I found this a clear, concise and coherent paper. Let me begin by outlining those aspects of the paper that I liked most. First, Hau has compiled a very nice dataset – as good, I would say, as any I have seen in this area. He uses this dataset to conduct some careful empirical analysis, using just enough econometric firepower to give us confidence in the results (but not too much to make us suspicious of them).

Second, Hau uses a clean and rather ingenious identifying restriction to get at the effects of a transactions tax on volatility – namely, tick size. The benefit of this type of restriction is that it can be applied cross-sectionally across stocks. It does not require us to make heroic conditioning assumptions across time-periods or across markets in different countries, as is the case in much of the rest of the literature. This also gives the tests more degrees of freedom and hence power.

Third, the paper reaches clean conclusions: an increase in tick-size reduces volatility in a statistically, if not economically, significant way. This result is not only clean, but novel. Table 1 compares the results of a selection of papers that have considered the effects of transactions taxes on volatility, usually using time-series data. In all of these cases, the authors have either found a statisically insignificant or a positive effect effect of transactions taxes on volatility. Against that backdrop, this paper's finding of a negative relationship is striking.

Finally, Hau's paper addresses an important public policy issue from a market *micro*structure perspective – namely, the effects of tick-size rules on market dynamics. As I discuss below, I think the paper has rather less to say about the wider *macro*economic debate on Tobin taxes.

Table 1: Volatility Effects of Transactions Taxes

Author	**Market**	**Sign of Effect**
Lindgren (1994)	12 stock markets	Positive
Hu (1998)	HK, Japan, Korea, Taiwan	Positive
Umlauf (1993)	Sweden	Positive/Zero
Saporta and Kan (1997)	UK	Zero
Green, Maggioni and Murinde (2000)	UK	Positive
Aitken and Swann (1998)	Australia	Positive
Hau (2001)	France	Negative

My comments or concerns on the paper are on two related themes. First, can we dig down to better understand the source of the volatility effect unearthed in the paper? Second, to what extent is the paper informative about transaction or Tobin tax type issues? I take these in turn.

Decomposing the Volatility Effect

Theoretical models point to two channels through which an increase in tick size could plausibly reduce market volatility. First, there is a transaction cost effect. The tick size places a lower bound on the bid-ask spread and thereby raises the cost of a round-trip transaction. So tick-size acts like a tax, reducing demand and hence transactions in the market – a "transactions" effect. If, as a consequence, speculators or noise traders are forced out of the market, then there is the scope for volatility to be reduced. It is clear, however, that this effect may not all go one way. If smart money or liquidity traders are also driven from the market, then the volatility effect of the tax is ambiguous (Schwert and Seguin (1993)).

Second, there is an insurance or profitability effect operating on the supply side of the market. Larger bid-ask spreads increase market-maker profitability. They also serve as a buffering device for market-makers if they are faced by a sharp price movement – an "insurance" effect. This also has the potential to reduce volatility, by increasing the absorbtive capacity and the degree of liquidity provision in the market (Harris (1996)).

Both of these channels imply that a rise in the transactions tax would damp *volatility*. But the two channels have opposing implications for market *liquidity*. This suggests that liquidity could be used as an additional identifying restriction, thereby allowing us to disentangle the "transactions" and "insurance" channels and hence better understand the source of the negative volatility effect. The paper makes clear that the necessary transactions data are available. So this additional test would be straightforward to conduct. There is of course already a fairly substantial literature on the effect of transactions taxes on trading volumes (see, for example, Schwert and Seguin (1993)). It tends to suggest fairly large transaction tax elasticities, in the range {0.5,1.5}.

Implications for Tobin Taxes

There are a number of reasons why the read-across from these market micro-structure results to the macro-economic debate on Tobin taxes is imprecise and potentially misleading. First, and most obviously, the Tobin tax is typically proposed as a levy on foreign exchange rather than equity market transactions.

Second, the tick size/transactions tax analogy has its limits. Transactions taxes do not have the "insurance" effect on market dynamics associated with tick size. So the empirical results in the paper provide, as the author acknowledges, an upper bound on the size of the negative volatility effect of a transactions tax. It would be possible to assess how much of an upper bound these results represented if the volatility decomposition outlined above were conducted. The net effect of transactions taxes on volatility could even be *positive*, once the insurance effect is controlled for.

Third, the sample results are drawn from a low-volatility regime. At times of small price movement, transactions taxes or tick size constraints are likely to bind on market participants' behaviour. But this is much less likely to be the case in stressed market situations, when price movements are sharp. As the Tobin tax is designed precisely for those stressed situations, it is unclear that the empirical results in this paper are informative about the efficacy of such a tax.

Finally, the paper only considers the risk dimension to transactions taxes. A full welfare analysis would also need to assess the return dimension. A rise in transactions taxes would be expected to lower mean returns to equity holdings, by an amount equal to the net present value of the future cost of the transactions tax. The existing literature points towards a significantly depressing effect of transactions taxes on mean returns (Amihud and Mendelson (1993)). This effect on mean returns can be translated into a direct measure of welfare - a Harberger triangle (see, for example, London Economics (1999)).

This welfare measure is (approximately) linear in the transactions tax elasticity. Since both this elasticity, and the implied change in mean returns, are typically found to be large and significant in the literature, the implication would be that the associated welfare cost of levying the tax would also be significant. When assessing the public policy implications of the Tobin tax, this negative effect on welfare would need to be weighed against the positive effect of reduced volatility. On the basis of the careful empirical results in the paper, this volatility effect is very unlikely to offset some of the negative externalities of a transactions tax on mean returns. In other words, the Tobin tax looks like a bad deal for aggregate welfare.

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