Discussion of:

“Term structure of risk in macrofinance models”

by Irina Zviadadze

Christian Julliard

London School of Economics

Key Idea: an "internal consistency" diagnostic

- Macrofinance models can be cast as restrictive cases of more general DGPs, so:
  1. For each model, estimate an "unrestricted" DGP of which the model is a "restricted" version.
  2. Compare the term structure of risk implied by "restricted" and "unrestricted" DGPs using Borovicka and Hansen (2014) shock elasticities (i.e. the term structure of risk)

Note: Given the models considered, the paper extends BH to non-normal shocks, and *jumps* in particular using:

$$z_{t+1} = \mu_{1,z} + \mu_{2,z}^{1/2} \varepsilon_{z,t+1}, \quad \varepsilon_{z,t+1} | \mathcal{I}_t \sim N(0,1)$$

where $\mu_{j,z}$ is the j-th moment centered around the mean.

But (Laplace) a 2nd order approximation of the log pdf of z gives:

$$p(z) \propto \exp \left[\left. \frac{\partial \log p(z)}{\partial z} \right|_{\bar{z}} (z - \bar{z}) - \frac{1}{2} \left(- \left. \frac{\partial^2 \log p(z)}{\partial z^2} \right|_{\bar{z}} \right) (z - \bar{z})^2 \right]$$

$\Rightarrow$ Gaussian iff 1st term is zero $\rightarrow$ use the mode for centering $\mu_{j,z}$

But, is an "internally consistent" model a better model?

Not necessarily!

Why? The unrestricted DGPs are model specific $\rightarrow$ a model can be "internally consistent" with a DGP that fits the data terribly.

$\Rightarrow$ report the posterior probabilities of the 3 unrestricted models considered $\rightarrow$ a structural model that is consistent with a zero probability DGP is not a good model.

E.g. if the best fitting unrestricted model is the one used for Bansal and Yaron (2004), then the restricted model that fits the data "better" is Wachter (20013)

The issue: the unrestricted DGPs consider are not nested.

The fix: Use Bayesian Model Averaging to have a *unique* "unrestricted" model to which the restricted ones are compared.

Empirical findings

- Bansal and Yaron (2004), LRR+SV: wrong level and slope of the term structure of risk.

- Wachter (2013) disaster model: ("nomina sunt consequentia rerum")
  1. Disaster states are stock markets booms!
  2. wrong level for the term structure of risk

Aside: part of a literature that hinges upon a wrong calibration. It's not "dark matter:" the data (and G.E. theory) overwhelmingly rejects this hypothesis → let's move on.

- a model à la Drechsler and Yaron (2011), no LRR but SV with stochastic mean reversion point and jumps:
  - downward sloping term structure of risk with a lot of action in the stochastic mean of SV.

a model à la Drechsler and Yaron 2011 cont'd

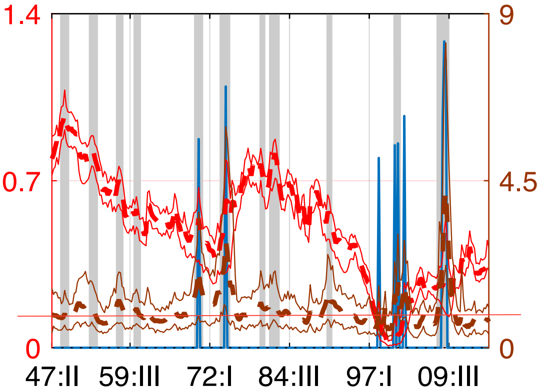


Figure: SV (brown), its "mean" (red), and jumps (blue).

- does not look like one can reject constant vol... test it!
- and the estimates are based on a *constant* expected consumption growth... more on this shortly.

On mean and variances

Recall: volatility clustering can be assessed by analysing, and is identified in an ML by, the serial correlation of the squared one-step ahead forecast errors (e.g. Engle (1982))

- Suppose consumption growth is predictable and consider its Wald $MA(\infty)$ representation with unconditional mean μ_c, parameters ρ and innovations f

$\rightarrow$ the k-th autocorrelation of $(\Delta c_{t,t+1} - \mu_c)^2$ is proportional to

$$Cov\left(\left(\sum_{j=k}^{\infty}\rho_j f_{t-j}\right)^2 ; \left(\sum_{j=k}^{\infty}\rho_{j-k} f_{t-j}\right)^2\right) \neq 0.$$

$\therefore$ if there is predictability in consumption, but one erroneously assumes constant conditional means, then mechanically one will find spurious evidence of time varying volatility.

Generally: wrong Vol process $\rightarrow$ still consistent mean process (e.g. QMLE)
wrong mean process $\rightarrow$ all estimates are not consistent

Is consumption growth predictable?

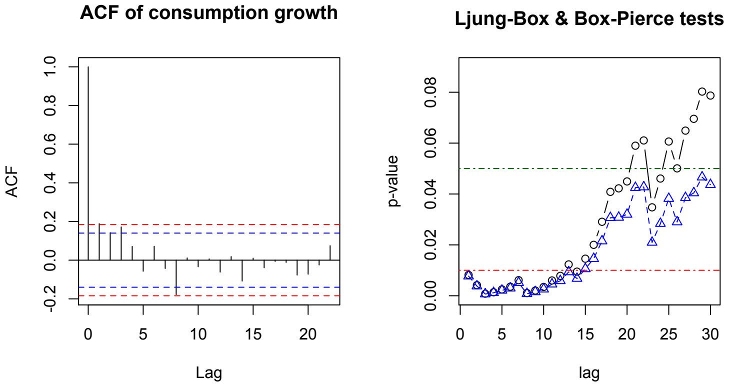
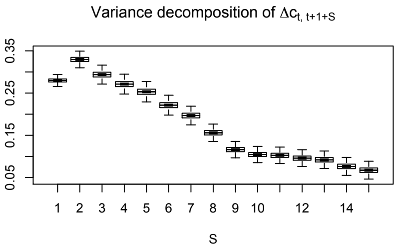


Figure: Autocorrelation structure of quarterly consumption growth. Left panel: autocorrelation function with 95% and 99% confidence bands. Right panel: $p-$values of LB (triangles) and BP (circles) tests.

But: if the predictability is orthogonal to asset returns, e.g. measurement error, it is possibly less of an issue here.

Is consumption growth predictable using asset returns?



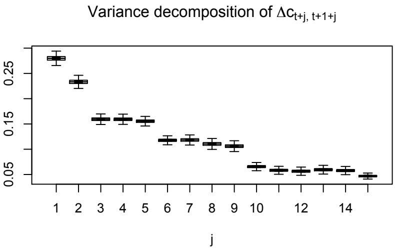


Figure: Box-plots of percentage of time series variances explained by innovations to asset returns.

Bryzgalova-Julliard (2016): more than a quarter of consumption growth time variation is explained by asset returns innovations, and this commonality prices stocks and bonds jointly.

Is consumption Vol autocorrelated?

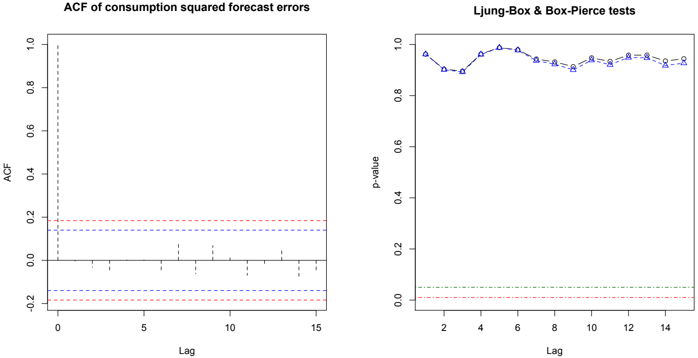


Figure: Autocorrelation structure of consumption growth squared forecast errors. Left panel: acf of $\left(\Delta c_{t,t+1} - \widehat{\mathbb{E}}_t\left[\Delta c_{t,t+1}\right]\right)^2$, where $\widehat{\mathbb{E}}_t$ denotes *MA* based forecasts, with 95% and 99% confidence bands. Right panel: *p*-values of LB (triangles) and BP (circles) tests.

A spurious link between Δc Vol and asset returns?

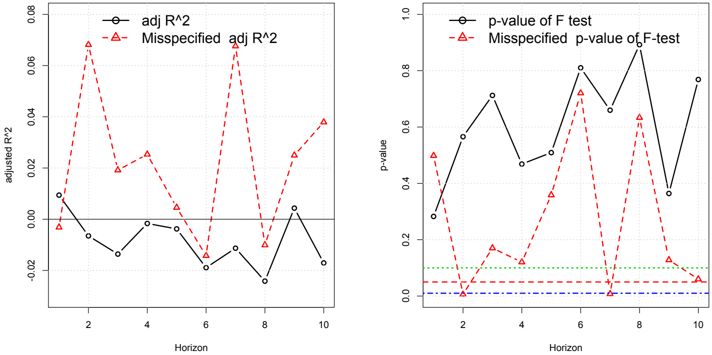


Figure: Predictability of consumption squared forecast errors on the first eight principal components of asset returns at several horizons.

- … not too unlikely …

Baseline

- A good idea, and a worthy extension of Borovicka and Hansen (2014).
- But, the paper is still "rough" and I would suggest to:
  1. center non-gaussian shocks at the mode in computing elasticities
  2. use a unique "unrestricted" DGP using BMA
  3. take mean process modelling seriously, or at least put back LRR in the model à la Drechsler and Yaron (2011)