Technical annex

Supplement to CP18/38

December 2018

Contents

Details on expected benefits of leverage limits

1. This technical annex sets out the details of assessment of the costs and benefits of leverage limits for consumers summarised in the CBA. We discuss:
   - our data request and data preparation;
   - the methodology used; and
   - the results.

Data request and preparation

2. To collect the data we sent an information request to 5 firms requesting data on all trades from the full population of retail traders over the period from 18 August 2014 to 30 June 2015, because in this period one UK CFD firm introduced lower leverage limits for a wide range of CFDs.[1] We requested detailed information on individual trades as well as on deposits and withdrawals, accounts and customer characteristics (non-attributable to individuals). Additionally, we requested information on products which included the standard leverage limits by volume tier and periods when these were applicable.

3. We discussed and resolved apparent data errors with firms and excluded trades as likely affected by a data error if:
   - the asset price or trade consideration at open is equal to zero;
   - the opening and closing identifiers for several trades are identical; and
   - the transaction costs at the opening or closing of the trade are recorded as greater than zero. (We requested costs to be reported as negative.)

   We also excluded trades affected by a corporate event, such as a merger.

4. We also omitted a very small percentage of trades with very large or very small net percentage P&L to avoid that untypically large or small observations have a large impact on our results. We considered the distribution of the net percentage P&L and excluded the top and bottom 0.05% for four firms and the top and bottom 1% for one firm with a much greater range of the net percentage P&L. In doing so, we still consider more than 98% of all trades not classified as likely affected by such an error.

5. Many firms apply different leverage limits to different tiers of transaction volumes for the same CFD and may have changed either the thresholds of the volume tiers or the leverage limits for given tiers in the period we considered. We therefore considered both changes to the volume thresholds and to the leverage limits for a given CFD as a change to the leverage limits.

6. We compared CFDs for which one UK CFD firm had introduced leverage limits with similar CFDs offered by other firms which had not changed leverage limits on those products in the period we consider. We considered CFDs to be similar if they are based on the same underlying asset. In most cases, we could identify CFDs based on the same underlying products using public identifiers. In addition, we manually compared CFDs offered by other firms with the treated products using product characteristics obtained based on Reuters Instrument Code (RICs). We only matched futures with

[1] The request included information requested for an analysis of the margin close out rule, which was subsequently replaced by the analysis presented here.

futures and options with options. We also matched CFDs only with underlying products in the same currency.

7. Tables 1 and 2 below show summary statistics for the variables used in the difference in difference regressions. We include the indicator for the Swiss franc (CHF) de-pegging because we use it to account for the effect of this event on consumer outcomes in our key regressions. The values shown are due to the fact that it takes only values of 0 or 1.

Table 1: **Summary statistics for treated group**

Variable	mean	median	sd	1-percentile	99-percentile
Value at open, £	1,251,687	114,163	6,803,011	1,618	25,856,778
Net P&L in %	-0.1%	0.0%	1.3%	-4.5%	3.1%
CHF event	0.52	1.00	0.50	0.00	1.00

Notes: sd: standard error

Table 2: **Summary statistics for control group**

variable	mean	median	sd	1-percentile	99-percentile
Value at open, £	670,181	46,205	5,098,094	71	10,932,205
Net P&L in %	0.0%	0.0%	0.8%	-2.7%	2.0%
CHF event	0.57	1.00	0.49	0.00	1.00

Notes: sd: standard deviation

Methodology

8. We considered the introduction of lower leverage limits by one UK CFD firm (the Firm) on 21 January 2015 for a wide range of CFDs based on 52 underlying assets. Table 3 below shows the changes in leverage limits on 21 January 2015 and the leverage limits proposed.

Table 3: **Leverage limits of the Firm and leverage limits proposed under the policy**

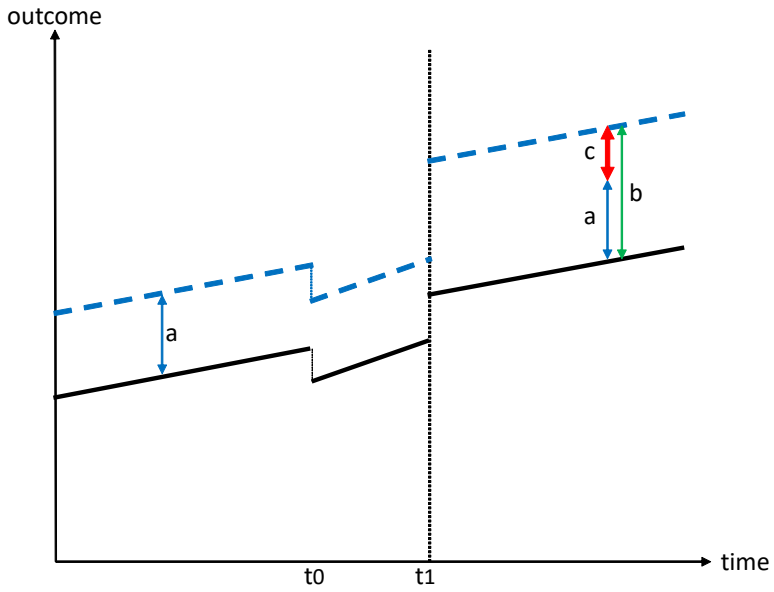
Limit pre 21/01/15	Limit post 21/01/15	Limit proposed by us	Number of products
100:1	10:1	10:1	3
100:1	50:1	20:1	15
100:1	50:1	30:1	5
100:1	33:1	30:1	3
100:1	20:1	20:1	1
50:1	33:1	20:1	5
25:1	20:1	20:1	3
25:1	10:1	10:1	7
17:1	13:1	20:1	4
13:1	7:1	20:1	1
13:1	7:1	30:1	5

9. We compared the differences in outcomes for products subject to this change in leverage limits (the treatment group, TG) and with those on products with unchanged

leverage limits offered by other firms (the control group, CG) before and after the change in leverage limits, ie $\Delta P\&L = \left(P\&L^{TG}{}_{after} - P\&L^{TG}{}_{before}\right) - \left(P\&L^{CG}{}_{after} - P\&L^{CG}{}_{before}\right)$. Other contemporaneous changes, such as the Swiss franc de-pegging[2], will likely affect the treatment and control group in a very similar way. Effects of such contemporaneous changes hence likely 'net out' in the comparison of treatment and control group. The key assumption for the interpretation of $\Delta P\&L$ as the effect of the lowering of leverage limits is that the trend in outcomes for the both groups would have been very similar in absence of it ('common trend' assumption).[3]

10. The hypothetical example for a 'crisis' in t_0 and an intervention in t_1 in Figure 1 illustrates this approach. The difference between treatment group (dashed lines) and control group (solid lines) is *a* before the intervention and *b* after the intervention. The difference-in-difference illustrating the impact of the treatment is hence *c (=b-a)*.

Figure 1: **Hypothetical example to illustrate the difference in difference approach**



11. To implement the difference-in-difference analysis we estimate the following equation

$$Y_{t,a,p} = \alpha_0 + \alpha_1 * did_{t,a,p} + \alpha_2 * CHF\ event + \boldsymbol{\beta} * week_t + \boldsymbol{\gamma} * account_a + \boldsymbol{\delta} * product_p + \varepsilon_{t,a,p}$$

where

t = time (week)

p = product (defined based on the underlying, eg 'Gold' for all CFDs related to gold)

a = account

$Y_{t,a,p}$ = outcome (profit or loss / value of trades opened / spread)

$did_{t,a,p}$ = indicator for trades with the Firm after 21 January 2015

$CHF\ event$= indicator for the period after 15 January 2015

$week$ = indicators for each week, defined as 7-day period starting 19 Nov 2014

$account$ = indicators for each account

[2] The Swiss central bank announced on 15 January 2015 that it would no longer hold the Swiss franc at a fixed exchange rate with the Euro causing its value to soar and the Swiss stock market to collapse.

[3] This analysis does not account for the effect of other policy measures which are part of our proposals. Introducing higher margin requirements may have different effects if other policy elements are introduced at the same time. For example, it is possible that consumers will be more willing to invest in CFDs if they can be sure that they cannot lose more than that (because of no-loss accounts). It cannot reflect firms' responses to the introduction of the proposed leverage limits, such as encouraging some consumers to trade as professionals.

$product$ = indicators for each product

For each group of indicators we omit the first entry as reference category. For example, for the variable *week* we omit the indicator for the first week in the period considered.

12. To ensure that the results are robust, we considered only trades in CFDs affected by the change in leverage limits and similar CFDs offered by other firms not affected by these changes (CFDs based on the same underlying asset). We consider the period from 19 November 2014 to 17 March 2015 to exclude a number of changes in margin requirements by other firms before and after that period. Since it is not clear whether trades opened on 21 January 2015 are affected by the increase in margin requirements we omitted those trades in our regressions (except for some regressions run as robustness checks). The data is aggregated by account, underlying product and week.

13. We tested the validity of our methodology in two ways. Firstly, we tested whether it shows a spurious effect of fake interventions prior to the actual lowering of leverage limits by the Firm ('placebo test'). Secondly, we tested whether the outcomes before the Firm lowered leverage limits developed in the same way for the treatment group and the control group ('common trend check').

14. To implement the common trend check we estimate the following equation

$$Y_{t,a,p} = \alpha_0 + \alpha_1 * CHF\ crisis + \boldsymbol{\beta} * swk_t + \boldsymbol{\gamma} * week_t + \boldsymbol{\delta} * account_a + \boldsymbol{\tau} * product_p + \varepsilon_{t,a,p}$$

Where variables are defined as for the difference in difference analysis and

swk = indicators for each of the weeks for trades with the Firm only

If the common trend assumption holds the indicators for the weeks for trades with the Firm prior to the intervention should be individually and jointly not statistically significant.[4]

Results – effect on bid-ask spreads

15. There is a risk that transaction costs for consumers will increase following the introduction of leverage limits. US academics Heimer and Simsek have analysed a policy that included lowering of leverage limits on CFDs sold to retail investors. Based on a theoretical model they suggest that such a policy reduces trading if the traders remaining in the market are on average better informed. In this case, firms have an incentive to widen their bid-ask spreads to mitigate the losses incurred from the change in composition of their customer base.

16. We used the difference in difference methodology and compared the changes in bid-ask spreads at open and at close before and after the Firm tightened the leverage limits for the treated group and the control group. The spread is defined as $(p_B - p_A)/((p_B + p_A) * 0.5)]$, with *B* for bid and *A* for ask. To ensure the robustness of our results we considered both the simple average of the spreads and the average weighted by the value of the trades at open. Other types of transaction costs are considered in our analysis of retail traders' profits and losses below.

17. The results suggest that this intervention has not had an effect on spreads at open or close. This holds whether we use simple weekly averages or averages weighted by the value at open. Detailed results are omitted.[5] Investors are hence unlikely to have paid larger spreads after the Firm tightened the leverage limits.

[4] For example, see Heimer, R. Z., & Simsek, A. (2017) or Autor (2003).

[5] The effects are statistically not different from zero. The results show a very small positive effect of c. 0.0000014 for the simple average of the spread at open, a small negative effect for the weighted average of the spread at close of c. -0.0000017 and a very small negative effect for the unweighted spread at close of -0.0000005 if all data is used. The

Results – net P&L in percent and trading activity

18. We use the total profit or loss net of transaction costs in percent (of the total value of trades at open) in each week as a measure for the P&L, and the total value of trades opened in each week as a measure of the trading activity.

19. Our analysis suggests that the decrease in the leverage limits considered has not had an effect on the P&L achieved.[6] [7] However, the value of CFDs traded appears to have decreased significantly (in both statistical and economic terms). Analyses for all data and analyses excluding currency pairs involving the Swiss franc also support this conclusion. Table 4 summarises these key regression results. Since we use the logarithm of the value of trades at open as the dependent variable, the results suggest a decline in the value of trades at open of c. 28% as a result of the increase in margin requirements.

20. Figure 2 illustrates the development of the log(value at open) for the Firm over the period considered. The coefficients of the indicators for each of the weeks for trades with the Firm are not statistically different from zero prior to the change in leverage limits (individually and jointly), but drop sharply after this change and stay well below zero. In weeks 15 and 16 this effect is not significantly different from zero.

21. The development of log(value at open) over time also suggests that the Firm's retail traders did not anticipate that the Firm would tighten the leverage limits due to the Swiss franc crisis (in week 9 in our data). The indicator for trades with the Firm in this week is not significantly different from zero.

simple average of the spread at open and the weighted and simple average of the spread at close fail the common trend check described above, but the difference between spreads for the treated group and the control group increases over time (for the weighted and unweighted averages); see Figure 3 at the end of this annex for an example. Any apparent increase in the spread might hence be overstated, any reduction understated. The weighted average of the spread at open does not fail the common trends check. If we cluster on product none of the measures of spread fails the common trend check and the effect of the intervention is again statistically not different from zero. Given these results we saw no need for a complex placebo test.

[6] More technically, the effect of the changes of leverage limits on profits and losses is statistically not different from zero. If we cluster on product instead the coefficient of the intervention for the net P&L in percent is significant at conventional levels. The results for the log(value at open) are significant at very similar levels of significance as in the key regressions above.

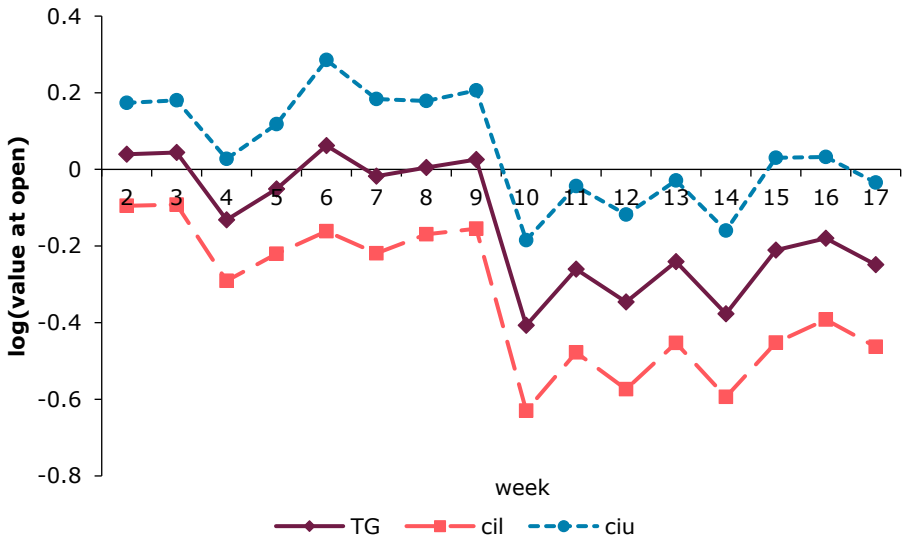
[7] This finding differs from Heimer and Simsek's who find a c. 25% increase in the return on investment (which equals the difference between the nominal value of the CFD when the position is closed (opened) and when it is opened (closed), divided by the trader's dollar stake in the trade; see Table A4 in their paper). The difference may be driven by the fact that evaluate a change in policy which tightened the leverage limits for FX pairs and required trading other types of CFDs through exchanges, while we evaluate the change implemented by one CFD firm on a range of their products. Imposition of higher margin requirements by a regulator may have a different effect because traders may interpret it as a product warning and may change their trading behaviour to reflect that.

***Table 4*: Summary of four key regression results**

outcome	P&L	P&L, excl. CHF-pairs	log(value of trades)	log(value of trades), excl. CHF-pairs
intervention	-0.0005	-0.0007*	-0.2767***	-0.2847***
standard error	0.0004	0.0004	-0.0690	0.0675
Indicator for CHF crisis	yes	yes	yes	yes
week fe	yes	yes	yes	yes
product fe	yes	yes	yes	yes
account fe	yes	yes	yes	yes
Adj. R^2	0.1538	0.1543	0.6878	0.6881
# obs.	641,839	638,985	652,743	649,789

Notes: # obs.: Number of observations; fe: fixed effects, ie indicators for each week / each product / each account; *** statistically significant at the 1% level, ** statistically significant at the 5% level, * statistically significant at the 10% level (ie not significant at the conventional 5% level).

***Figure 2:* Development of the log(value at open) for the Firm over time**



Notes: TG: treated group, ie trades with the Firm in products subject to a change in leverage limits, ciu: lower bound of the confidence interval, ciu: upper bound of the confidence interval.

22. In these key regressions, we capture the Swiss franc de-pegging through an indicator for the period after 15 January 2015. It is possible that the leverage limits were changed on 21 January 2015 because the Swiss franc de-pegging led to larger losses for traders or to a reduction in trading (and hence larger losses for the firms). The results shown in Figure 2 for week 9 suggest that there was no such difference between the Firm and other firms. This is also true for the net P&L in percent of value at open. We account for the effects of Swiss franc de-pegging by including an indicator for the period after 15 January to account for this potential reverse link.

23. If we model the effects of the Swiss franc de-pegging by including the difference between the EUR-CHF exchange rates and the average exchange rate prior to the de-pegging[8] or by the volatility of the EUR-CHF exchange rate on the first day of each week the regression results are similar. There is again no significant effect on the P&L. The value of trades at open is shown to decrease by c. 28%. This finding is consistent with the significant decrease in trading volumes of 75% (or 41% taking in to account

[8] We used a CHF to EUR value of 1.2012.

the increase in trading volumes for elective professionals) following the introduction of ESMA's temporary measures, though the more recent data around ESMA's changes does not allow us to establish whether this effect is causal. Intervention by a regulator may plausibly have a larger effect, for example, because retail traders understand it as a product warning.

Tests of key assumptions

24. For each of the four key regressions we ran 6000 placebo regressions. To do so we considered the 139 firm-product pairs in our sample and randomly drew 1200 pseudo treatment groups of 35 firm-product pairs each and assessed the effect of fake interventions affecting these in each of the weeks 3, 4, 5, 6 and 7. This placebo test does not suggest an effect on the P&L or the value traded for the pseudo treatment groups. Moreover, the true effect shown by the key regressions is very different from the placebo-effects. This test does hence not invalidate the difference-in-difference approach.

25. Figures 4 to 7 at the end of this annex illustrate the results of the placebo tests for the key regressions. The distribution of the coefficients of the fake interventions are shown as a histogram. The vertical lines to the left indicate the true coefficient for the actual lowering of leverage limits (from the key regressions). The figures show that the effects of the placebo intervention are centred on zero. The ratio of placebo tests showing an effect that is lower than the true coefficient offers an indication of whether the true coefficient is close to the placebo results. A maximum of 16 of 6,000 regressions (or 0.3%) suggest such an effect. For most of the tests, none of the regressions suggest such an effect. This test does hence not invalidate the difference-in-difference approach.

26. The common trend check for the P&L and log(value of trades at open) shows that the coefficients on the additional weekly indicators for the treated products are not individually or jointly statistically significant.[9] The same holds for the value of trades at open.[10] [11] This suggests that there is no difference between the treatment group and the control group regarding the development of the P&L or the value at open prior to 21 January 2015.

27. We cannot exclude that some consumers responded to the Firm's lowering of leverage limits by trading with another CFD firm instead. Such switching would imply that trading volumes with the Firm went down and trading volumes with other firms went up after the intervention. If switching between firms was important, our analysis may therefore overstate the decline in trading volumes due to the lowering of leverage limits (given the lower scope for switching once our measures apply).

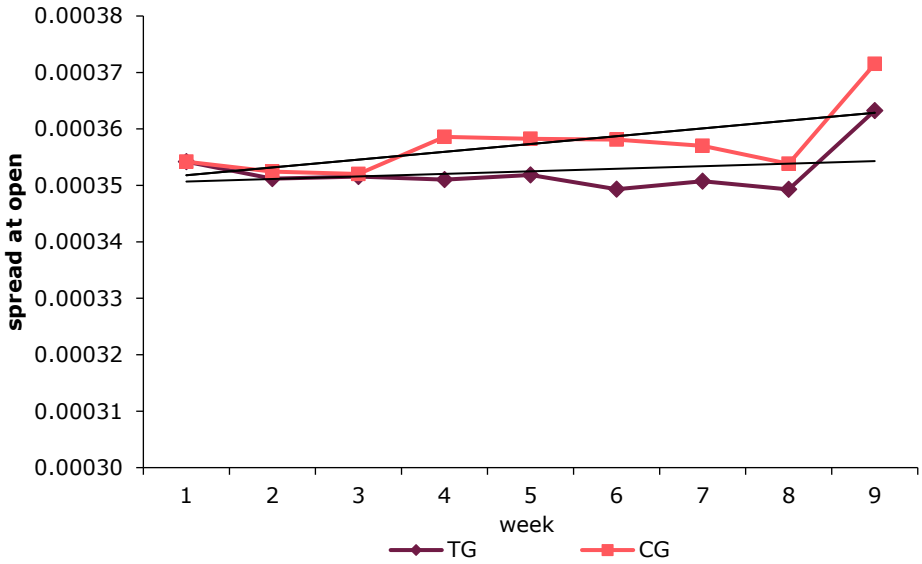
28. If the proposed policy is implemented, the scope for such switching is much smaller since consumers would have to trade as professionals or switch to a non-EU jurisdiction (assuming that countries in the EU adopt the same or a similar policy).

[9] This is also true if trades open on 21 January 2015 are included. If trades open on 21 January 2015 are included the effect shown and significance levels are very similar to those if they are excluded. The effect of the intervention is not statistically significant if CHF-pairs are included, excluding CHF-pairs it is again significant at the 10%-level.

[10] If we cluster on product instead on account, the common trend check fails for the key regressions for log(value at open), but not for the P&L in percent. A graphical analysis shows that the trend in the log(value at open) in trades with the Firm and trades with other firms diverges and that the difference increases over time. Therefore, the coefficient of the intervention might understate the reduction in trading volumes with the Firm after 21 January 2015.

[11] If we cluster on the accounts, but block bootstrap the standard errors with 400 or 800 repetitions, the results are very similar to those presented above, but the effect of the increase in margin requirements on the net P&L (of -0.0007) is statistically significant at the 5% level. Our choice of the number of 400 repetitions follows Cameron and Miller, 2015. The regressions with 800 repetitions are a further robustness test.

Figure 3: **Trends in spreads at open prior to the lowering of leverage limits**



Note: the graphs for the weighted average spread at open and omitting CHF pairs are similar.

Figure 4: **Frequency plot for the placebo test for value of trades at open (all data)**

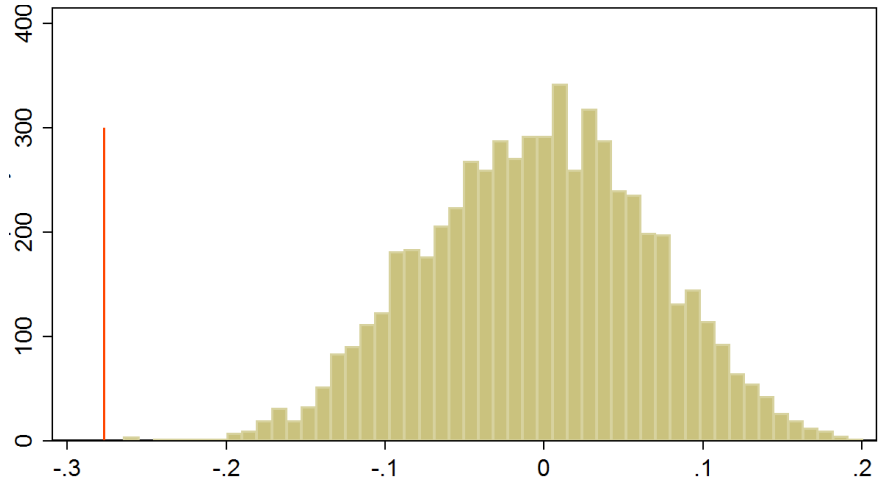


Figure 5: **Frequency plot for the placebo test for value of trades at open (excluding CHF pairs)**

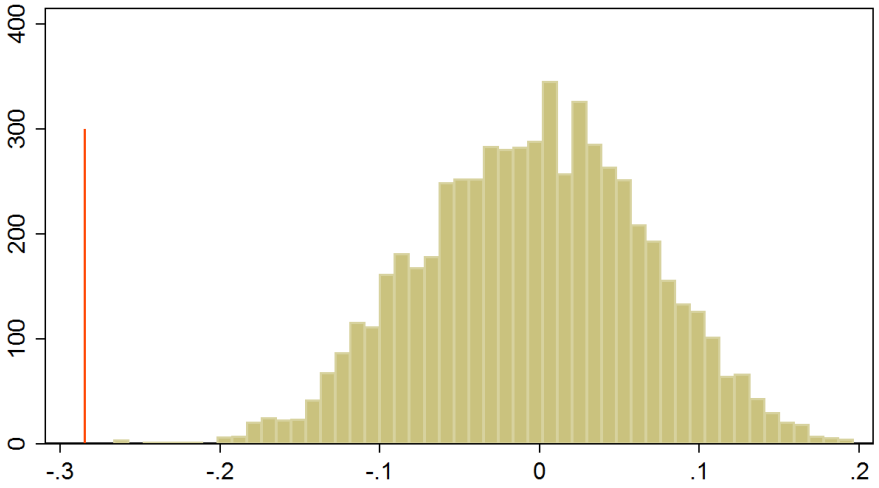


Figure 6: **Frequency plot for the placebo test for P&L (all data)**

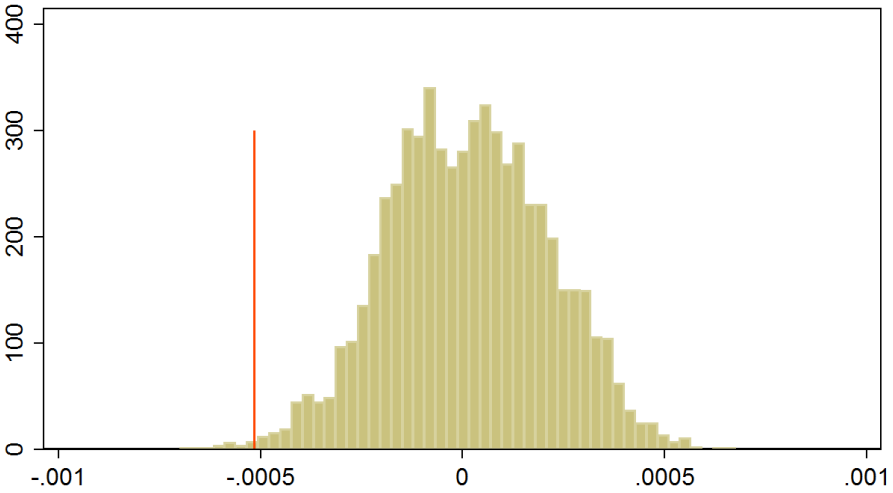
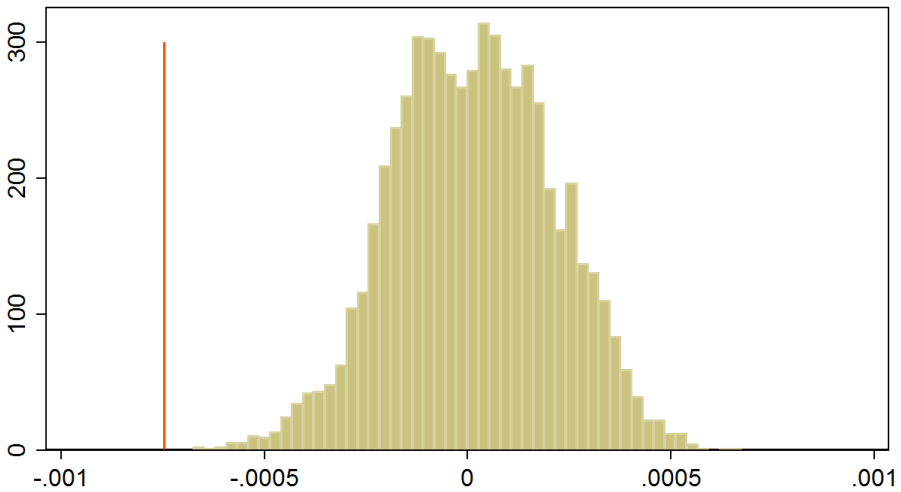


Figure 7: **Frequency plot for the placebo test for P&L (excluding CHF pairs)**



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