

Assessing Australian Interchange Regulation: Comments on Chang, Evans and Garcia Swartz

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Abstract

In this short comment, I provide my views on “The Effect of Regulatory Intervention in Two-Sided Markets: An Assessment of Interchange-Fee Capping in Australia” (published in this issue) that was presented at the *Antitrust Activity in Card-Based Payment Systems: Causes and Consequences* conference.

1 Summary

Howard Chang, David Evans, and Daniel Garcia Swartz (2005; hereafter CEG) look back at the effects of the Reserve Bank of Australia (RBA)’s 2003 intervention in four-party credit card interchange fees. The RBA argued that relatively high credit card interchange (passed through to consumers in rebates, interest-free float, and the absence of transaction charges) encouraged such overuse by cardholders. The RBA thus intervened to reduce four-party credit card interchange fees. To gauge the effect of this intervention, CEG look at evidence regarding *quantities* (how much Australians use credit cards), and regarding usage *prices* for credit cardholders.

On the quantity side, CEG do not find econometrically that growth rates in credit card usage took a sharp downturn with the regulation. In fact, they proffer spline regressions that suggest that growth rates *increased* relative to an estimated but-for world. Moreover, they argue that the marginal usage price facing credit cardholders had not (yet) increased in response to lower interchange. They thus suggest that the regulation has not had its intended effects. They offer this interpretation cautiously and guardedly, but not, I think, cautiously and guardedly enough. In my view, what we learn is that the evidence is just not yet in.

Through no fault of their own, CEG work with a small, trendy, noisy, data set. It has low power: even if the regulation had the intended effects, you would not necessarily expect to find statistically significant evidence in these data. I’ll say more below about the dataset, why it seems low-powered, and how one should respond to that. When CEG impose enough structure so that the data seem to say something, they find that the RBA’s reforms had the perverse effect

* Mailing address: University of California, Berkeley, Department of Economics, 549 Evans Hall #3880 Berkeley, CA 94720-3880, Phone: (510) 642-9854, E-mail: farrell@econ.berkeley.edu. I have twice testified on behalf of the Reserve Bank of Australia, once in the credit card interchange intervention, and again in the proposed PIN debit (EFTPOS) interchange intervention. I have no current ongoing relationship with the RBA, and I haven’t talked to them about the Chang et al. paper or my comments on it.

of *increasing* growth rates of credit card usage. This, and in particular the estimated effects in number of accounts and usage per account, buttress my suspicion that the data are being too harshly interrogated, if not tortured.

CEG also tentatively find that cardholder marginal usage fees haven't changed much, though annual fees have. They also argue that economic theory suggests this outcome because in Australia both banking and retailing are quite concentrated. I will argue that (even assuming concentration here indicates market power) economic theory *does not* suggest – though it is consistent with – a failure to pass through price changes facing issuing banks or retailers, and also that one wouldn't necessarily expect to see the full issuer pass-through effect in the marginal usage price for credit cards.

2 The Quantity Data and Interpretation

One would naturally frame the problem as follows: there would be some level of usage of credit cards before the regulatory intervention, and some level of usage after the regulatory intervention; the econometric goal would be to see how those *levels* differ.

But the data are unfriendly to that econometric approach: all the time series have strong upward trends. CEG briefly observe that a naïve levels test would suggest that the regulation increased credit card use, but recognize that the continuing trends would make this interpretation silly, and accordingly focus on whether the regulation affected *growth rates*. Intuitively, that seems sensible, but of course it distances the econometrics from the question of interest. Why are these data so trendy? The answer may well be that the market is adjusting gradually toward some equilibrium or “target” level. If it has an effect, the regulation presumably moves that target. If you weren't yet close to the old target, and you don't quickly approach the new target, picking up the shift in target levels in the data may be hard and/or demand strong assumptions. One would certainly expect that interrupting convergence toward target x and beginning convergence toward target (say) $0.9x$ would show up in a lower growth rate, but it's not clear how much, so it's not clear (quantitatively) what to look for in the growth statistics. Thus this sensible-seeming shift from levels to growth rates exacerbates any problem of low power; inability to find statistically significant changes in growth rate is *not* the right test.

The growth statistics are summarized in CEG's Table 4. An unsophisticated booster might say that Table 4 shows that the RBA's intervention slowed credit card growth. In columns 2 (purchase transactions), 3 (purchase transactions per account), 4 (real purchase volume), and 5 (real purchase volume per account), the 2003 and 2004 growth rates are well below the 1996-2002 averages. Since the regulation took effect late in 2003, it is not entirely clear how to count the 2003 data, and in each of these columns the 2004 growth rate is below the 2003 rate, which itself is *well* below the 1996-2002 average. Thus, *if* one can work in growth rates (see above), and *if* one can learn much from Table 4 (see below), one could argue that the data support the booster. As CEG note (p.22), “[t]he aggregate data suggest that (1) industry growth was lower after regulation than before, with the exception of [column 1:] the number of accounts...”

For somewhat different reasons, both CEG and I decline to be that booster. After noting observation (1), CEG state that “(2) the decline in the rates of growth that started around 1999 has continued ... but has not accelerated.” Citing their “prior,” they prefer the interpretation, stressing (2), that an unidentified force slowed credit card growth beginning around 1999, rather than stressing (1). Clearly there is something to this, but it is not completely satisfactory, since

the 2003 and especially the 2004 growth rates *are* noticeably lower (except in column 1) than the 1999-2002 rates, so “but has not accelerated” isn’t what the data directly say. Rather, I think, it emerges from their spline regressions. Inevitably, that is driven by something about the *shape* of the *slowdown in growth rates* – are we up to fourth derivatives yet? – during 1999-2002 and by the way in which the splines extrapolate that slowdown to the but-for world post-2003. I’m not saying it’s a sin to override the data’s direct message with an extrapolation from splines based on an unexplained feature of the data – but it does call for plenty of caution.

That brings me to *my* reason for not being the booster. It seems to me that the data summarized in Table 4 are sparse and noisy (even recognizing that the underlying data are quarterly). Most of 2003 was pre-regulation, so there is *very* little post-regulation data, and the pre-regulation data has a lot of unexplained variation. CEG stress that growth rates fell starting around 1999, but there is plenty of unexplained variance not captured by that description either. Others might explore this with more econometric firepower, but as a consumer of empirical IO, I would think the dataset simply lacks statistical power to do what CEG ask it to do.

This needn’t be a matter of opinion. To test the hypothesis that the RBA’s intervention had an economically significant effect, classical statistics would tell us first to specify what that would mean. Somewhat arbitrarily, above, I for-instanced a ten percent change in steady-state credit card usage; one could use a different threshold. Then we see, *not* whether the data *fail to reject* the hypothesis of no (or perverse) effect, but whether the data *reject* the hypothesis of *at least* such an effect. In that way, if the data have low power, we will notice because we will find it difficult to draw conclusions. Bearing in mind the uncertainty about how the change in steady state would translate into changes in growth rates, I can’t look at Table 4 and believe that one can confidently reject the hypothesis that there was an effect such as the RBA contemplated. CEG could be read to do so, but only by imposing a spline that I think ends up doing too much work. I do agree with CEG that one can’t just point to the lower growth rates and say “see: intervention worked,” because of the slowdown in 1999-2002 (or -2003), but it puts a lot of weight on the splines to say (Table 5), that growth rates picked up in 2003-2004 relative to what extrapolating the pattern of previous slowdown would suggest. In short, I think the data just don’t tell us whether the RBA’s intervention did much to slow credit and charge card growth. They don’t tell us voluntarily, and although torture can make them talk more, it can’t necessarily extract more truth.

CEG recognize this at two levels. First, they repeatedly acknowledge throughout the paper that it may just be too soon to tell.¹ Unfortunately, they mingle this with repeated shorthand that might be read to suggest the opposite. Second, despite Table 4a’s finding of statistical significance, CEG stop short of inferring that the regulation really *increased* (growth rates of) credit card use overall – in other words, they can’t take it too seriously either. This is particularly true when we set it next to their findings on consumer pricing of credit cards, as I’ll discuss in a minute, after one more point about the quantity data.

CEG’s quantity data (I think) mix the regulated, four-party “credit cards” (Visa, MasterCard, and BankCard) with the unregulated “charge cards” (notably American Express and Diners Club). The change in interchange applied only to the former. While that was over 80% of the total, CEG cite RBA figures suggesting that substantial volume (over 3% of the total in 18

¹ In addition to data limitations, they observe that since the regulations were recent, we can only observe a short-run effect. Stickiness that would make short-run and long-run effects differ would also make it sensible for banks to “pre-respond” to the regulation, which had been hinted at for several years, and CEG cite evidence that they did. Thus some unknown share of pre-regulation dynamics should be attributed to the regulation.

months) shifted from credit to charge. CEG rationalize looking at the aggregate because of a “regulatory desire to reduce the use of cards.” I’d like to see future analyses separate out the questions (a) did the intervention substantially reduce the use of four-party credit cards, as the RBA’s theory would suggest, and (b) where did that purchase volume go? If a hefty shift was mostly from credit cards to charge cards, the regulation could have been counterproductive. But I’d like to see this clearly distinguished from the very different (though also critical of the regulation) theory that there just wasn’t much of a shift.

3 What Happened to Prices?

Broadly, CEG examine two consumer-level prices: the generally positive fixed fees for holding a credit card, or one that gives rewards, and the often (for transactors) negative variable fees for using one’s card. Recognizing that long-run effects could well be stronger than short-run effects, they find that fixed fees have risen substantially and that variable fees haven’t changed much yet. In a single-product analysis, this would suggest that the number of cardholders, or cards held, would fall but (loosely) that usage per card wouldn’t change. This isn’t what Table 4, Table 4a, or Table 5 says. Thus, while CEG argue that both the quantity analysis and the price analysis suggest that the intervention was ineffectual, it seems to me that the price analysis makes it even harder to take the quantity analysis at face value.

What should we make of the price analysis in itself? I’ll focus on two issues: which prices to look at, and the role of competition in pass-through.

CEG focus on consumer prices of credit cards. But surely substitution among credit, charge, and debit is more important than the effects of interchange on total commerce. That is, the most relevant prices for credit card use are not the absolute level of credit card fees, but their level relative to debit and charge card fees. If, as I understand, Australian PIN debit card (EFTPOS) fees have fallen in the last few years, wouldn’t that shift volume from credit to debit, just as the RBA wanted?

Now, you might think that’s a cheat: the intervention was in credit cards, so why bring in changes in the EFTPOS market? Even if true, doesn’t it sound like sheer dumb luck? No, and for a reason that CEG recognize in another context. Unquestionably the prices *to issuers* of inducing their customers to use credit versus alternatives changed. And issuers can affect credit card use through many prices and non-price means, notably the prices and availability of substitutes. CEG stress that issuers responded to these incentives by starting to issue American Express; why not also look at whether issuers responded by making debit cheaper? This argument is stronger when an issuer is also a cardholder’s regular bank, but the concentration of the Australian banking industry would suggest that’s not unusual.

Finally, CEG argue that concentration in Australian banking and retailing suggest that pass-through will be quite limited – especially short-run pass-through of cost reductions. I won’t go into whether concentration indicates market power here. CEG say that perfect competition would imply 100% pass-through, which is correct if supply is infinitely elastic. They correctly note that without perfect competition, both less than 100% and more than 100% pass-through are theoretically possible, and that empirical studies have found both cases. But they seem to view more than 100% pass-through as basically an unlikely theoretical possibility. I don’t think that’s right.

On the theory side, CEG argue that for small changes, one can assume linear demand, in which case pass-through is 50%. Unfortunately this is simply an error: the calculus-inspired technique of approximating demand as linear for small changes relies on a (sometimes unstated) lemma that when you work the full problem, the second derivative (curvature of the demand function) appears *only* multiplied by second-order effects, $(\Delta c)^2$ or higher, which become small relative to terms involving the first derivative times Δc , as $\Delta c \rightarrow 0$. For instance, to predict the change in demand resulting from a price change, the intermediate-value theorem gives us $\Delta Q = Q'(p)\Delta p + \frac{1}{2}Q''(t)[\Delta p]^2$ for some t between p and $p + \Delta p$, and as $\Delta p \rightarrow 0$ the second term becomes arbitrarily small compared to the first provided that $Q'(p) \neq 0$ and the second derivative is continuous; thus for that problem we *can* take demand as linear for small price changes. Theory tells us clearly that no such lemma holds for pass-through: the second derivative of demand appears with the *linear* term in Δc , so one cannot legitimately approximate by taking demand to be linear for small cost changes.

CEG also cite a number of empirical studies, arguing that pass-through of less than 100% is found more often than the opposite, and that above-100% pass-through appears mostly in tax contexts. Readers will draw their own conclusions; personally I am concerned enough about publication bias to be wary of such a meta-analysis. I'd just stress that, as CEG recognize, pass-through of idiosyncratic (firm-specific) cost changes is quite different from pass-through of "industry-wide" changes; tax changes may be more likely to be the latter. I think there are subtle issues about where in that spectrum a cost change for issuing credit, but not debit or charge, cards would fall.

4 What if CEG are Right?

I've described my reasons for scepticism, but what if the authors are right that customer-side demand is unresponsive to changes in interchange, perhaps because issuers don't pass it through in a way that affects usage? Is merchant-side demand also unresponsive? If so, that suggests a lot of market power (perhaps largely unexploited, so far, because of the four-party structure). On the other hand, if merchant side demand is more elastic, then it would seem the association is choosing a two-sided pricing structure that oddly loads most of the fees on the elastic side of the market.

5 Conclusion

The authors have taken on a valuable and difficult challenge. Read correctly, their paper helps illuminate what the data show and don't show. As I've explained, I think the correct reading is that so far the data don't show much. I hope both the RBA and the card associations and banks will provide more and better data. Ex post evaluation of policy interventions is a good idea, we should do more of it, and I look forward to seeing this one done right when better data are available.