Long-Run Risk-Return Trade-Offs
(Very preliminary - comments welcome)

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Abstract

Expected excess market returns depend on past variance. This dependence is statistically mild at short-horizons (thereby leading to a hard-to-detect risk-return trade-off as in the previous literature) but increases with the horizon and is stronger in the long-run. These findings are robust to the statistical properties of long-horizon stock-return predictive regressions. If past variance risk is compensated, our results point to long-run notions of the classical risk-return trade-off. The reported long-run trade-offs are not simple aggregations of short-run trade-offs but imply reasonable levels of risk aversion.

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1 Introduction

The well-known risk-return trade-off implies the existence of a positive relation between the conditional expected excess return on the market and the market’s conditional variance, i.e.,

\[ E_t[R_{t,t+1}] = \gamma Var_t[R_{t,t+1}] \] (1)

with \( \gamma > 0 \). Both \( E_t[.] \) and \( Var_t[.] \) are expectations conditional on time \( t \) information.

This paper studies the classical risk-return relation in Eq. (1) by looking at different time horizons. Specifically, we rewrite the model in Eq. (1) as

\[ E_t[R_{t,t+h}] = \gamma_h Var_t[R_{t,t+h}] \] (2)

where \( h \) defines the horizon and \( \gamma_h \) denotes the corresponding risk-return slope. The monthly model is expressed by setting \( h \) equal to 1. We consider values of \( h \) between 1 and 120 (up to 10 years of monthly data). Hence, \( \{ \gamma_h : h = 1, 3, ..., 120 \} \) defines a term-structure of risk-return slopes.

Using classical methods of inference and the conventional rolling-window estimator of French et al. (1987), i.e., the sum of within-period daily squared returns, we find strongly significant dependences between expected excess returns and past conditional variances for values of \( h \) equal to 72, 84, 96, 108, and 120 (6 to 10 years). The \( R^2 \)’s from a regression of \( R_{t,t+h} \) on the sum of daily squared returns between \( t - h \) and \( t \) are between about 36% and 67%. This finding contrasts sharply with the short-horizon results. When focusing on horizons between one month (\( h = 1 \)) and 4 years (\( h = 48 \)), the corresponding \( R^2 \)’s are never larger than 2%. We show that the use of alternative methods of inference providing more accurate representations of the finite sample distributions of the relevant test statistics under the null of no dependence only mitigates, but by no means eliminates, the statistical significance of the reported long-run relations. These robust methods unequivocally point to stronger dependences between expected excess market returns and conditional market variances in the long-run than in the short-run. If long-run variance is predicted using past long-run variance, these dependences lead to classical risk-return trade-offs. Interestingly, the long-horizon regressions deliver coefficients \( \gamma_h \) which are between 3 and 5. These values are consistent with conventional, structural interpretations of \( \gamma_h \) as a risk-aversion parameter (see, e.g., Merton, 1973).

The existing work on the risk-return trade-off has focused on short horizons, i.e., horizons between one month and one year at most. The findings are mixed. Baillie and De Gennaro
(1990), French et al. (1987), and Campbell and Hentschel (1992), among others, find a positive, but largely insignificant, relation between conditional variance and conditional expected returns. The results of Campbell (1987) and Nelson (1991), \textit{inter alia}, point to a significantly negative relation. Glosten et al. (1989) and Turner et al. (1989), among others, report either a positive or a negative relation depending on the model used. More recently, Scruggs (1998), Ghysels et al. (2005), and Guo and Whitelaw (2006) find a risk-return trade-off.

This mixed evidence has led to a search for richer models for conditional variances and conditional expected returns. While much of the existing research has focused on modelling conditional variances, it is well-known that conditional means are harder to identify. Most papers use realized returns. At short-horizons, realized returns are of course very noisy proxies of expected returns. In recent work, Lundbland (2005) points out that an extremely long sample of market return data is necessary to clearly uncover the existing (positive) relation between risk and return. His sample covers almost two centuries of monthly data, i.e., it extends from 1836 to 2003. As in Campbell (1987) and Whitelaw (1994), among others, Lettau and Ludvingson (2006) use conditioning variables to estimate conditional excess returns. The implied cost of capital is employed in Pástor et al. (2006).

In this paper we step back and take a somewhat different approach. We use realized daily returns (sampled between 1962 and 2004) and simple rolling-window estimators of conditional variance to abstract, as much as possible, from unnecessary (in our framework) methodological complications. However, our focus on the long-run distinguishes our work from the existing literature on the subject. In the long-run, the relation between expected excess market returns and conditional past variance is positive and statistically significant. While we show that this relation can be uncovered using classical statistical methods, robust statistical inference is necessary to clearly assess its extent.

The rest of the paper is structured as follows. Section 2 presents the data. Section 3 discusses our main empirical finding, i.e., the long-run dependence between excess market returns and past conditional variance. Section 4 provides simulations, a representation of the finite sample distributions of the relevant statistics under the null of no dependence, and a method of robust inference. Section 5 re-evaluates and confirms our findings in the presence of an extended sample (1933 to 2004). Section 6 discusses important issues of interpretation.

of the empirical results. Section 7 concludes.

2 The data

We use the NYSE/Amex value-weighted index with dividends as our market proxy. The risk-free rate is the 30-day T-bill rate. The data are downloaded from CRSP for the period July 3, 1962 - December 31, 2004. To compute monthly continuously-compounded excess returns \( R \) we aggregate daily continuously-compounded excess returns \( r - r^f \) by defining

\[
R_{t,t+1} = 22 \sum_{j=1}^{n_t} \frac{1}{n_t} \left( r_{t+j/n_t} - r^f_{t+j/n_t} \right),
\]  

(3)

where \( n_t \) is the number of trading days in month \( t \). To remove scaling effects deriving from a different number of trading days for each month, we express the data in terms of a typical month of 22 trading days.

Our basic variance measure is the monthly realized variance obtained by summing squared continuously-compounded daily returns during each month, i.e.,

\[
\sigma^2_{t,t+1} = 22 \sum_{j=1}^{n_t} w_j r^2_{t+j/n_t},
\]  

(4)

\[
w_j = \frac{1}{n_t}.
\]  

(5)

Due to its simplicity, this measure has a very long history in finance. French et al. (1987), for instance, use it in the study of the risk-return trade-off at the monthly level \((h = 1)\). Ghysels et al. (2005) have recently extended this traditional approach to allow for \((i)\) a longer window of data \((i.e., n_t \text{ larger than the number of days in a month})\) and \((ii)\) the use of unequally-weighted daily squared returns \((w_j \neq w_i \text{ for } i \neq j)\).\(^2\) Among other results, Ghysels et al. (2005) show that their more flexible approach delivers statistically significant \(\gamma\) estimates at short horizons. This is in contrast with the findings in French et al. (1987). It is also in contrast with the results in Ghysels et al. (2005) when using the rolling-window estimator in Eq. (4). In addition, the magnitude of their estimated parameter \(\gamma\) is economically meaningful and consistent with structural interpretations.

\(^2\)Their maximum \(n_t\) is set equal to 252, virtually one year of trading days. Their weights are defined as \(w_j(\kappa_1, \kappa_2) = \frac{\exp\{\kappa_1 j + \kappa_2 j^2\}}{\sum_{n=0}^{\infty} \exp\{\kappa_1 n + \kappa_2 n^2\}}\). The weights are positive, sum up to one, and decay to zero if \(\kappa_2\) is negative. The parameters \(\kappa_1\) and \(\kappa_2\) are estimated by quasi-maximum likelihood.
In what follows, we find economically meaningful and statistically significant \( \gamma \) estimates in the long-run. Importantly, we do so by using simple rolling-window estimates, i.e., without resorting to either the flexible approach of Ghysels et al. (2005) or alternative variance forecasts. Our short-run results agree with those obtained by French et al. (1987) (and those obtained by Ghysels et al. (2005) when using rolling-window estimates). Hence, it is the long-run that matters.

Table 1 presents descriptive statistics for \( R_{t,t+1} \) and \( \sigma^2_{t,t+1} \). As typically found in the literature, monthly conditional variances are considerably more skewed and fat-tailed than monthly returns.

### 3 Long-run risk-return trade-offs

Write the \( h \)-period continuously-compounded return as \( R_{t,t+h} = \sum_{i=1}^{h} R_{t+i-1,t+i} \). The corresponding \( h \)-period variance process is \( \sigma^2_{t,t+h} = \sum_{i=1}^{h} \sigma^2_{t+i-1,t+i} \). As in French et al. (1987) and Ghysels et al. (2005), among others, one might predict \( \text{Var}_t[R_{t,t+h}] \) using past values, i.e.,

\[
\sigma^2_{t-h,t} = \sum_{i=1}^{h} \left( 22 \sum_{j=1}^{n_{t-i}} \frac{1}{n_{t-i}} r^2_{(t-i)+j} \right).
\]

Notice that if \( h = 1 \), then

\[
\sigma^2_{t-1,t} = 22 \sum_{j=1}^{n_{t-1}} \frac{1}{n_{t-1}} r^2_{(t-1)+j}.
\]

This is the estimator used in French et al. (1987) and, barring important differences of data length and data weighing as discussed above, in Ghysels et al. (2005).

Figure 1 reports scatter plots of excess market returns and realized \( \text{past} \) variance at four levels of aggregation, namely \( h = 1, 12, 60, \) and 120. At the monthly frequency, the risk-return relation is unclear and certainly not revealed by the use of standard proxies for conditional means and variances, such as realized returns and realized variances. As we increase the level of aggregation, an apparent trade-off appears. To provide an initial assessment of the extent of the trade-off, we run the regression

\[
R_{t,t+h} = a_h + b_h \sigma^2_{t-h,t} + \varepsilon_{t,t+h}
\]

for values of \( h \) between 1 (one month) and 120 (10 years). The estimated slopes, standard errors, and \( R^2 \)s are reported in Table 2. In Table 2, and in all other tables below, the notation...
\( h = 3 \), for instance, signifies use of overlapping quarterly data. We correct the standard errors for the serial correlation induced by the overlapping nature of the data by using a kernel variance estimator with a quadratic spectral kernel and a bandwidth selected according to Andrews’ (1991) data-based rule.

At the monthly level (\( h = 1 \)), our results perfectly mirror the results of French et al. (1987). We find a slope coefficient equal to \(-.596\) and insignificant. French et al. (1987) also find a statistically insignificant coefficient equal to \(-.349\) using data from 1928 to 1984. Except for the quarterly frequency, we obtain a positive, and significant, slope coefficient only when aggregating data for 72 months (6 years) and over. Our long-run slope estimates are stable between 4 and 5.5. These values are economically meaningful and consistent with conventional structural interpretations relating the regressions’ slopes to the risk-aversion parameter (we will come back to this observation in what follows). The long-run \( R^2 \)'s are large, i.e., between about 36\% (at 6 years) and about 67\% (at 9 years). These preliminary findings point to a statistically significant, long-run risk-return trade-off when conditional variance is predicted using past values.

For a clearer assessment, Figure 2 provides a graphical representation of the term structure of estimated slopes and corresponding 95\% bands. The figure illustrates our main point. On the one hand, the presence of a risk-return trade-off is hard to detect at short/medium horizons (1 month to about 5 years). In this range, the slopes’ 95\% bands include zero. This difficulty is well-known and has lead to a search for more sophisticated methods of inference. On the other hand, the risk-return trade-off is more apparent at longer horizons. Classical statistical methods appear to clearly reveal it.

### 3.1 A useful restriction: zero intercept

In this subsection we constrain the intercept to be zero. This restriction is consistent with the classical risk-return relation in Eq. (1). It is also justifiable based on the insignificance (with only a few exceptions) of the estimated intercept coefficients in the previous regressions. From a statistical standpoint, provided the restriction is true, consistency of the slope estimator is preserved in general. In addition, it is well-known that the slope parameter is estimated with increased precision.\(^3\) Lanne and Saikkonen (2006) have recently made a similar point in a GARCH-in-mean model. Specifically, they have shown that the inclusion of an intercept term

\[ \sum_{i=1}^{n} (x_i - \bar{x})^2 \leq \sum_{i=1}^{n} x_i^2 \]

\(^3\)In a univariate regression model with predetermined regressors \( x \), the variance of the slope estimator goes from \( \frac{\sigma^2}{\sum_{i=1}^{n} (x_i - \bar{x})^2} \) to \( \frac{\sigma^2}{\sum_{i=1}^{n} x_i^2} \) when imposing the restriction. Clearly, \( \sum_{i=1}^{n} (x_i - \bar{x})^2 \leq \sum_{i=1}^{n} x_i^2 \).
in these models can lead to imprecise estimates of the variance-in-mean coefficient. The results are in Table 3.

We find that the restriction increases the statistical significance of our estimates at virtually all horizons. Consistent with a standard risk-return trade-off, all estimates are positive and, with the sole exception of the 1-month case, statistically significant at all conventional levels. Importantly, the long-run slopes increase monotonically, as do their corresponding t-statistics. The dependence between long-run market returns and past long-run variance is strong, and stronger than in the unrestricted case. The t-statistics increase from about 4.5 at the 5-year horizon to about 15.5 at the 10-year horizon.

4 Inferential issues

4.1 Simulations

This section evaluates the accuracy of classical and Newey-West-adjusted asymptotic inference in our framework. To this extent, we simulate monthly returns and an autoregressive variance process under the assumption of no dependence. Subsequently, we aggregate as previously done with data. The simulated process is:

\[ R_{t,t+1} = b_1 \sigma_{t-1,t}^2 + \varepsilon_{t,t+1}, \]  

(9)\[ \sigma_{t,t+1}^2 = \rho_0 + \rho_1 \sigma_{t-1,t}^2 + u_{t,t+1}, \]  

(10)

with \( \rho_0 = 0, \rho_1 = 0.6, \sigma_\varepsilon = 1, \sigma_u = 1, \) and \( \rho_{\varepsilon u} = -0.3 \) under \( H_0 : b_1 = 0 \) (i.e., the null of no dependence). The parameter values are meant to replicate the properties of our disaggregated monthly series (more on this later). Consistent with data, we simulate 510 monthly observations. The number of simulated paths is equal to 10,000.

We run the regression in Eq. (8) for \( h = 1, 3, ..., 120 \). We also consider regressions where the intercept \( a_h \) is constrained to be 0. We test the null \( b_h = 0 \) for each choice of \( h \) at nominal level 5%. As above, we correct the standard errors by using a kernel variance estimator with a quadratic spectral kernel and a bandwidth selected according to Andrews’ (1991) data-based rule.

The third and fourth row of Table 4 report the test sizes for the unconstrained intercept case when the standard \( t \) statistic and the HAC statistic are used. As is well-known, the overlapping leads to severe size distortions of the test \( b_h = 0 \). HAC corrections (applied earlier with data) reduce these distortions drastically, but the actual size is much beyond the nominal
size. With an horizon of 60 months (5 years), the actual size of the HAC test is 31.3% rather than 5%. The results from constraining the constant are in the second panel of Table 4. The same pattern is observed, but size distortions are a little less pronounced than in the unconstrained case (mainly when employing HAC estimates). At the 60-months horizon, for example, we find a size of 26.8%.

The spuriously increasing slope estimates and $R^2$'s, as well as the fairly large size distortions leading to over-rejections of the null of zero slope, are obvious concerns. Despite differences in aggregation, these findings are reminiscent of similar findings in the context of classical predictive regressions of long-run returns on persistent (non-aggregated) financial ratios (Valkanov, 2003, and Boudouck et al., 2005, among others). These behaviors have led to questioning the informational content of long-run regressions about the predictability of stock returns (Boudouck et al., 2005). The next subsection modifies the asymptotic framework proposed by Valkanov (2003) in the context of predictive regressions of long-run returns on financial ratios in order to derive tests of the null of no trade-off with improved size properties.

4.2 An alternative asymptotic approximation

Write

$$R_{t,t+1} = b_1 \sigma_{t-1,t}^2 + \varepsilon_{t,t+1},$$

$$\sigma_{t,t+1}^2 = \rho_0 + \rho_1 \sigma_{t-1,t}^2 + u_{t,t+1},$$

with $\rho_0 = 0$ and $\rho_1 = 1 + \frac{\varphi}{T}$. Assume the vector $[\varepsilon_{t,t+1}, u_{t,t+1}]$ is a vector martingale difference sequence with covariance matrix $[\sigma^2_\varepsilon, \sigma_{\varepsilon u}, \bullet, \sigma^2_u]$. The parameter $c$ is a constant measuring deviations from unity that are decreasing in $T$. This framework is widely adopted in predictive regressions with persistent regressors (see, among others, Campbell and Yogo, 2005, Valkanov, 2003, and Bandi, 2004, for a nonlinear approach). In our context, $\rho_1$ is smaller than in predictive regressions with persistent financial ratios (i.e., the negative parameter $c$ is larger in absolute value). However, we will show by simulations that the convenient local-to-unity approach captures the salient finite sample features of our long-run regressions.

Consider again the regressions in Eq. (8). As in Valkanov’s asymptotic framework (Valkanov, 2003), we assume that $h = [\Delta T]$, i.e., the portion of the overlap is a constant fraction of the sample size ($[x]$ denotes, as always, the largest interval that is less than or equal to $x$). Differently from Valkanov’s framework, however, regressor and regressand are aggregated over nonoverlapping periods.
We are interested in the behavior of the slope estimates, coefficient of determination, and test size for the null \( b_h = 0 \) (no trade-off) under Eq. (11) and Eq. (12). Proposition 1 and Proposition 2 contain the relevant asymptotic approximations. Their proofs follow classical embedding methods in the unit-root literature (see, e.g., Phillips, 1991, and Cavanagh et al., 1995, among others).\(^4\) For a thorough treatment of these methods in the context of long-run predictability issues in finance, we refer the reader to Valkanov (2003). In what follows the symbol \( \Rightarrow \) denotes weak convergence as \( T \to \infty \).

**Proposition 1 (The unrestricted regressions.)**

If the return and variance process follow Eq. (11) and Eq. (12), \( b_1 = 0 \), and the regression in Eq. (8) is run, then

\[
\sqrt{T} b_{h=\lfloor T \rfloor} \Rightarrow \frac{\sigma_x}{\sigma_u} \int_0^{1-\lambda} W(s, \lambda) \mathcal{J}_c(s, -\lambda) ds
\]

\[
\sqrt{T} \hat{a}_{h=\lfloor T \rfloor} \Rightarrow \frac{\sigma_x}{1 - 2\lambda} \int_0^{1-\lambda} (W(s + \lambda) - W(s)) ds \mathcal{J}_c(s, -\lambda)
\]

\[
\frac{\sqrt{T} b_{h=\lfloor T \rfloor}}{\sqrt{T}} \Rightarrow \frac{\int_0^{1-\lambda} W(s, \lambda) \mathcal{J}_c(s, -\lambda) ds}{\sqrt{\left( \int_0^{1-\lambda} W^2(s, \lambda) ds \int_0^{1-\lambda} \mathcal{J}_c^2(s, -\lambda) ds - \left( \int_0^{1-\lambda} W(s, \lambda) \mathcal{J}_c(s, -\lambda) ds \right)^2 \right)}}
\]

\[
R^2 \Rightarrow \frac{\left( \int_0^{1-\lambda} W(s, \lambda) \mathcal{J}_c(s, -\lambda) \right)^2}{\int_0^{1-\lambda} \mathcal{J}_c^2(s, -\lambda) ds \int_0^{1-\lambda} W^2(s, \lambda) ds}
\]

where

\[
W(t, \lambda) = \{ W(t + \lambda) - W(t) \} - \frac{1}{1 - 2\lambda} \int_0^{1-\lambda} (W(s + \lambda) - W(s)) ds,
\]

and

\[
\mathcal{J}_c(t, -\lambda) = \left\{ \int_{t-\lambda}^t \mathcal{J}_c(s) ds \right\} - \frac{1}{1 - 2\lambda} \int_0^{1-\lambda} \left( \int_{t-\lambda}^t \mathcal{J}_c(s) ds \right) dt
\]

with

\[
d\mathcal{J}_c(s) = cs + dB(s) \quad \mathcal{J}_c(0) = 0
\]

and

\[
\{ W(s), B(s) \},
\]

is a vector of standard Brownian motions with covariance \( \frac{\sigma_u \sigma_w}{\sigma_u \sigma_c} \).

\(^4\) They are available from the authors upon request.
Proposition 2 (The restricted regressions.)

If the return and variance process follow Eq. (11) and Eq. (12), \( b_1 = 0 \), and the regression in Eq. (8) is run with \( a_h = 0 \), then

\[
\begin{align*}
Tb_{h=|\lambda T|} & \Rightarrow \frac{\sigma_x}{\sigma_u} \int_{\lambda}^{1-\lambda} W(s, \lambda) J_c(s, -\lambda) \frac{1}{\int_{\lambda}^{1-\lambda} J_c^2(s, -\lambda)}, \\
\frac{t_{b_{h=|\lambda T|}}}{\sqrt{T}} & \Rightarrow \sqrt{\int_{\lambda}^{1-\lambda} \left( \int_{\lambda}^{1-\lambda} W^2(s, \lambda) ds \int_{\lambda}^{1-\lambda} J_c^2(s, -\lambda) ds - \left( \int_{\lambda}^{1-\lambda} W(s, \lambda) J_c(s, -\lambda) \right)^2 \right)}, \\
R_{h=|\lambda T|}^2 & \Rightarrow \left( \int_{\lambda}^{1-\lambda} W(s, \lambda) J_c(s, -\lambda) \right)^2 \frac{1}{\int_{\lambda}^{1-\lambda} J_c^2(s, -\lambda) ds \int_{\lambda}^{1-\lambda} W^2(s, \lambda) ds},
\end{align*}
\]

where

\[ W(t, \lambda) = W(t + \lambda) - W(t), \]

and

\[ J_c(t, -\lambda) = \int_{t-\lambda}^{t} J_c(s) ds \]

with

\[ dJ_c(s) = cds + dB(s) \quad J_c(0) = 0, \]

and

\[ \{W(s), B(s)\}, \]

is a vector of standard Brownian motions with covariance \( \frac{\sigma_x}{\sigma_u} \).

The variance process is embedded in a mean-reverting Ornstein-Uhlenbeck (OU) process with mean parameter \( c \). Under the null of no trade-off, long-run returns are embedded in a standard Brownian motion. The correlation between the system’s shocks makes the limiting OU process and the limiting Brownian motion correlated. The asymptotic distributions are stochastic functionals of these processes (and/or their demeaned versions). Differently from the results in Valkanov (2003), in light of the different aggregation method, the range of integration of the functionals is \( (\lambda, 1-\lambda) \) and the distance between the upper and lower limit of the stochastic integrands \( W(t, \lambda) \) and \( J_c(t, -\lambda) \) is \( 2\lambda \).

Despite the autocorrelation of our regressor (variance) being lower than in classical long-run predictive regressions (and arguably not a near-unit-root), the asymptotic approximations capture the qualitative features of the simulations reported in the previous section. Under the null of no trade-off, the slope estimator is super-consistent. However, its limiting distribution
has a bias that is increasing (in absolute value) with the degree of overlap (i.e., with $\lambda$). If $\sigma_{\tilde{e}_u} < 0$, as in our data, the bias is positive. It is negative with $\sigma_{\tilde{e}_u} > 0$. Similarly, the $R^2$ converges to a random variable whose mean increases with the overlap. Importantly, the standard t-statistic diverges with $T$, thereby determining likely over-rejections in the classical asymptotic framework. As in Valkanov (2003), we will rely on the pivotal (given the parameters $c$ and $\sigma_{\tilde{e}_u}$) statistic $t_{b_{bh}} = \frac{t_{bh} - \lambda T}{\sqrt{T}}$ to test the null of no trade-off. The last row of Table 4 (first and second panel) report the rejection probabilities when using $t_{bh} - \lambda T$ in our simulations. The critical values are generated assuming $c = (\rho_1 - 1)T$ with $\rho_1 = 0.6$ and $\sigma_{\tilde{e}_u}^2 = -0.3$. In other words, we assume the parameters are known. In the relevant region of the parameter space, however, we find that the distribution is not very sensitive to these values. Hence, the results appear to be a good indication of what can be achieved in practice. All rejection probabilities lie between 4.8% and 5.6% and are very close to the nominal size of 5%.

5 Robust trade-offs

Table 5 contains inference on the risk-return trade-off based on the $t_{bh} - \lambda T$ statistic. We report the statistic as well as the 5% right-tail critical values for the limiting distributions in Proposition 1 and Proposition 2 obtained using autoregressive parameters equal to 0.2 and 0.6. The former corresponds to the estimated autoregressive parameter of the monthly volatility series over the full sample. The latter roughly corresponds to the estimated autoregressive parameter of the monthly series when excluding the 1987 crash.

The statistical significance of the trade-off has now decreased somewhat. The long-run trade-off is still significant at the 5% level over 7, 8, and 9 years. It is also significant at the 10% level at 6 years. As pointed out earlier, restricting the intercept to be zero (as implied by the classical risk-return trade-off) leads to more precise slope estimates. This is particularly true in the long-run since the intercepts estimate expected values and expected values can hardly be identified with only few observations. In this case, we again find a strong long-run trade-off at all conventional levels (see Table 5, panel 2).

In order to provide further evidence about the significance of our results at long horizons, we also report critical values based on the bootstrap. Our algorithm is similar to the one suggested by Killian (1999). It involves resampling the monthly excess returns and realized variance using a simple VAR(1) model and then aggregating up these monthly data as we do in our empirical analysis. In addition, we impose the null hypothesis of no trade-off and allow for
heteroskedasticity through the use of the wild bootstrap. More specifically, we fit the following bivariate VAR(1) model to our data:

$$
\begin{pmatrix}
R_{t,t+1}^* \\
\sigma_{t,t+1}^*
\end{pmatrix} =
\begin{pmatrix}
a_{11} & a_{12} \\
a_{21} & a_{22}
\end{pmatrix}
\begin{pmatrix}
R_{t-1,t}^* \\
\sigma_{t-1,t}^*
\end{pmatrix} +
\begin{pmatrix}
\varepsilon_{1,t+1}^* \\
\varepsilon_{2,t+1}^*
\end{pmatrix},
$$

(13)

and generate our bootstrap data by i.i.d. resampling of the residuals \( \begin{pmatrix}
\varepsilon_{1,t+1}^* \\
\varepsilon_{2,t+1}^*
\end{pmatrix} \). Our bootstrap sample is constructed from:

$$
\begin{pmatrix}
R_{t,t+1}^* \\
\sigma_{t,t+1}^*
\end{pmatrix} =
\begin{pmatrix}
\hat{a}_{11} & 0 \\
\hat{a}_{21} & \hat{a}_{22}
\end{pmatrix}
\begin{pmatrix}
R_{t-1,t}^* \\
\sigma_{t-1,t}^*
\end{pmatrix} + z_t
\begin{pmatrix}
\varepsilon_{1,t+1}^* \\
\varepsilon_{2,t+1}^*
\end{pmatrix},
$$

(14)

where \( z_t \) is an independent random variable with expectation zero and variance 1 to allow for heteroskedasticity (we use a standard normal distribution). We start the recursion at the unconditional expectation of the VAR(1) process. The 0 in the right-hand corner of the coefficient matrix imposes the null hypothesis of a zero trade-off.

Given a bootstrap sample \( \{ R_{t,t+1}^*, \sigma_{t,t+1}^* \}_{t=1}^{510} \), we aggregate the data to order \( h \), estimate the regression

$$
R_{t,t+h}^* = a_h + b_h \sigma_{t-h,t}^* + \varepsilon_{t,th}^*,
$$

(15)

and compute the \( \frac{t_{t-b(\chi^2)}}{\sqrt{T}} \) statistic. We do this 999 times and report the right-tail critical value for an equal-tailed 5% bootstrap test to make results easily comparable. As earlier, without constraining the intercept, the trade-off is significant at the 5% level at 7, 8, and 9 years. With the constrained intercept, the results are significant at 3 months, and between 6 and 10 years. The bootstrap provides further evidence about the existence of a long-run trade-off.

Learning about long-run behavior with 40 years of monthly data can be a complicated task. We further robustify our inference by considering the extended sample 1934-2004. Table 7 contains the corresponding results with a constrained intercept. As in the shorter sample, in the long-run the standardized t-statistic is virtually twice as large as the right-tail critical values for a 5% test. Importantly, the slope estimates have the same magnitude as earlier and, of course, are still consistent with structural interpretations relating them to risk aversion.

6 Issues of interpretation

6.1 Observations on variance prediction

Martingale prediction, i.e., the use of past variance to predict future variance, is of course justifiable when variance is highly persistent. Table 6 presents the variance first-order autocorrelations at all levels of aggregation. We report averages of autocorrelations computed using
non-overlapping realized variances over each period. Variance is highly positive dependent at short horizons. This is well-known. The relatively low, but positive, autocorrelation coefficient (0.2) for the 1-month variances should not come as a surprise. It is simply a by-product of the 1987 crash. If we confined ourselves to pre-crash data (or if we used the extended sample in the previous section), we would find an autocorrelation value equal to about 0.6 for \( h = 1 \). Using only post-crash data we would obtain an autocorrelation value equal to about 0.5 over the same horizon. Starting at 48 months, the point estimates of the autocorrelation parameters become negative. The negative long-run correlations should again not be surprising. These values are statistically insignificant and fully consistent with our assumed model. Table 6 shows estimated autoregressive parameters obtained from simulating the process in Eq. (12) with \( \rho_0 = 0 \) and \( \rho_1 = 0.6 \). The long-run autocorrelations are negative. This effect is spurious but fairly typical. Given the data generating process, the volatility autocorrelation should converge to zero with the level of aggregation. In typical simulations (and in the data) the point estimates can of course be negative. In sum, as implied by a standard autoregression, variance is virtually uncorrelated in the long-run.

This result deserves particular attention in our framework. If agents formed expectations of future long-run variance using the wrong variance model, i.e., assuming high persistence in the long-run, then testing the classical risk-return trade-off using past variance would be meaningful even in the long run. Consequently, the interpretation of the slope estimates as a risk-aversion parameter would seem valid. Conversely, if agents realize that past long-run variance is likely not the best predictor of future long-run variance, then the regression in Eq. (8) can not be interpreted as a test of the classical risk-return trade-off. However, the regression would still uncover a different, important, risk-return trade-off, i.e., the trade-off between long-run returns and past long-run variance. Importantly, here predictability derives from the positive lagged correlation between two series, i.e., long-run market returns and long-run variances, whose individual temporal dynamics are largely uncorrelated.

6.2 Observations on return prediction

The previous discussion has provided robust statistical evidence against the null hypothesis of no long-run trade-off, i.e., \( H_0 : b_1 = 0 \). Since \( H_0 : b_1 = 0 \) is rejected in the long-run, is it believable that \( H_0 : b_1 = b \), where \( b \) is a fixed number (possibly a risk-aversion parameter)? In other words, are the reported long-run results compatible with a disaggregated risk-return model with \( b_1 = b \) as in Eq. (11)? This subsection argues that they are not. Proposition
3 discusses the asymptotic properties of our long-run regressions’ slope estimators and $R^2$'s when the disaggregated data generating process implies a trade-off between monthly returns and past monthly variance.

**Proposition 3**

*If the return and variance process follow Eq. (11) and Eq. (12), $b_1 = b$, and the regression in Eq. (8) is run with $a_h = 0$, then

$$\hat{b}_{h=|\lambda T|} = b \frac{\int_0^{1-\lambda} J_c(s, \lambda) J_c(s, -\lambda)}{\int_0^{1-\lambda} J_c^2(s, -\lambda)},$$

$$R^2_{h=|\lambda T|} \Rightarrow 1 - \left( \frac{\int_0^{1-\lambda} J_c^2(s, \lambda) J_c^2(s, -\lambda) - \left( \int_0^{1-\lambda} J_c(s, \lambda) J_c(s, -\lambda) \right)^2}{\int_0^{1-\lambda} J_c^2(s, -\lambda) \int_0^{1-\lambda} J_c^2(s, \lambda)} \right),$$

where

$$J_c(t, -\lambda) = \int_{t-\lambda}^t J_c(s) ds$$

with

$$dJ_c(s) = c ds + dB(s) \quad J_c(0) = 0$$

and $B(s)$ is a standard Brownian motion.*

The proposition implies that $\hat{b}_{h=|\lambda T|}$ does not estimate $b$ consistently. Neglecting embedding arguments, simple aggregation of the model in Eq. (11) and Eq. (12) makes this statement obvious since, for a degree of overlap $\lambda$, the true slope coefficient is $b \rho^{\lambda T}$. Naturally, $b \rho^{\lambda T}$ becomes smaller and smaller with the level of aggregation. Interestingly, given the parameter values in the simulation above, the limiting distribution of $\hat{b}_{h=|\lambda T|}$ has a negative bias that increases with $\lambda$. In the data the long-run slopes fail to decrease with the degree of overlap. Instead, they tend to stabilize around values that are generally associated with acceptable risk aversion parameter values. Similarly, Proposition 3 implies that, for a large $\lambda$ and our assumed parameter values, the limiting distribution of $R^2$ should be more concentrated around zero. This is again contrary to our findings. Taken all together, these observations imply that the disaggregated model in Eq. (11) and Eq. (12), and the type of aggregation that would derive from it, do not seem to be supported by the data. In other words, a more general model than a classical disaggregated monthly specification appears to be needed to account for our long-run findings. Future research should focus on this issue and its structural justification.
7 Conclusions

This paper illustrates, and provides support for, an interesting empirical phenomenon, i.e., the dependence between long-run market returns and past long-run variance.

Numerous variables are believed to predict long-run returns. Financial ratios are a well-known example (see, e.g., the discussion in Cochrane, 2001). Long-run variance is a somewhat less obvious predictor than predictors with prices in them, like the dividend yield. Prices are low (high dividend yield) when people expect/demand higher returns. Prices are low, and expected returns are high, in recessions. Moving forward, low prices predict higher expected and realized returns (in expansions). In other words, prices are bound to go back up eventually.

In the case of long-run (past) variance, despite the documented higher volatility in recessions, the link between predictor and future long-run returns seems somewhat less mechanical.

In the short-term, the positive dependence between past variance and future returns has been documented by Whitelaw (1994). His use of predictors to forecast expected excess returns and conditional variance reduces the noise that is present in short-term realized returns yielding a risk-return relation. While the lack of a short-term, statistically significant relation is not surprising in our case (we simply use realized returns), the extent of the long-run dependence between past variance and excess returns is novel.

Why are long-run returns related to past variance? Past variance is certainly not the best predictor of future variance. Hence, why should agents use past long-run variance to predict future long-run variance and demand a premium for it? This paper’s goal is to illustrate an empirical fact. Future work should study its economic determinants.
References


Figure 1. Excess returns and realized variance at different levels of aggregation
Figure 2. Slope estimates at different levels of aggregation and 95% confidence intervals.
### Table 1. Descriptive statistics

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<td>Kurtosis</td>
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### Table 2. Risk-return estimates at different levels of aggregation (HAC t-stats in parenthesis): 1962-2004

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<td>(2.606)*</td>
<td>(.807)</td>
<td>(1.131)</td>
<td>(.752)</td>
<td>(1.038)</td>
<td>(1.081)</td>
<td>(.559)</td>
<td>(-.149)</td>
<td>(-.989)</td>
<td>(-1.749)</td>
<td>(-1.775)</td>
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<td>.852</td>
<td>.901</td>
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<td>-.505</td>
<td>.831</td>
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<td>4.951</td>
<td>5.172</td>
<td>5.398</td>
<td>4.493</td>
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<td></td>
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<td>(1.388)</td>
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<td>(-.185)</td>
<td>(-.278)</td>
<td>(.470)</td>
<td>(1.779)</td>
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<td>(5.440)*</td>
<td>(4.991)*</td>
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<td>.7</td>
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<td>.1</td>
<td>.5</td>
<td>1.3</td>
<td>13.5</td>
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<td>54.8</td>
<td>61.5</td>
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### Table 3. Risk-return slopes at different levels of aggregation (HAC t-stats in parenthesis) - intercept constrained to be zero: 1962-2004

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<td>(.037)</td>
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<td>(2.141)*</td>
<td>(3.163)*</td>
<td>(2.291)*</td>
<td>(2.081)*</td>
<td>(3.183)*</td>
<td>(4.559)*</td>
<td>(5.426)*</td>
<td>(7.321)*</td>
<td>(10.556)*</td>
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<td>2.39</td>
<td>2.64*</td>
<td>2.95</td>
<td>3.24*</td>
<td>3.50</td>
<td>4.04*</td>
<td>4.88*</td>
<td>5.35*</td>
<td>6.41*</td>
<td>7.60*</td>
<td>8.89*</td>
</tr>
</tbody>
</table>
Table 4. Simulation results - Comparison of size of 5% tests of null slope

\[ y_t = \varepsilon_t \]

DGP: \( \sigma^2 = 0.6\sigma_{t-1}^2 + u_t \)

\[ \text{corr}(\varepsilon_t, u_t) = -0.3 \]

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<tbody>
<tr>
<td><strong>Estimated intercept</strong></td>
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<td></td>
<td></td>
<td></td>
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<td>Slope estimates</td>
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<td>0.0020</td>
<td>0.0031</td>
<td>0.0051</td>
<td>0.0086</td>
<td>0.0127</td>
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<td>0.0277</td>
<td>0.0326</td>
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<td>(R^2(%))</td>
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<td>.5</td>
<td>.9</td>
<td>1.8</td>
<td>3.6</td>
<td>5.5</td>
<td>7.6</td>
<td>9.9</td>
<td>12.3</td>
<td>14.6</td>
<td>16.9</td>
<td>19.3</td>
<td>21.6</td>
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<td>Standard t-ratio</td>
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<td>22.5</td>
<td>37.9</td>
<td>51.9</td>
<td>64.1</td>
<td>70.6</td>
<td>74.8</td>
<td>77.5</td>
<td>79.6</td>
<td>81.7</td>
<td>82.5</td>
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<td>85.0</td>
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<td>HAC</td>
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<td>6.8</td>
<td>8.2</td>
<td>11.2</td>
<td>16.1</td>
<td>21.4</td>
<td>26.7</td>
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<tr>
<td>(t/\sqrt{T})</td>
<td>5.3</td>
<td>5.1</td>
<td>4.8</td>
<td>5.6</td>
<td>5.0</td>
<td>5.0</td>
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<td>5.3</td>
<td>5.1</td>
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<td>5.1</td>
<td>5.0</td>
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|                |     |     |     |     |     |     |     |     |     |     |     |     |     |
| **Constrained intercept** |     |     |     |     |     |     |     |     |     |     |     |     |     |
| Slope estimates | 0.0004 | 0.0005 | 0.0010 | 0.0015 | 0.0017 | 0.0022 | 0.0028 | 0.0037 | 0.0047 | 0.0050 | 0.0053 | 0.0060 | 0.0067 |
| Standard t-ratio | 5.0 | 22.1 | 37.7 | 51.9 | 63.7 | 70.1 | 74.0 | 77.0 | 78.9 | 80.1 | 81.1 | 82.8 | 84.2 |
| HAC             | 5.2 | 6.5 | 7.8 | 10.4 | 14.8 | 19.2 | 23.4 | 26.8 | 30.8 | 34.9 | 38.5 | 42.1 | 47.0 |
| \(t/\sqrt{T}\)  | 5.2 | 4.9 | 5.4 | 5.1 | 5.2 | 4.8 | 5.6 | 4.9 | 5.1 | 5.4 | 5.6 | 4.8 | 4.8 |

Note: 10,000 replications and \(T = 510\).
Table 5. Risk-return estimates – $t/\sqrt{T}$ statistic and critical values: 1962-2004

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<td><strong>Estimated intercept</strong></td>
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<tr>
<td>$t/\sqrt{T}$</td>
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<td>.126</td>
<td>.085</td>
<td>.102</td>
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<td>.999*</td>
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<td>.182</td>
<td>.262</td>
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<td>.494</td>
<td>.571</td>
<td>.681</td>
<td>.772</td>
<td>.858</td>
<td>.937</td>
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<td>1.112</td>
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<td>.203</td>
<td>.275</td>
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<td>.513</td>
<td>.595</td>
<td>.689</td>
<td>.788</td>
<td>.868</td>
<td>.931</td>
<td>1.056</td>
<td>1.101</td>
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<td>Boot. c.v.</td>
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<td>.134</td>
<td>.182</td>
<td>.257</td>
<td>.363</td>
<td>.470</td>
<td>.609</td>
<td>.693</td>
<td>.740</td>
<td>.789*</td>
<td>.931*</td>
<td>1.032*</td>
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<tr>
<td><strong>Constrained intercept</strong></td>
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<tr>
<td>$t/\sqrt{T}$</td>
<td>.003</td>
<td>.199*</td>
<td>.207*</td>
<td>.284*</td>
<td>.288</td>
<td>.377</td>
<td>.575*</td>
<td>.800*</td>
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<td>1.686*</td>
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<td>2.882*</td>
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<td>Right-tail c.v. (ρ=.2)</td>
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<td>.980*</td>
<td>1.080*</td>
<td>1.270*</td>
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Note: “Right-tail c.v.” is the right-tail critical value for a 5% two-sided test with equal probability of rejection in both tails. The critical values are obtained by simulating the asymptotic distribution with autoregressive coefficients of .2 and .6, correlation between disturbances of -.3, and sample size of 510. 10,000 replications have been used. * denotes significant at 5% level based on a two-sided test. The bootstrap critical values are obtained using the wild bootstrap algorithm described in the text.
Table 6. First-order autoregressive parameter for variance at different levels of aggregation (t-stats in parenthesis): 1962-2004

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<td>.266</td>
<td>.225</td>
<td>.113</td>
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<td>-.598</td>
<td>-.392</td>
<td>-.251</td>
<td>-.225</td>
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<td></td>
<td>(2.10)</td>
<td>(2.83)</td>
<td>(2.39)</td>
<td>(1.50)</td>
<td>(.57)</td>
<td>(.16)</td>
<td>(-1.45)</td>
<td>(-1.87)</td>
<td>(-2.76)</td>
<td>(-2.09)</td>
<td>(-1.45)</td>
<td>(-1.90)</td>
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<td>-.268</td>
<td>-.316</td>
<td>-.363</td>
<td>-.409</td>
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</table>

Table 7. Risk-return estimates at different levels of aggregation (t-stats in parenthesis): 1934-2004

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<tbody>
<tr>
<td></td>
<td>(.408)</td>
<td>(2.479)*</td>
<td>(3.077)*</td>
<td>(2.713)*</td>
<td>(1.801)</td>
<td>(2.437)*</td>
<td>(3.709)*</td>
<td>(4.984)*</td>
<td>(5.671)*</td>
<td>(6.158)*</td>
<td>(7.114)*</td>
<td>(7.506)*</td>
<td>(7.661)*</td>
</tr>
<tr>
<td>Boot. c.v.</td>
<td>2.37</td>
<td>2.22*</td>
<td>2.23*</td>
<td>2.35*</td>
<td>2.39</td>
<td>2.63</td>
<td>2.83*</td>
<td>3.05*</td>
<td>3.31*</td>
<td>3.46*</td>
<td>3.74*</td>
<td>4.11*</td>
<td>4.41*</td>
</tr>
<tr>
<td>t/sqrt(T)</td>
<td>.026</td>
<td>.181*</td>
<td>.236*</td>
<td>.303*</td>
<td>.288</td>
<td>.481*</td>
<td>.772*</td>
<td>1.059*</td>
<td>1.231*</td>
<td>1.386*</td>
<td>1.569*</td>
<td>1.692*</td>
<td>1.823*</td>
</tr>
<tr>
<td>Right-tail c.v.</td>
<td>.068</td>
<td>.110</td>
<td>.148</td>
<td>.211</td>
<td>.301</td>
<td>.363</td>
<td>.436</td>
<td>.492</td>
<td>.548</td>
<td>.607</td>
<td>.680</td>
<td>.736</td>
<td>.794</td>
</tr>
<tr>
<td>Boot. c.v.</td>
<td>.073</td>
<td>.113*</td>
<td>.170*</td>
<td>.239*</td>
<td>.339</td>
<td>.442*</td>
<td>.487*</td>
<td>.556*</td>
<td>.628*</td>
<td>.720*</td>
<td>.807*</td>
<td>.900*</td>
<td>.995*</td>
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