \right)^{1/2}, \tag{25}$$

where P_{t+1} is the number of provided forecasts at time $t+1$ and $\hat{c}_{t+1,p}$ is the consumption growth forecast made by forecaster ID denoted p.[30] Our second ambiguity proxy follows e.g. Bloom (2009) and Williams (2014) defined as the implied volatility, IV_{t+1}, on a hypothetical at-the-money option on the S&P 100 index which is consistent with

[28]This definition is similar to concept of realized volatility used in the high-frequency literature, see e.g. Andersen, Bollerslev, Diebold, and Labys (2001, 2003)

[29]Various papers use dispersion in professional forecasters as proxy for ambiguity. A few references are Ulrich (2013), Drechsler (2013), Rossi and Sekhposyan (2015), and Lee et al. (2019). Moreover, Della Corte and Krecetovs (2019) show formally how forecast dispersion can be seen as a natural proxy for uncertainty and Patton and Timmermann (2010) contend that forecast dispersion among professional forecasters can only reflect heterogeneity in models and not in information sets.

[30]We have also used the difference between the 75'th and 25'th percentile of the forecast distribution consistent with the definition of forecast dispersion by Federal Reserve Bank of Philadelphia. Conclusions are unaltered.

the use of implied volatility in Dew-Becker et al. (2019) and Berger, Dew-Becker, and Giglio (2019a). We use the VXO, as opposed to the VIX option-implied volatility, because the former is available from 1986 and the latter from 1990. The VXO and VIX are, however, almost perfectly correlated. We note that this variable is not explicitly formulated with a relation to consumption growth, as opposed to the remaining variables. Data availability limits the starting point of our analysis to 1982:Q1 using SPF data and 1986:Q3 using VXO data, and the end point to 2009:Q3 when using the markov-switching conditional volatility of Boguth and Kuehn (2013).

To examine the relationship between revision uncertainty, $|v_{t+1|t+1+k}|$, and our risk and ambiguity proxies, Table 9 reports the results of regressing absolute revisions onto various combinations of our proxies. We standardize all variables in the interest of exposition. That is, let $RISK_{t+1} = (\hat{\sigma}^{\text{GARCH}}_{t+1}, RV_{t+1}, \hat{\sigma}^{\text{MS}}_{t+1})'$ and $AMB_{t+1} = (SPF_{t+1}, IV_{t+1})'$. We consider the following regressions

$$|v_{t+1|t+1+k}| = \theta RISK_{t+1} + \psi AMB_{t+1} + u_{t+1}, \tag{26}$$

for various permutations of $RISK_{t+1}$ and AMB_{t+1}, $k = 1$, and u_{t+1} is an error term. The results are striking. Across all combinations, revisions are unrelated to our risk proxies except in one specification. When it comes to ambiguity we find that RUC is significantly related to both our ambiguity proxies. This relation is particularly strong when using SPF cross-sectional dispersion, showing t-statistics using HAC standard errors well above five in most cases. The sign of the ambiguity coefficients are always positive and large in magnitude compared to mixed signs of the risk coefficients and relatively small magnitudes.[31] In Figure 4 we depict $|v_{t+1|t+1+k}|$, $k = 1$, together with SPF_{t+1}. The figure echoes our findings above, showing a striking similarity in the dynamics of absolute revisions and SPF dispersion both within and outside crisis periods.[32]

[31] We have also implemented the regressions in (26) with the newspaper-based economic policy uncertainty (EPU) index of Baker et al. (2016) and the credit spread (CS) (Bekaert, Engstrom, and Xu, 2019) measured as the difference in BAA and AAA-rated corporate bond yields in place of the ambiguity proxies used in the main results. Conclusion are identical, with EPU and CS statistically significant on generally 1% level and with positive and large coefficients. The risk proxies remain insignificant.

[32] Replacing $v_{t+1|t+1+k}, k = 1$ by the SPF ambiguity proxy in the Revised CCAPM leads to an adjusted cross-sectional R^2 on the 25 size and book-to-market ratio sorted portfolios of 56% and statistically significant λs on 10% and 5% level for first release and revision component, respectively.

E.3. Implications for interpretation of the Revised CCAPM

Our evidence above suggests that absolute revisions are strongly related to the ambiguity surrounding consumption growth. Our asset pricing results indicate that when absolute revisions are interacted with first release, as in the Revised CCAPM, the pricing ability is striking. It also strongly outperforms a model that uses the absolute revisions only. We understand these results in the context of *state-dependent ambiguity attitudes*. Recent psychological experiments show that ambiguity aversion exists after having seen a particular data sample (Smithson et al., 2019). This means that investors might be uncertain about the underlying distribution of consumption even if they have observed all past releases. A recent contribution by Brenner and Izhakian (2018) finds evidence of *state-dependent* ambiguity attitudes in the relation to stock market returns.[33] Ambiguity aversion increases with increasing probability of a favourable outcome, in this case a return, and vice versa. The logic is that with a high probability of a favourable outcome, ambiguity aversion is high as investors prefer the certainty around the outcome. On the other hand, with a high probability of an unfavourable outcome, ambiguity aversion is low as investors like that this is in fact uncertain.

To provide some further intuition, we can revisit our results in Section II. With a state of the economy determined by $c_{t+1|t+1}$ and $|v_{t+1|t+1+k}|$, $k = 1$, measuring ambiguity, the interaction between those, i.e. φ^k_{t+1}, captures directly the state-dependency of ambiguity. Our asset pricing results conclude that the compensation for positive covariance risk with this RUC is negative. That is, investors perceive high values of φ^k_{t+1} as low marginal utility and are willing to pay a premium for an insurance, that is, pay a premium for a positive covariance, $\text{cov}[r_{i,t+1}, \varphi^k_{t+1}]$. On the other hand, investors dislike low or negative covariances and require compensation for holding assets with those characteristics.

Suppose now that investors care about whether their consumption is going up or down and classify states as positive and negative on this basis. That is, $c_{t+1|t+1} > 0$ defines a good state and vice versa. Conditional on being in a good state, φ^k_{t+1}

[33] There is also strong evidence from the behavioral and experimental economics literature that attitudes towards ambiguity are state dependent, with ambiguity aversion for positive states (high probability of gain) and ambiguity seeking for negative states (high probability of a loss), see, e.g., Mangelsdorff and Weber (1994); Di Mauro and Maffioletti (1996); Du and Budescu (2005); Chakravarty and Roy (2009); Kothiyal (2012)

increases if $|v_{t+1|t+k}|$ increases. Conditional on being in a bad state, φ^k_{t+1} increases if $|v_{t+1|t+k}|$ decreases. Both cases are disliked by an investor with state-dependent ambiguity attitudes. The investor is averse to increasing uncertainty in good states and decreasing uncertainty in bad states, which is jointly captured by increases in φ^k_{t+1}. As noted above, the investor likes assets that pay returns in periods with increasing φ^k_{t+1} as it provides a hedge against unfavourable ambiguity, which explains the negative SDF loading and risk premium.

Note, however, that this interpretation and our baseline results above rely on the implicit assumption that investors classify states according to a reference point equal to zero. In other words, positive consumption growth is a positive state and vice versa. This could naturally be obtained simply by de-meaning the first release before entering the model, which would not change any results if it entered linearly. However, the first release enters non-linearly through φ^k_{t+1}, which does not guarantee that de-meaning of the first release to ensure a zero reference point has no implication for results. To address this potential concern, one may consider the general situation where investors classify states according to some generic reference point $\tilde{c}_{t+1}$. This reference point can be fixed or time varying, possibly driven by habit formation (Campbell and Cochrane, 1999; Atanasov, Møller, and Priestley, 2019) or certainty equivalents (Delikouras and Kostakis, 2019). We investigate this case in the Appendix and find that all conclusions are insensitive to choosing among a large set of plausible candidates for a fixed or time-varying reference point, e.g. the unconditional mean, the time varying certainty equivalent of first release consumption growth, cyclical consumption growth, or using either equally- and exponentially-weighted moving averages over the past 1-10 years. The SDF loadings always have the same sign and similar statistical significance as in our baseline results that implicitly set $\tilde{c}_{t+1} = 0$. The adjusted cross-sectional R^2 remains high. If anything, our baseline results are slightly conservative. We may therefore conclude that investors make portfolio decisions and evaluate financial assets as if they assess their aversion to ambiguity, depending on the state of consumption growth.

As a remark, we may also use this interpretation to understand the negative price obtained on revisions in levels, which was not reconcilable with the Standard CCAPM and Proposition 1. To see this, note that for any time series $y_t, t = 1, \dots, T$, it holds that $y_t = \text{sign}(y_t)|y_t|$. It was clear that from the summary statistics in Table 1 that revisions are strongly business cycle dependent, with its sign capturing the states in consumption growth. Using revisions in levels is, thus, identical to using the interaction between the sign of revisions and its absolute value, $\text{sign}(v_{t+1|t+1+k}) \cdot |v_{t+1|t+1+k}|$, that is, to the interaction between a proxy of the state of consumption growth and its surrounding ambiguity. In other words, revisions in levels may capture similar features as the RUC, φ^k_{t+1}, above, and rationalizes the negative SDF loading obtained in Table 7.

As a final remark, this interpretation suggests that absolute revisions, i.e. ambiguity, is perceived differently by investors according to states. Simply using absolute revisions as in |Standard CCAPM| above does not capture this state dependence implying a cancelling out effect unconditionally, causing the pricing performance decrease relative to our Revised CCAPM. Since good states, where ambiguity is disliked and positive covariances between returns and absolute revisions preferred, occur more frequently and last longer, they dominate unconditionally causing the SDF loading in Table 8 to be negative.

IV. Large panel of characteristics-sorted portfolios

To ensure that our results are not driven by the choice of test assets, and to alleviate the critique of Lewellen et al. (2010), we conduct an analysis on a large panel of characteristics-sorted portfolios in a spirit similarly to Giglio and Xiu (2018). We gather 202 portfolios over the 1965-2015 period, comprised by 17 industry portfolios, 25 portfolios sorted by size and book-to-market ratio, 25 portfolios sorted by operating profitability and investment, 25 portfolios sorted by size and variance, 25 portfolios sorted by size and momentum, 35 portfolios sorted by size and net issuance, 25 portfolios sorted by size and accruals, and 25 portfolios sorted by size and stock market beta. Portfolio returns are obtained from Kenneth French's library. This set of portfolios captures a vast cross-section of anomalies and exposures to various factors. We then randomly sample, without replacement, 25 portfolios and re-estimate the Standard CCAPM, First CCAPM, Revised CCAPM and benchmark models on each sample.[34] Repeating this exercise 10,000 times obtains a distribution of SDF loadings, λs, their t-statistics, and the adjusted cross-sectional R^2 across the subsamples. We are specifically interested in, firstly, whether the sign and significance of the SDF

[34]Conclusions are similar by resampling 50 and 101 portfolios.

loadings obtained in the Revised CCAPM on the 25 portfolios sorted on size and book-to-market ratio generalize to this large, comprehensive set of assets and, secondly, whether the strikingly strong pricing ability of the Revised CCAPM compared to its consumption-based benchmarks remains.

Figure 5 depicts the distribution of SDF loadings and associated t-statistics when estimating the Revised CCAPM on each subsample. It is clear that the the SDF loading of first release risk is positive and the loading on RUC risk is negative in almost all subsamples. In fact, this happens in 99.2% (98.5%) of the cases, respectively.[35] These findings are, as such, consistent with the findings in Table 2. In the vast majority of the cases the SDF loading is also statistically significant on conventional levels for both factors. In 67.1% of the cases the first release risk is significant on a 5% level, whereas in 62.5% of the cases the RUC risk is statistically significant on the same significance level. We therefore conclude that the sign and significance of the SDF loadings in the Revised CCAPM generalize to this large cross-section of portfolio returns and, as such, the economic intuition presented in Section E is valid in general across stocks.

Table 10 reports the average adjusted cross-sectional R^2 across all subsamples for each model under consideration in addition to the proportion of the subsamples where the Revised CCAPM generates a greater R^2 than its comparing models. Overall, the Revised CCAPM performs well in this expanded set of portfolios, outperforming the Standard CCAPM, First CCAPM, Q4-Q4 CCAPM and Cay CCAPM in most subsamples while performing approximately on par with the Ultimate CCAPM.

V. Concluding remarks

This paper provides an analysis of the implications of the consumption data release process, as reported in vintage NIPA tables, for asset pricing. We propose a new consumption-based asset pricing model, the Revised CCAPM, which captures this data release process explicitly using vintage data. Our empirical results conclude that first data releases are more suitable than final releases for asset pricing. This is due to the former avoiding revisions that serve to mitigate measurement errors, providing an explanation as to why unfiltered consumption (Kroencke, 2017), garbage (Savov,

[35] The associated risk premia share a similar conclusion, with 77.4% and 76.6% of the subsambles yielding a positive and negative sign for the first release and RUC risk, respectively.

2011), and electricity (Da and Yun, 2010) are successful in asset pricing. Moreover, the pricing ability of the Revised CCAPM is striking when modelling the uncertainty captured by early revisions. We find that absolute revisions are strongly related to consumption growth ambiguity but neither to consumption growth shocks nor to risk. Our results can be understood in the context of state-dependent ambiguity attitudes. In good consumption states, ambiguity is disliked, but in bad states it is preferred. Investors are willing to pay a large premium to insure against state-dependent ambiguity risk, constituting the vast majority of total risk premium on consumption growth.

The implications of our findings are wide. They suggest that, based on a relation to Kroencke (2017), that early consumption data releases are more suitable for asset pricing, since they avoid a filtering process in revisions occuring after the most early ones. It also raises the question whether the use of first versus final releases has an impact on other consumption-based asset pricing models' pricing ability, as well as the incorporation of the important uncertainty or ambiguity coming from early revisions. Whether our findings extent to general macro-based factor models as in, e.g., Chen et al. (1986) is another interesting question. A related aspect is whether absolute revisions from other macroeconomic series are as strongly linked to ambiguity as we document in this present paper. Moreover, capturing time-varying and in particular state-dependent ambiguity attitudes in a consumption-based structural framework is a further natural step for future research, possibly extending the model in Ju and Miao (2012). We expect to pursue several of these questions in future work.

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Table 1: Summary statistics of consumption growth vintages
This table reports the summary statistics of real time data, revisions and final data for real nondurable consumption growth for the period 1965:Q1 to 2018Q4. The three panels cover, respectively, the full sample, NBER expansions, and recessions. The final revised data is labeled $c_{t|T}$, first release is labeled $c_{t|t}$ and revisions released k periods after first release are labeled $v_{t|t+k}$. The column labeled SD denotes the standard deviation, NS denotes the noise to signal ratio, var[$v_{t|t+k}$]/var[$c_{t|T}$], and AC(1) the first-order autocorrelation. Superscripts ***, **, and *, used only in the first column on means of revisions, correspond to statistical significant at significance levels one, five, and ten percent, respectively, using HAC standard errors that are robust to heteroskedasticity and autocorrelation by Newey and West (1987) with a Bartlett kernel and data-driven lag selection based on Andrews (1991).

							Correlations	
Variable	Mean	SD	NS	Min	Max	AC(1)	$c_{t\|T}$	$c_{t\|t}$
				Panel A: 1965:Q1-2018:Q2				
$c_{t\|T}$	2.38	2.73	-	-5.88	13.63	0.23	-	0.69
$c_{t\|t}$	2.16	3.26	1.19	-7.39	10.20	0.07	0.69	-
$v^{rev}_{t\|t+1}$	0.12	1.17	0.43	-4.27	4.14	0.06	0.21	-0.06
$v^{rev}_{t\|t+2}$	0.10	1.24	0.45	-4.27	4.01	0.01	0.16	-0.17
$v^{rev}_{t\|t+4}$	0.20*	1.48	0.54	-4.39	4.33	0.02	0.07	-0.28
$v^{rev}_{t\|t+12}$	0.18	1.92	0.70	-4.48	5.49	-0.07	0.08	-0.53
$v^{rev}_{t\|T}$	0.22	2.42	0.88	-10.17	7.21	0.00	0.21	-0.57
				Panel A: 1965:Q1-2018:Q2, NBER expansions				
$c_{t\|T}$	2.89	2.40	-	-3.52	13.63	0.04	-	0.61
$c_{t\|t}$	2.55	3.07	1.28	-6.25	10.20	-0.00	0.61	-
$v^{rev}_{t\|t+1}$	0.22***	1.13	0.47	-2.91	4.14	0.01	0.08	-0.21
$v^{rev}_{t\|t+2}$	0.17**	1.19	0.50	-3.79	4.01	-0.03	0.10	-0.25
$v^{rev}_{t\|t+4}$	0.27**	1.43	0.59	-4.39	4.33	-0.06	0.06	-0.32
$v^{rev}_{t\|t+12}$	0.28*	1.97	0.82	-4.48	5.49	-0.10	0.05	-0.62
$v^{rev}_{t\|T}$	0.34*	2.49	1.04	-10.17	7.21	0.01	0.22	-0.65
				Panel B: 1965:Q1-2018:Q2, NBER recessions				
$c_{t\|T}$	-0.33	2.81	-	-5.88	4.07	0.23	-	0.84
$c_{t\|t}$	0.09	3.50	1.24	-7.39	6.36	0.07	0.84	-
$v^{rev}_{t\|t+1}$	-0.42*	1.24	0.44	-4.27	1.54	0.15	0.36	0.24
$v^{rev}_{t\|t+2}$	-0.29	1.42	0.51	-4.27	2.65	0.15	0.14	-0.10
$v^{rev}_{t\|t+4}$	-0.17	1.71	0.61	-4.27	2.65	0.33	-0.09	-0.38
$v^{rev}_{t\|t+12}$	-0.32	1.60	0.57	-2.32	3.85	0.13	-0.08	-0.44
$v^{rev}_{t\|T}$	-0.42	1.90	0.68	-4.97	3.55	-0.14	-0.07	-0.60

Table 2: Estimation of asset pricing models

This table reports the estimated SDF loadings (λs), adjusted cross-sectional R^2 and mean absolute pricing errors (MAPE) for the Standard CCAPM, First CCAPM, and Revised CCAPM using $k = 1, 2, 4, 12, T$ and the benchmark models described in Section III.B. The test assets are the 25 size and book-to-market ratio sorted portfolios. Standard errors of the SDF loadings are reported in parenthesis below each estimate, using GMM standard errors that are robust to errors-in-variables as well as to heteroskedasticity and autocorrelation by Newey and West (1987) with a Bartlett kernel and data-driven lag selection based on Andrews (1991).

Model	λ^{final}	λ^{first}	$\lambda^{\text{rev},k}$	$R^2(\%)$	MAPE(%)
Standard CCAPM	0.17			8.63	2.05
	(1.05)				
First CCAPM		0.27		18.50	1.92
(No revisions)		(1.54)			
Revised CCAPM		0.73	-0.68	73.82	1.07
($k = 1$)		(3.55)	(-3.38)		
Revised CCAPM		0.56	-0.44	50.02	1.50
($k = 2$)		(2.98)	(-2.75)		
Revised CCAPM		0.54	-0.28	40.44	1.67
($k = 4$)		(2.71)	(-2.10)		
Revised CCAPM		-0.15	0.20	27.10	1.74
($k = 12$)		(-0.77)	(2.58)		
Revised CCAPM		-0.21	0.11	25.94	1.65
($k = T$)		(-1.17)	(3.01)		
Ultimate CCAPM				56.89	1.30
Q4-Q4 CCAPM				8.24	1.99
Cay CCAPM				46.55	1.47
Fama-French				69.75	1.01

Table 3: Predictability of revisions

This table reports in-sample and out-of-sample results for predictive regressions at revision horizons $k = 1, 2, 4, 12, T$. Panel A reports results using the first release only as predictor, whereas Panel B reports the result most in favour of predictability across all models that uses in addition a single exogenous predictor. That is, the minimum Wald p-value, maximum R^2, maximum R^2_{OoS}, and minimum Diebold-Mariano p-value. Those predictors are presented in the Appendix along with all underlying results. The left panel shows p-values associated with a Wald test of $\mathbb{H}_0 : a = 0, b = 0$ in Panel A and $\mathbb{H}_0 : a = 0, b = 0, d = 0$ in Panel B as well as the R^2 from the in-sample analysis. The right panel shows R^2_{OoS} and p-values associated with a Diebold-Mariano (DM) test of equal predictive ability between the predictive model and the benchmark model employing $\nu_{t|t+k} = \varepsilon_{t|t+k}$. We use HAC covariance matrices that are robust to heteroskedasticity and autocorrelation by Newey and West (1987) with a Bartlett kernel and data-driven lag selection based on Andrews (1991). For the out-of-sample analysis, we use an expanding window scheme with an initial window of 15 years (60 time periods), and generate the first forecasts for 1980Q1. Bold values indicate rejection of 5% significance level (without possible correction for multiple testing).

	In-sample		Out-of-sample	
Revision horizon	Wald p-value	$R^2(\%)$	$R^2_{OoS}(\%)$	DM p-value
Panel A: Using first release only ($d = 0$)				
$k = 1$	0.389	0.35	-1.07	0.644
$k = 2$	0.069	2.92	3.63	0.034
$k = 4$	0.000	7.98	12.18	0.010
$k = 12$	0.000	27.61	32.75	0.049
$k = T$	0.000	32.51	-	-
Panel B: Strongest predictability among all predictors				
$k = 1$	0.071	0.35	1.11	0.359
$k = 2$	0.002	6.81	4.65	0.014
$k = 4$	0.000	11.19	15.01	0.003
$k = 12$	0.000	34.11	38.91	0.029
$k = T$	0.000	41.03	-	-

Table 4: Wald tests for univariate beta identification

This table reports Wald test and associated p-values regarding joint significance and cross-sectional dispersion of Revised CCAPM univariate betas on the 25 portfolios sorted on size and book-to-market ratio as test assets. The table uses univariate betas from the first-stage Fama-MacBeth estimation in Section III and forms test statistics using a HAC covariance matrix robust to heteroskedasticity and autocorrelation by Newey and West (1987) with a Bartlett kernel and four lags. Based on these estimates, we conduct the following five Wald tests: i) whether the 25 portfolio betas are jointly equal to 0 ($\mathbb{H}_0 : \beta_i = 0\ \forall i$), ii) whether the 25 portfolio betas are jointly equal to one another ($\mathbb{H}_0 : \beta_i = \beta_j\ \forall i, j, i \neq j$), iii) whether the 25 portfolio betas are jointly equal to the average beta, $\bar{\beta}$, ($\mathbb{H}_0 : \beta_i = \bar{\beta}\ \forall i$), iv) whether the maximum portfolio beta is less than or equal to the minimum portfolio beta ($\mathbb{H}_0 : \beta_{\max} \leq \beta_{\min}$), and v) whether the beta of the portfolio with highest average excess return, $\beta_{\max(r)}$, (the small value portfolio in this sample) is less than or equal to the beta of the portfolio with smallest average excess return, $\beta_{\min(r)}$, (the small growth portfolio in this sample) ($\mathbb{H}_0 : \beta_{\max(r)} \leq \beta_{\min(r)}$).

	Null hypothesis				
	$\beta_i = 0\ \forall i$	$\beta_i = \beta_j\ \forall i, j, i \neq j$	$\beta_i = \bar{\beta}\ \forall i$	$\beta_{\max} \leq \beta_{\min}$	$\beta_{\max(r)} \leq \beta_{\min(r)}$
	Panel A: First release				
Wald	45.878	43.057	43.714	42.286	0.073
p-value	0.007	0.010	0.012	0.000	0.394
	Panel B: Revision uncertainty				
Wald	66.763	66.072	71.775	35.018	3.984
p-value	0.000	0.000	0.000	0.000	0.023

Table 5: Revised CCAPM with characteristics

This table reports the estimated SDF loadings (λs), adjusted cross-sectional R^2 and mean absolute pricing errors (MAPE) for the standard CCAPM, First CCAPM and Revised CCAPM (using $k = 1$), when including the time series average of market capitalization and book-to-market ratio on the second stage regression of the Fama and MacBeth (1973) two-pass methodology. The test assets are the 25 size and book-to-market ratio sorted portfolios. Standard errors of the SDF loadings are reported in parenthesis below each estimate, using GMM standard errors that are robust to errors-in-variables as well as to heteroskedasticity and autocorrelation by Newey and West (1987) with a Bartlett kernel and data-driven lag selection based on Andrews (1991).

Model	λ^{final}	λ^{first}	$\lambda^{\text{rev},1}$	λ^{ME}	λ^{BM}	R^2(%)	MAPE(%)
Standard CCAPM	-0.08			-0.77	0.83	71.85	1.11
	(-0.63)			(-2.70)	(2.44)		
First CCAPM		-0.05		-0.70	0.85	70.96	1.15
		(-0.34)		(-2.70)	(2.38)		
Revised CCAPM		0.40	-0.44	-0.37	0.39	79.00	0.92
		(2.25)	(-2.75)	(-1.46)	(1.22)		

Table 6: First stage betas and cross-sectional dispersion
This table shows average real excess return (Panel A) and estimates of first-stage multivariate regression betas of the Standard CCAPM (Panels B) and Revised CCAPM, k = 1, (Panels C and D) across quintiles using 25 size and book-to-market sorted portfolios. The bottom row in each panel shows the difference between returns and betas of the smallest and largest size portfolios for a given book-to-market quintile. Similarly, the rightmost column shows the difference between the highest and lowest book-to-market quintile for a given size quintile.

	Low BM	2	3	4	High BM	High-Low
	Panel A: Average real excess returns (ann. %)					
Small size	4.53	10.79	10.48	13.40	14.60	10.07
2	6.95	10.09	11.32	11.99	12.53	5.58
3	7.12	10.42	9.52	11.26	13.19	6.06
4	8.26	7.76	8.76	10.81	10.60	2.34
Big size	6.01	6.50	6.56	5.98	8.30	2.29
Small-Big	-1.48	4.29	3.91	7.42	6.29	
	Panel B: Final release betas					
Small size	4.05	4.53	3.49	3.60	4.02	-0.03
2	3.64	3.21	3.03	3.20	3.54	-0.10
3	3.91	3.16	2.75	2.76	2.94	-0.97
4	3.25	2.92	2.92	2.60	3.97	0.72
Big size	2.45	2.11	2.49	2.46	2.96	0.51
Small-Big	1.60	2.42	1.00	1.15	1.06	
	Panel C: First release betas					
Small size	1.10	1.52	1.54	1.89	2.43	1.32
2	0.24	0.73	0.67	0.98	1.80	1.56
3	0.23	0.90	0.92	0.98	1.24	1.02
4	0.29	0.86	1.07	0.74	0.76	0.47
Big size	-0.63	-0.05	0.22	0.08	0.84	1.47
Small-Big	1.73	1.57	1.32	1.82	1.58	
	Panel D: Revision uncertainty betas ($k = 1$)					
Small size	1.02	0.77	0.23	0.12	-0.17	-1.19
2	1.16	0.67	0.46	0.64	0.20	-0.96
3	1.11	0.61	0.40	0.31	0.09	-1.02
4	0.97	0.54	0.55	0.45	0.79	-0.17
Big size	1.40	1.08	0.88	1.06	0.61	-0.79
Small-Big	-0.38	-0.32	-0.65	-0.95	-0.78	

Table 7: Standard CCAPM with revisions in levels

This table reports the estimated SDF loadings (λs), adjusted cross-sectional R^2 and mean absolute pricing errors ($MAPE$) for the Standard CCAPM, First CCAPM and modified version of the Standard CCAPM where consumption data is decomposed into a first release and the revision component at different horizons k, as per Proposition 1. The test assets are the 25 size and book-to-market ratio sorted portfolios. Standard errors of the SDF loadings are reported in parenthesis below each estimate, using GMM standard errors that are robust to errors-in-variables as well as to heteroskedasticity and autocorrelation by Newey and West (1987) with a Bartlett kernel and data-driven lag selection based on Andrews (1991).

Model	λ^{final}	λ^{first}	$\lambda^{\text{rev},k}$	R^2(%)	MAPE(%)
Standard CCAPM	0.17			8.63	2.05
	(1.05)				
First CCAPM		0.27		18.50	1.92
(No revisions)		(1.54)			
Standard CCAPM		0.27	-0.63	48.64	1.48
($k=1$)		(1.48)	(-2.12)		
Standard CCAPM		0.30	-0.48	37.28	1.59
($k=2$)		(1.71)	(-1.73)		
Standard CCAPM		0.26	-0.51	40.33	1.58
($k=4$)		(1.49)	(-1.80)		
Standard CCAPM		0.25	-0.25	18.29	1.87
($k=12$)		(1.34)	(-0.88)		
Standard CCAPM		0.26	-0.10	16.05	1.89
($k=T$)		(1.46)	(-0.55)		

Table 8: Standard CCAPM with absolute revisions

This table reports the estimated SDF loadings (λs), adjusted cross-sectional R^2 and mean absolute pricing errors ($MAPE$) for the Standard CCAPM, First CCAPM and modified version of the Standard CCAPM, |Standard CCAPM|, where risk factors are the first release data and the absolute value of revisions at different horizons k. The test assets are the 25 size and book-to-market ratio sorted portfolios. Standard errors of the SDF loadings are reported in parenthesis below each estimate, using GMM standard errors that are robust to errors-in-variables as well as to heteroskedasticity and autocorrelation by Newey and West (1987) with a Bartlett kernel and data-driven lag selection based on Andrews (1991).

Model	λ^{final}	λ^{first}	$\lambda^{\text{rev},k}$	R^2(%)	MAPE(%)
Standard CCAPM	0.17			8.63	2.05
	(1.05)				
First CCAPM		0.27		18.50	1.92
(No revisions)		(1.54)			
| Standard CCAPM |		0.31	-1.15	30.06	1.70
($k = 1$)		(1.77)	(-1.89)		
| Standard CCAPM |		0.30	-0.94	25.95	1.75
($k = 2$)		(1.73)	(-1.83)		
| Standard CCAPM |		0.28	-0.67	34.08	1.68
($k = 4$)		(1.64)	(-1.69)		
| Standard CCAPM |		0.36	0.71	30.47	1.60
($k = 12$)		(1.95)	(2.10)		
| Standard CCAPM |		-0.05	0.61	35.48	1.45
($k = T$)		(-0.39)	(2.62)		

Table 9: Revision uncertainty as risk or ambiguity?

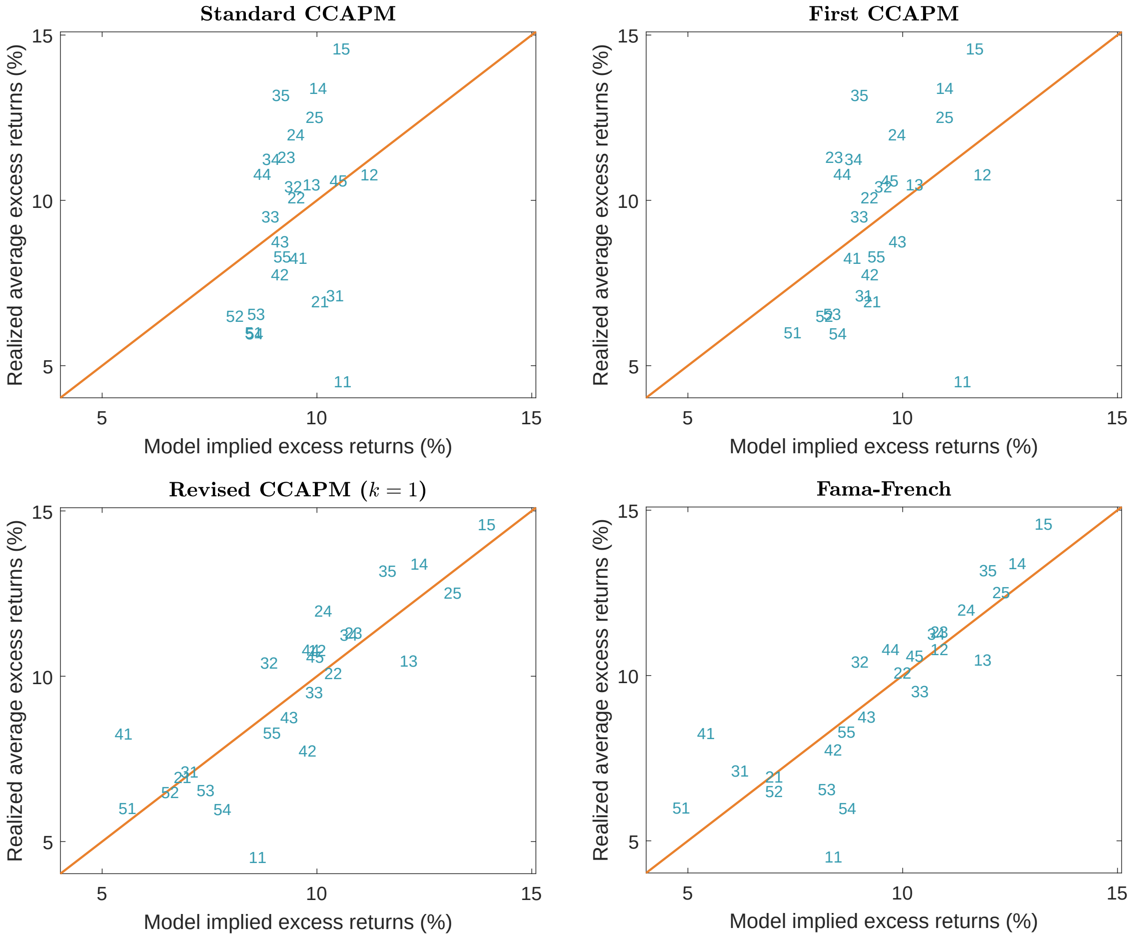
This table reports estimated coefficients, t-statistics (in parenthesis), R^2 and time period of various permutations of risk and uncertainty proxies, $RISK_{t+1} = (\hat{\sigma}^{\text{GARCH}}_{t+1}, RV_{t+1}, \hat{\sigma}^{\text{MS}}_{t+1})'$ and $AMB_{t+1} = (SPF_{t+1}, IV_{t+1})'$, respectively, in the regressions $|v_{t+1|t+1+k}| = \theta RISK_{t+1} + \psi AMB_{t+1} + u_{t+1}$, $k = 1$. Details of the construction of $RISK_{t+1}$ and AMB_{t+1} can be found in Section E.2. GARCH indicates conditonal volatility estimated by a GARCH(1,1), $\hat{\sigma}^{\text{GARCH}}_{t+1}$, RV indicates realized volatility of consumption growth estimated as the sum of squared intraquarter monthly first release consumption growth rates, RV_{t+1}, and MS indicates the markov-switching conditional volatility from Boguth and Kuehn (2013), $\hat{\sigma}^{\text{MS}}_{t+1}$. SPF indicates the cross-sectional variance at each time point across all professional forecasters' nowcast on current consumption growth and VXO indicates the implied volatility, VXO Index. All variables are standardized. The t-statistics are constructed with HAC standard errors that are robust to heteroskedasticity and autocorrelation by Newey and West (1987) with a Bartlett kernel and data-driven lag selection based on Andrews (1991).

Risk proxy			Ambiguity proxy			
GARCH.	RV	MS	SPF	VXO	R^2(%)	Time period
-0.09			0.35		11.47	1982-2015
(-1.21)			(5.86)			
	0.09		0.33		11.53	1982-2015
	(0.91)		(5.52)			
		-0.07	0.35		11.47	1982-2009
		(-0.75)	(5.05)			
-0.17				0.33	10.25	1986-2015
(-2.11)				(2.40)		
	0.02			0.28	7.78	1986-2015
	(0.16)			(1.96)		
		-0.18		0.36	12.19	1986-2009
		(-1.17)		(2.10)		
-0.09	0.12	-0.08	0.34		12.97	1982-2009
(-0.94)	(1.07)	(-0.66)	(4.89)			
-0.15	0.05	-0.16		0.37	13.98	1986-2009
(-1.37)	(0.51)	(-0.99)		(2.04)		
-0.14	0.04	-0.14	0.26	0.29	19.93	1986-2009
(-1.53)	(0.39)	(-0.94)	(3.32)	(1.65)		

Table 10:
Resampling from 202 portfolios: R^2 summary
This table reports a summary of the adjusted cross-sectional R^2 using the Standard CCAPM, First CCAPM, Revised CCAPM with $k = 1$ and benchmark models across subsamples of the set of 202 test portfolios. We randomly sample 10,000 times (without replacement) 25 portfolios. The left column reports average adjusted cross-sectional R^2 and the right column reports the share of samples where the Revised CCAPM with $k = 1$ provides a strictly greater adjusted cross-sectional R^2 than its benchmark models, i = {Standard CCAPM, First CCAPM, Benchmarks}. Numbers are in percentages.

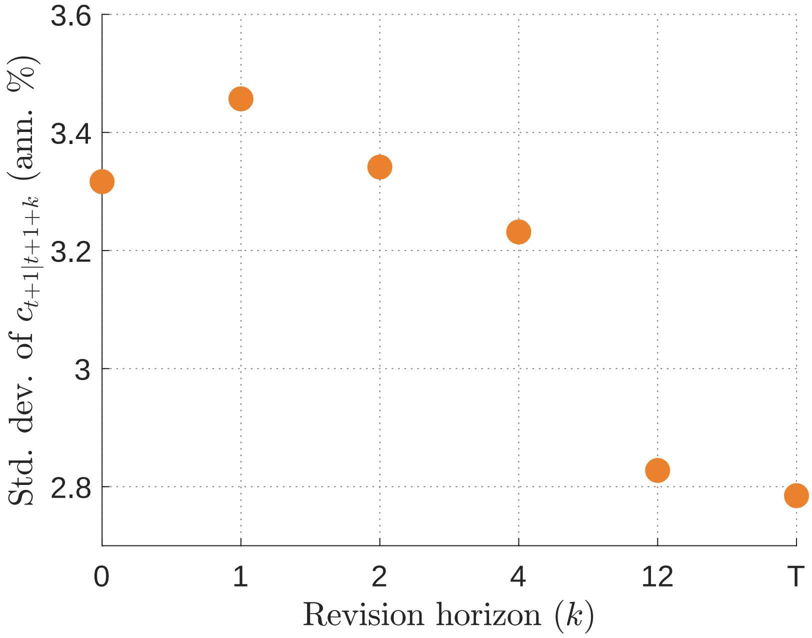
Model	Mean R^2	R^2(Revised CCAPM) > $R^2(i)$
Standard CCAPM	7.78	91.86
First CCAPM	12.94	87.29
Revised CCAPM	28.23	-
Ult. CCAPM	28.64	48.51
Q4-Q4 CCAPM	7.28	85.18
Cay CCAPM	16.60	77.33

Figure 1: Realized versus predicted returns



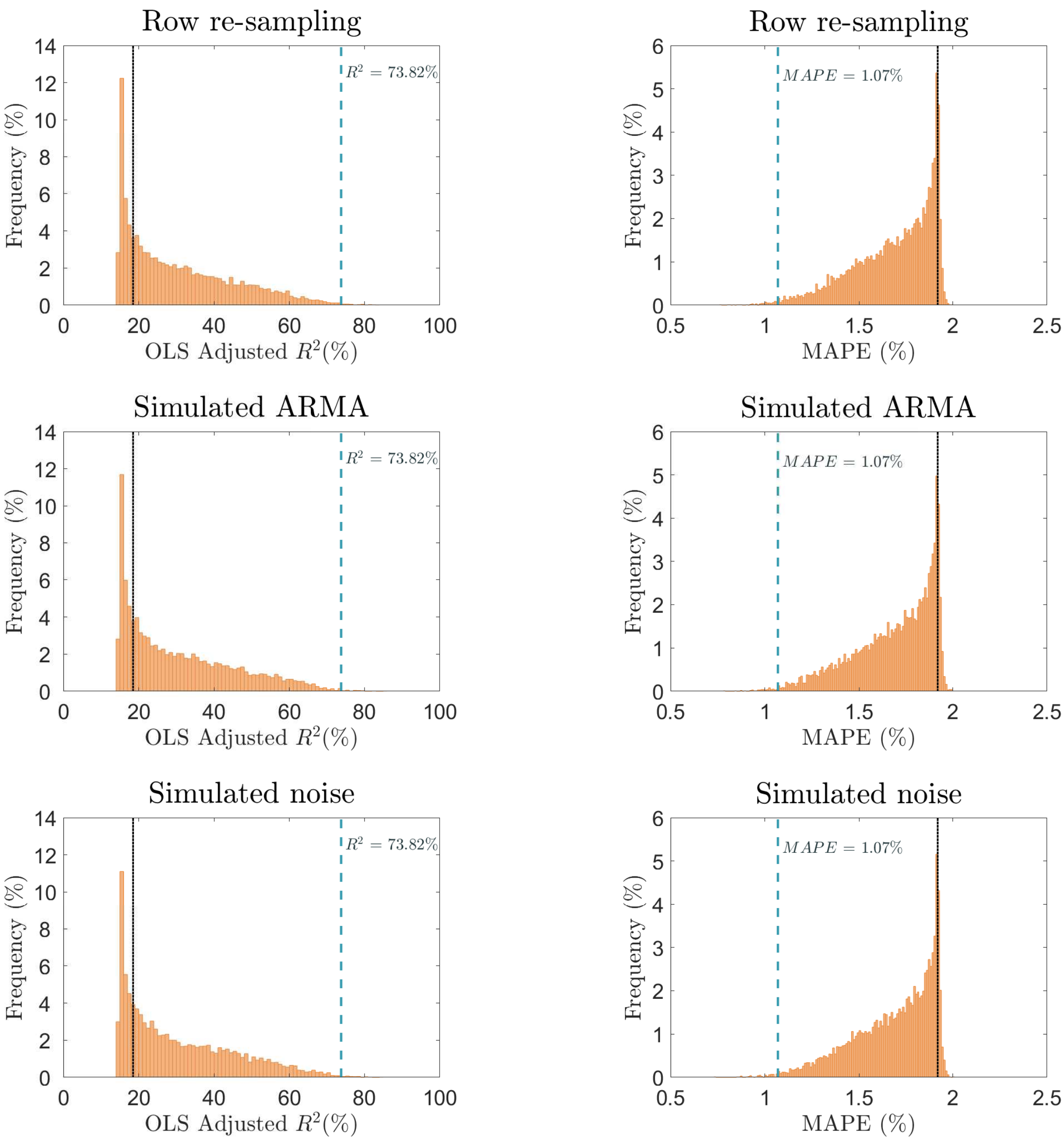
This figure depicts realized average excess returns versus model predicted excess returns (annualized percentage per quarter) for the 25 Fama-French size and book-to-market ratio sorted portfolios. The estimated models are the Standard CCAPM using final data, the First CCAPM using first release data only, the Revised CCAPM using first release data and a revision uncertainty component ($k = 1$), and the three-factor Fama-French model. The first (second) number indicates the size (book-to-market) quintile.

Figure 2: Variance profile of consumption releases



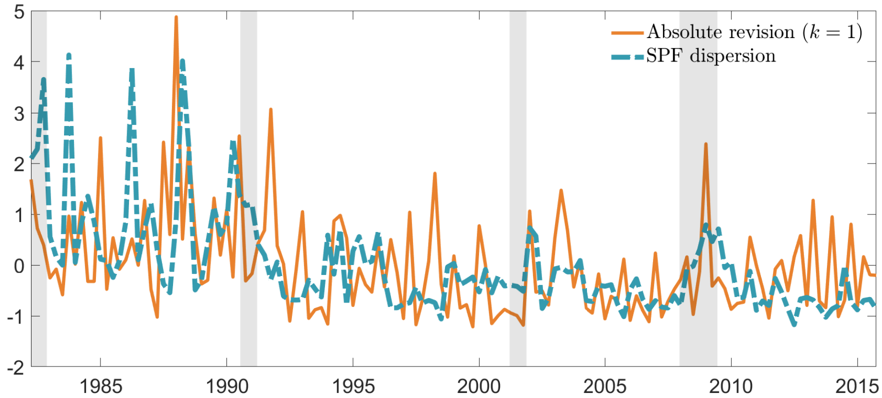
This figure depicts the standard deviation of consumption growth data releases, $c_{t|t+k}$ for $k = 0, 1, 2, 4, 12, T$ over the sample period 1965Q1:2015Q3. The values are annualized and reported in percentages.

Figure 3: Empirical placebo distributions



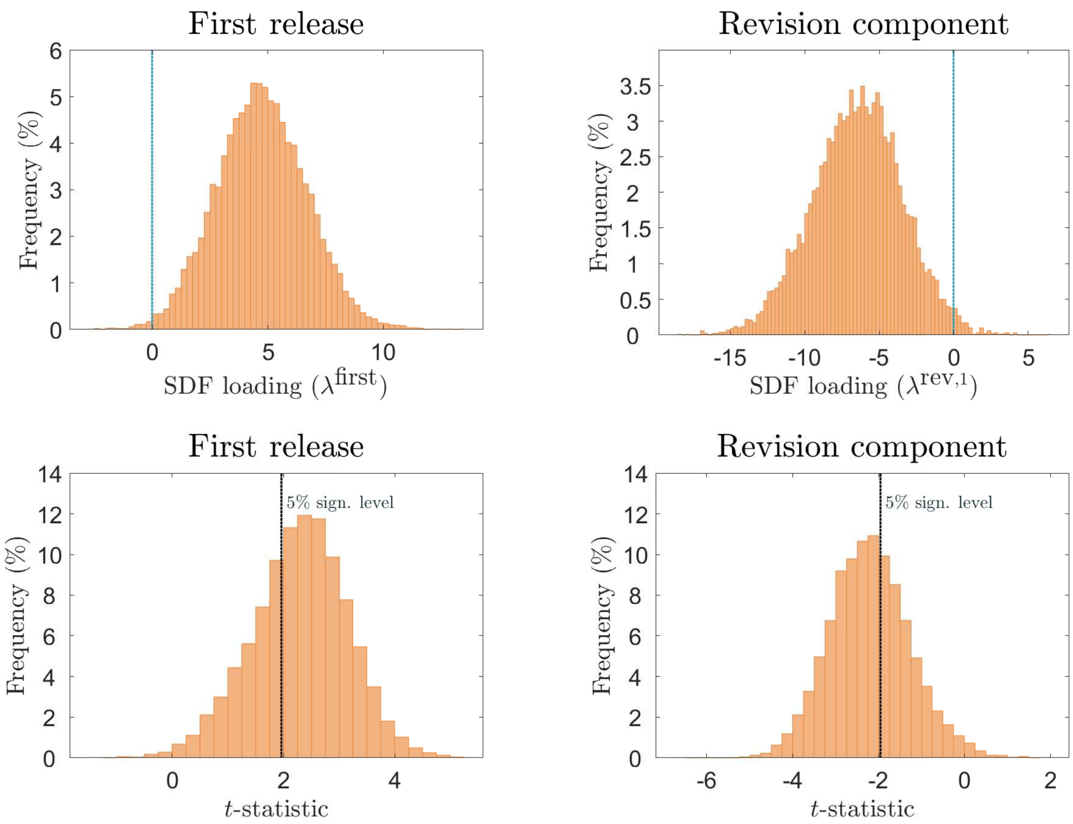
This figure shows empirical placebo distributions of the adjusted cross-sectional R^2 (left panel) and MAPE (right panel) for the Revised CCAPM with $k = 1$. The upper panel report results from row resampling (with replacement), the middle panel from simulations used the best fitting ARMA(p,q) representation of revisions obtained using the BIC criteria, and the lower panel using simulations from a normal distribution. In the two latter cases the variance is matching the sample variance of the revisions series. The black dashed line indicate adjusted cross-sectional R^2 (left panel) and MAPE (right panel) using the First CCAPM. The blue dashed line indicate the adjusted cross-sectional R^2 (left panel) and MAPE (right panel) using the Revised CCAPM with $k = 1$. We conduct 10,000 placebo replications.

Figure 4: Absolute revisions versus SPF dispersion



This figure depicts the time series of absolute revisions, $k = 1$, (orange solid line) together with our ambiguity proxy obtained as the cross-sectional variance of SPF nowcasts of consumption growth (blue dashed line). Values are standardized. Gray shaded areas indicate NBER recessions.

Figure 5: Resampling from 202 portfolios: SDF loading summary



This figure depicts histograms of SDF loadings (λs) (upper panel) and associated t-statistics (lower panel) using the Revised CCAPM with $k = 1$ across subsamples of the set of 202 rest portfolios. We randomly sample 10,000 times (without replacement) 25 portfolios and in each sample we estimate the Revised CCAPM with $k = 1$ following Section A. The dotted blue line indicates zero in the upper panel and the black dotted line indicates the 5% critical value in a two-sided statistical significance test of the SDF loading in the lower panel.

Supplementary Appendix for

Asset pricing with data revisions

(not intended for publication)

A. Data sources and summary statistics

This section outlines the data sources and transformation used in the paper.

A.1. Consumption data

We obtain quarterly data on personal consumption expenditure for nondurable goods and its subsequent releases from the Archival Federal Reserve Economic Database (ALFRED) at the Federal Reserve Bank of St. Louis. This data set contains first releases as well as all subsequent releases. The data spans from 1965:Q1 to 2018:Q2 and is transformed to continuously compounded growth rates. Since we will consider a revision horizon up to $k = 12$ this limits the end of the sample to 2015:Q2, using that $k = T$ simply uses all available revisions up until the end of the original sample. The first advance estimate of nondurables consumption expenditure related to the previous quarter is released at the end of the first month of each quarter. Subsequent revisions are released at the end of the second and third month of each quarter. Given these multiple releases within the same quarter, we pick the first release as the relevant release series, in line with Aruoba (2008). We do not adjust for population growth since it is substantially revised but only available in vintage data form from 1999 and onwards, hence avoiding contaminating results by using final revised population growth data. Nominal excess returns are deflated using the final Personal Consumption Expenditures implicit price deflator obtained from the U.S. Bureau of Economic Analysis on nondurables consumption.

A.2. Predictors

Our set of predictors is inspired by the literature on consumption forecasting. Stock prices are quarterly averages based on the Standard and Poor's 500 Index. The interest rate is the quarterly average based on the three-month Treasury bill rate, reported monthly by the Board of Governors of the Federal Reserve System. Both variables are obtained from CRSP. Labor income is obtained from the Bureau of Economic Analysis and defined as wages and salaries plus transfers minus personal contributions for social insurance, as it appears in the quarterly components from the Department of Commerce's National Income and Product Accounts. Since it is not possible to obtain first release series for the labor income, we use final release data. All three variables are log differenced. We also include the quarterly average of the overall index and expectations index of Consumer sentiment from the Survey Research Center of the University of Michigan. We construct the first release inflation and industrial production growth predictors using the continuously compounded growth series for the Consumer Price Index and Industrial Production Index from the Archival Federal Reserve Economic Database (ALFRED) at the Federal Reserve Bank of St. Louis. The Economic Policy Uncertainty index is the quarterly average of the monthly overall EPU index developed by Baker et al. (2016) and obtained from https://www.policyuncertainty.com/). For data prior to 1985 we use the News-Based Historical Economic Policy Uncertainty and standardize the data such that the average difference between the two series is minimized in the period in which both series overlap. We obtain the dividend-yield ratio and default yield from the website of Amil Goyal (http://www.hec.unil.ch/agoyal/). The former is defined as the difference between the log of dividends and the log of lagged prices and the latter as the difference between the return on BAA and AAA corporate bonds. All predictors span the period from 1965:Q1 to 2018:Q2.

A.3. Risk and ambiguity proxies

We use three proxies for risk. The first is the conditional volatility as estimated by a markov-switching model in Boguth and Kuehn (2013) using a Markov model. The data which spans 1965:Q1 to 2009:Q4, is obtained from Oliver Boguth's website (http://www.public.asu.edu/~oboguth/research.html). The second risk proxy is the conditional variance estimate of a GARCH(1,1) model on consumption growth rates during our complete sample period 1965:Q1 to 2018:Q4. Table A.1 reports the parameter estimates and log likelihood value. The third risk proxy is the realized volatility, which we construct as the sum of the squared intraquarter monthly logarithmic growth rates of real nondurable consumption growth. We obtain monthly vintage data similarly to the baseline quarterly data set from ALFRED. We construct two ambiguity proxies. The first proxy is the cross-sectional forecast dispersion between individual forecasters for total consumption growth from the Survey of Profesional Forecasters. We measure dispersion using the cross sectional variance estimate at each point in time. We also use the difference between the 75th percentile and the 25th percentile. Our SPF data for total consumption covers the period from 1982:Q1 to 2018:Q2. Our second measure of ambiguity is the quarterly average of the Chicago Board Options Exchange S&P 100 Volatility Index (VXO). The data, which spans from

1986:Q2 to 2018:Q2 is obtained from the Chicago Board Options Exchange (CBOE).

Table A.1: GARCH(1,1) estimates

This table reports the parameters from estimation of the GARCH(1,1) model on $c_{t+1|t+1}$ specified as $\sigma_{t+1}^2 = \omega + \beta\sigma_t^2 + \alpha\varepsilon_t^2$, where ε_t is the innovation from the conditional mean equation, assuming iid $\varepsilon_t \sim N(0,1)$. We report t-statistics in parenthesis.

ω	β	α	Log likelihood
0.62	0.91	0.04	-565.71
(0.41)	(6.35)	(0.68)	

B. Revision predictability

Our set of exogenous predictors is derived from the literature on consumption forecasting and our data sources and construction of predictors are described in the former section of this Appendix. The baseline macro-financial variables typically included in the existing literature are the return to the S&P 500 Index, the three-month Treasury Bill rate and labor income growth, see Carroll, Fuhrer, and Wilcox (1994) and Ludvigson (2004). We also include measures of consumer confidence, see Lahiri, Monokroussos, and Zhao (2016), obtained form the Survey of Consumers administrated by the Survey Research Center of the University of Michigan. They publish an overall Index of Consumer sentiment, which can be decomposed into a present conditions index and an expectations index. Following Lahiri et al. (2016), we use both the overall index and the expectations index. We also form a first release inflation predictor, see e.g. Boons and Prado (2019), as well as a first release industrial production growth predictor. We also include a measure of economic policy uncertainty (Baker et al., 2016) as well as the dividend-yield ratio (Goyal and Welch, 2008). All predictors are available over the 1965-2018 period.

In Tables A.2-A.6 we report predictability results using this broad set of predictors and the model in (14) of the paper.

Table A.2: Revision predictability ($k = 1$)

This table provides in-sample and out-of-sample results for predictive regressions of revisions at a horizon of one period ahead, $k = 1$. The panel on the left shows the slope coefficients, Wald test for joint significance and R^2 of a regression of the revision component on the first release and an exogenous predictor, $v_{t|t+1} = a + \tilde{b}c_{t|t} + dx_t + \varepsilon_t$, over the over the 1965-2018 period. The panel on the right shows the out-of-sample results for the same regression. Specifically, it shows the out-of-sample R^2, $R^2_{OoS} = 1 - \sum_{t=1}^{T}\left(v_{t|t+1} - \widehat{v_{t|t+1}})^2\right)/\sum_{t=1}^{T}\left(v_{t|t+1}\right)^2$ and corresponding p-value using a Diebold-Mariano test. We use an expanding window scheme with an initial window of 15 years (60 time periods), and generate the first forecasts for 1980Q1.

	In-sample		Out-of-sample	
Variable	Wald p-value	$R^2(\%)$	$R^2_{OoS}(\%)$	DM p-value
Economic policy uncertainty	0.494	0.42	-5.11	0.744
Inflation	0.443	0.42	-1.98	0.703
Labor income	0.434	1.56	-6.99	0.775
Industrial production	0.292	1.28	-0.28	0.536
Michigan Index (overall)	0.071	2.38	1.11	0.359
Michigan Index (expectations)	0.148	1.61	0.35	0.451
SP500 Index returns	0.488	0.44	-2.75	0.841
T-bill rate	0.475	0.40	-4.05	0.826
Dividend yield	0.210	0.66	-4.50	0.846
$c_{t\|t}$	0.389	0.35	-1.07	0.644

Table A.3: Revision predictability ($k = 2$)

This table provides in-sample and out-of-sample results for predictive regressions of revisions at a horizon of one period ahead, $k = 2$. The panel on the left shows the slope coefficients, Wald test for joint significance and R^2 of a regression of the revision component on the first release and an exogenous predictor, $\nu_{t|t+2} = a + \tilde{b}c_{t|t} + dx_t + \varepsilon_t$, over the over the 1965-2018 period. The panel on the right shows the out-of-sample results for the same regression. Specifically, it shows the out-of-sample R^2, $R^2_{OoS} = 1 - \sum_{t=1}^{T}\left(\nu_{t|t+2} - \widehat{\nu_{t|t+2}})^2\right) / \sum_{t=1}^{T}\left(\nu_{t|t+2}\right)^2$ and corresponding p-value using a Diebold-Mariano test. We use an expanding window scheme with an initial window of 15 years (60 time periods), and generate the first forecasts for 1980Q1.

	In-sample		Out-of-sample	
Variable	Wald p-value	$R^2(\%)$	$R^2_{OoS}(\%)$	DM p-value
Economic policy uncertainty	0.139	3.07	-0.78	0.546
Inflation	0.057	3.22	3.32	0.096
Labor income	0.002	6.81	2.26	0.369
Industrial production	0.109	3.26	3.07	0.071
Michigan Index (overall)	0.019	4.52	4.27	0.043
Michigan Index (expectations)	0.022	4.34	4.65	0.014
SP500 Index returns	0.093	2.96	1.67	0.264
T-bill rate	0.060	3.30	0.71	0.425
Dividend yield	0.059	3.29	1.09	0.393
$c_{t\|t}$	0.069	2.92	3.63	0.034

Table A.4: Revision predictability ($k = 4$)

This table provides in-sample and out-of-sample results for predictive regressions of revisions at a horizon of one period ahead, $k = 4$. The panel on the left shows the slope coefficients, Wald test for joint significance and R^2 of a regression of the revision component on the first release and an exogenous predictor, $\nu_{t|t+4} = a + \tilde{b}c_{t|t} + dx_t + \varepsilon_t$, over the over the 1965-2018 period. The panel on the right shows the out-of-sample results for the same regression. Specifically, it shows the out-of-sample R^2, $R^2_{OoS} = 1 - \sum_{t=1}^{T}\left(\nu_{t|t+4} - \widehat{\nu_{t|t+4}})^2\right)/\sum_{t=1}^{T}\left(\nu_{t|t+4}\right)^2$ and corresponding p-value using a Diebold-Mariano test. We use an expanding window scheme with an initial window of 15 years (60 time periods), and generate the first forecasts for 1980Q1.

	In-sample		Out-of-sample	
Variable	Wald p-value	$R^2(\%)$	$R^2_{OoS}(\%)$	DM p-value
Economic policy uncertainty	0.002	7.99	6.31	0.262
Inflation	0.000	9.00	13.04	0.013
Labor income	0.000	9.71	6.64	0.151
Industrial production	0.000	8.78	12.45	0.004
Michigan Index (overall)	0.000	11.19	15.01	0.004
Michigan Index (expectations)	0.000	10.63	14.88	0.003
SP500 Index returns	0.000	8.75	12.49	0.012
T-bill rate	0.000	8.48	9.80	0.056
Dividend yield	0.000	9.35	10.89	0.059
$c_{t\|t}$	0.000	7.98	12.18	0.010

Table A.5: Revision predictability ($k = 12$)

This table provides in-sample and out-of-sample results for predictive regressions of revisions at a horizon of one period ahead, $k = 12$. The panel on the left shows the slope coefficients, Wald test for joint significance and R^2 of a regression of the revision component on the first release and an exogenous predictor, $v_{t|t+12} = a + \tilde{b}c_{t|t} + dx_t + \varepsilon_t$, over the over the 1965-2018 period. The panel on the right shows the out-of-sample results for the same regression. Specifically, it shows the out-of-sample R^2, $R^2_{OoS} = 1 - \sum_{t=1}^{T}\left(v_{t|t+12} - \widehat{v_{t|t+12}})^2\right) / \sum_{t=1}^{T}\left(v_{t|t+12}\right)^2$ and corresponding p-value using a Diebold-Mariano test. We use an expanding window scheme with an initial window of 15 years (60 time periods), and generate the first forecasts for 1980Q1.

	In-sample		Out-of-sample	
Variable	Wald p-value	$R^2(\%)$	$R^2_{OoS}(\%)$	DM p-value
Economic policy uncertainty	0.000	29.07	27.42	0.029
Inflation	0.000	27.71	31.60	0.064
Labor income	0.000	31.14	32.06	0.070
Industrial production	0.000	29.45	34.36	0.045
Michigan Index (overall)	0.000	34.11	38.91	0.040
Michigan Index (expectations)	0.000	33.23	38.62	0.037
SP500 Index returns	0.000	29.23	33.75	0.037
T-bill rate	0.000	27.61	28.54	0.090
Dividend yield	0.000	27.66	28.72	0.075
$c_{t\|t}$	0.000	27.61	32.75	0.049

Table A.6: Revision predictability ($k = T$)

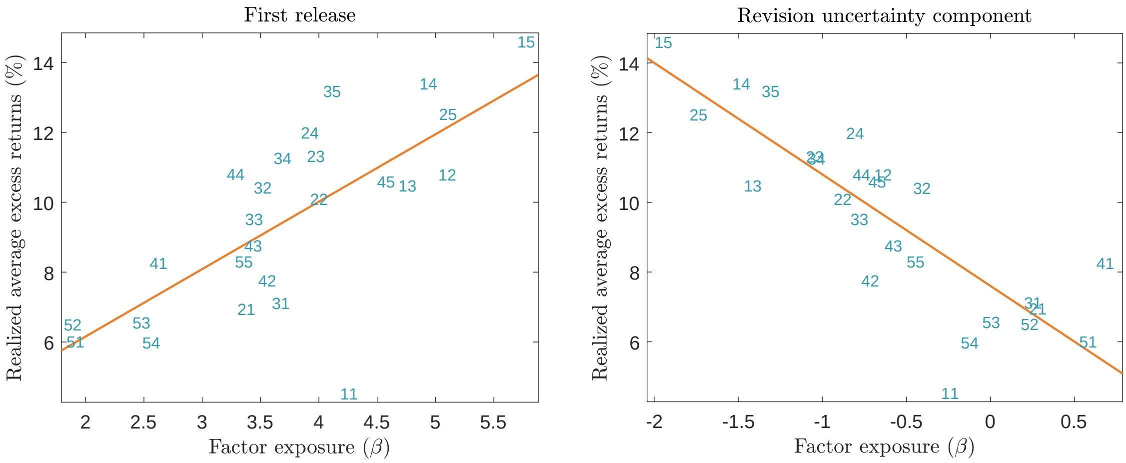
This table provides in-sample and out-of-sample results for predictive regressions of revisions at a horizon of one period ahead, $k = T$. The panel on the left shows the slope coefficients, Wald test for joint significance and R^2 of a regression of the revision component on the first release and an exogenous predictor, $\nu_{t|T} = a + \tilde{b}c_{t|t} + dx_t + \varepsilon_t$, over the over the 1965-2018 period. The panel on the right shows the out-of-sample results for the same regression. Specifically, it shows the out-of-sample R^2, $R^2_{OoS} = 1 - \sum_{t=1}^{T}\left(\nu_{t|T} - \widehat{\nu_{t|T}})^2\right)/\sum_{t=1}^{T}\left(\nu_{t|T}\right)^2$ and corresponding p-value using a Diebold-Mariano test. We use an expanding window scheme with an initial window of 15 years (60 time periods), and generate the first forecasts for 1980Q1.

	In-sample		Out-of-sample	
Variable	Wald p-value	$R^2(\%)$	$R^2_{OoS}(\%)$	DM p-value
Economic policy uncertainty	0.000	34.56	-	-
Inflation	0.000	32.89	-	-
Labor income	0.000	40.59	-	-
Industrial production	0.000	33.95	-	-
Michigan Index (overall)	0.000	41.03	-	-
Michigan Index (expectations)	0.000	40.27	-	-
SP500 Index returns	0.000	34.43	-	-
T-bill rate	0.000	32.52	-	-
Dividend yield	0.000	32.57	-	-
$c_{t\|t}$	0.000	32.51	-	-

C. Factor exposures and average excess returns

This section shows the relationship between the realized average excess returns and the factor exposure to first release consumption growth and the revision uncertainty component.

Figure A.1: Realized average excess returns versus factor exposures



This figure depicts realized average excess returns (annualized percentage per quarter) versus factor exposures for the 25 Fama-French size and book-to-market ratio sorted portfolios. The estimated model are the Revised CCAPM using first release data and a revisions uncertainty component ($k = 1$). The first (second) number indicates the size (book-to-market) quintile. The solid orange line indicates the best linear relationship between realized average excess returns and factor exposures.

D. Implication of state reference point

Define the reference point dependent revision uncertainty term by

$$\tilde{\varphi}^k_{t+1} = (c_{t+1|t+1} - \tilde{c}_{t+1}) \cdot |\nu_{t+1|t+1+k}|,$$

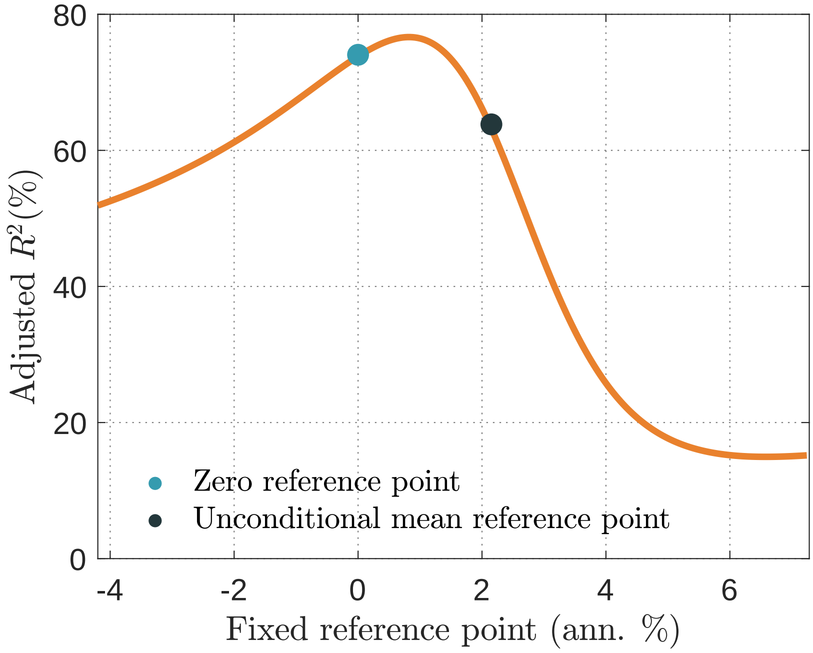
where $\tilde{c}_{t+1}$ is the reference point. If $\tilde{c}_{t+1} = 0$ we obtain the model used in our baseline results presented in the paper and $\tilde{\varphi}^k_{t+1} = \varphi^k_{t+1}$. We consider a large set of either fixed or time-varying values of the reference point. In the former cases, we consider a dicretization of the values lying between the 5'th and 95'th percentile first release consumption growth rates, encompassing zero and the unconditional mean. Arguably, values close to zero or slighly positive are the most likely candidates for a plausible reference point. We show, however, the full results for transparency. In the time-varying reference point cases, we consider equal-weighted and exponential-weighted moving averages over a window of 1,3,5, and 10 years in the past. We also adapt the certainty equivalent consumption growth as per Delikouras and Kostakis (2019) to using first releases, which is defined as

$$\tilde{c}^{\text{CE}}_{t+1} = \mu_c(1-\phi_c) + \phi_c c_{t|t} + d_2\sqrt{1-\phi_c^2}\sigma_c, \tag{D.1}$$

where μ_c, σ_c, and ϕ_c are the unconditional mean, standard deviation and first-order autocorrelation, respectively. The parameter d_2 is a solution to a fixed-point problem and we use the value of -0.770 reported in Table A6 in the Online Appendix of Delikouras and Kostakis (2019), which is estimated on quarterly data and the 25 size and book-to-market portfolios as test assets. We obtain estimates for μ_c, σ_c, and ϕ_c from Table 1 in the main paper. Lastly, we consider the cyclical component of consumption growth obtained by the linear projection method of Hamilton (2018). This component was recently applied in the context of personal consumption expenditure by Atanasov et al. (2019), finding clear state-dependencies in expected returns as a function of whether consumption resides above or below its cyclical component. We follow their approach. Specifically, we regress the first release in logarithmic levels on a constant and four lags. We choose these lags following the recommendations of Hamilton (2018), according to a two-year cycle horizon. The regression error is the measure of cyclical consumption. We then compute logarithmic growth rates of this cyclical component over the cycle horizon and use it as a measure of time-varying reference point. Indeed, this can under suitable assumptions be seen as an approximation to the surplus consumption in the external habit formation model of Campbell and Cochrane (1999) as shown by Atanasov et al. (2019).

We report the results for a fixed reference point in Figure A.2 with a discretization of the range of fixed state reference points by values of 0.01%. It is evident that in all specifications of plausible fixed reference points the pricing ability of the Revised CCAPM is high and sometimes even higher than our baseline results reported in the paper. In fact, it reveals that our natural and implicit choice of $\tilde{c}_{t+1} = 0$ in the base line results presented above is conservative relative to the best achievable values. As the reference point becomes increasingly unlikely, performance deteriorates unsurprisingly. Interestingly, the figure reveals an asymmetric shape in the fixed reference point. A similar conclusion is obtained from the time varying reference point cases. In all specifications considered, the SDF loading on both the first release and revision uncertainty component maintain their signs from the baseline results and remain statistically significant on conventional significance levels, most on the 1% level. The pricing performance metrics R^2 and MAPE remain high and low, respectively, and show little sensitivity to the alteration of the method of constructing a time-varying reference point. Taken together, these results suggest no concern that our interpretation of the pricing of the revision uncertainty component in the Revised CCAPM is sensitive to the implicit choice of reference point.

Figure A.2: Fixed state reference point and adjusted R^2



This figure depicts the adjusted cross-sectional R^2 from the Revised CCAPM using values between the 5'th and 95'th percentile first release consumption growth rates as fixed reference point, $\tilde{c}$, in the revisions uncertainty component. The zero reference point is highlighted with a blue circle and the unconditional mean with a black circle.

Table A.7: Implication of time varying state reference point
This table reports the t-statistics associated with the SDF loadings (λs), adjusted cross-sectional R^2 and mean absolute pricing error MAPE for the Revised CCAPM using various specifications of a time-varying reference point, $\tilde{c}_{t+1}$, in the revisions uncertainty component. Panel A considers the certainty equivalent return from Delikouras and Kostakis (2019) and cyclical consumption growth computed as the logarithmic cyclical growth rates of cyclical consumption as in Atanasov et al. (2019). Panel B (C) uses equal-weighted (exponentially-weighted) moving averages over past horizons of 1 to 10 years. The test assets are the 25 size and book-to-market ratio sorted portfolios. Standard errors of the SDF loadings are reported in parenthesis below each estimate, using GMM standard errors that are robust to errors-in-variables as well as to heteroskedasticity and autocorrelation by Newey and West (1987) with a Bartlett kernel and data-driven lag selection based on Andrews (1991).

Time varying reference point	λ^{first}	$\lambda^{\text{rev},1}$	$R^2(\%)$	MAPE(%)
Panel A: Certainty equivalent and cyclical component				
Certainty equivalent	2.86	-2.58	70.66	1.15
Cyclical component	2.76	-2.48	51.30	1.39
Panel B: Equal-weighted moving average				
1 year	2.37	-2.33	69.21	1.06
3 years	1.90	-3.08	73.73	0.97
5 years	2.49	-3.13	68.36	1.09
10 years	2.49	-3.59	62.64	1.14
Panel C: Exponentially-weighted moving average				
1 year	2.32	-2.16	63.16	1.16
3 years	2.31	-2.69	74.94	0.98
5 years	2.41	-2.94	73.62	0.98
10 years	2.58	-3.17	67.60	1.10

E. Proof of theoretical result

Proof of Proposition 1: Recall the beta representation

$$\mathbb{E}[r_{i,t+1}] = \beta_i^c \gamma^c, \tag{E.2}$$

where $\beta_i^c = \text{cov}[r_{i,t+1}, c_{t+1}]/\text{var}[c_{t+1}]$ and $\gamma^c \geq 0$ is the market price of immediate consumption growth risk. Replacing (the generic) c_{t+1} by the data sample $c_{t+1|t+1+k}$ implies together with the decomposition in (8) of the paper that

$$\begin{aligned}
\beta_i^{\text{final}} &= \frac{\text{cov}\left[r_{i,t+1}, c_{t+1|t+1+k}\right]}{\text{var}\left[c_{t+1|t+1+k}\right]} \\
&= \frac{\text{cov}\left[r_{i,t+1}, c_{t+1|t+1} + v_{t+1|t+1+k}\right]}{\text{var}\left[c_{t+1|t+1+k}\right]} \\
&= \frac{\text{cov}\left[r_{i,t+1}, c_{t+1|t+1}\right] + \text{cov}\left[r_{i,t+1}, v_{t+1|t+1+k}\right]}{\text{var}\left[c_{t+1|t+1+k}\right]} \\
&= \frac{\text{var}\left[c_{t+1|t+1}\right]}{\text{var}\left[c_{t+1|t+1}\right]} \frac{\text{cov}\left[r_{i,t+1}, c_{t+1|t+1}\right]}{\text{var}\left[c_{t+1|t+1+k}\right]} + \frac{\text{var}\left[v_{t+1|t+1+k}\right]}{\text{var}\left[v_{t+1|t+1+k}\right]} \frac{\text{cov}\left[r_{i,t+1}, v_{t+1|t+1+k}\right]}{\text{var}\left[c_{t+1|t+1+k}\right]} \\
&= \eta^{\text{first}} \beta^{\text{first}} + \eta^{\text{rev},k} \beta^{\text{rev},k},
\end{aligned} \tag{E.3}$$

where

$$\eta^{\text{first}} = \frac{\text{var}\left[c_{t+1|t+1}\right]}{\text{var}\left[c_{t+1|t+1+k}\right]} \quad \text{and} \quad \eta^{\text{rev},k} = \frac{\text{var}\left[v_{t+1|t+1+k}\right]}{\text{var}\left[c_{t+1|t+1+k}\right]}, \tag{E.4}$$

and

$$\beta^{\text{first}} = \frac{\text{cov}\left[r_{i,t+1}, c_{t+1|t+1}\right]}{\text{var}\left[c_{t+1|t+1}\right]} \quad \text{and} \quad \beta^{\text{rev},k} = \frac{\text{cov}\left[r_{i,t+1}, v_{t+1|t+1+k}\right]}{\text{var}\left[v_{t+1|t+1+k}\right]}. \tag{E.5}$$

Note that the implied betas from these derivations are univariate betas, obtainable from a single time series regression of returns, $r_{i,t+1}$, onto each factor separately (including a constant). The result in the Proposition now follows by combining (E.2) and (E.3) and defining

$$\gamma^{\text{first}} = \gamma^c \eta^{\text{first}} \quad \text{and} \quad \gamma^{\text{rev},k} = \gamma^c \eta^{\text{rev},k}. \tag{E.6}$$

□