Realized Volatility as an Instrument to Official Intervention

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Abstract

This paper proposes a novel orthogonality condition based on realized volatility that allows instrumental variable estimation of the effects of spot intervention in foreign exchange markets. We consider parametric and nonparametric instrumental variable estimation, and propose a test based on the average treatment effect of intervention. We apply the method to a unique dataset for the BRL/USD market with full records of intervention and net order flow intermediated by the financial system. For each 1 billion USD buy (sell) intervention in the spot market there is a corresponding depreciation (appreciation) of the order of 0.50%. The estimates are a bit lower controlling for derivative operations, which suggests the intervention policies are complementary.

Keywords: realized volatility, intervention, exchange rate, order flow, instrumental variable, nonparametric.

JEL Classification: F31, C26, C54

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1. Introduction

Estimating the effect of official spot intervention on the level of the foreign exchange rate is challenging due to the simultaneity problem. Instrumental variables related to news, market expectations and the reaction function of the Central Bank have been used with mixed results (Domingues and Frankel (1993), Galati and Melick (1999), Galati et al (2005), Kearns and Rigobon (2002), Tapia and Tokman (2004)). We argue that realized volatility calculated from intraday data is an ideal instrument for intervention at a daily frequency. The argument is built from deductive reasoning based on formal properties of conditional volatility models. We apply this idea to a unique dataset for the Brazilian foreign exchange market with full records of official intervention and net order flow intermediated by the financial system. The results of standard parametric tests and novel nonparametric tests based on the average treatment effect are both consistent with effective intervention.

The intuition for the use of observed realized volatility as an instrument for intervention is straightforward. First, since excessive volatility is the most common motivation for intervention policy in foreign exchange markets, intervention activity should be correlated with realized measures of volatility. Second, suppose the error in the conditional expectation of the foreign exchange return is the product of a time varying scale factor and a standardized random variable. To the extent there is an appropriate orthogonality condition relating the scale factor and realized variance, we have the second condition for an instrumental variable.

The required orthogonality condition can be obtained by exploring some extensions of the GARCH family of models that incorporate intraday information (Hansen et.al (2011), Shephard and Sheppard (2010), Engle and Gallo (2006)). For concreteness, we motivate our proposed orthogonality condition in the context of the Realized-GARCH framework of Hansen et.al (2011). In that model, realized volatility is related to latent volatility through measurement and state equations, such that lagged realized volatility satisfies the orthogonality condition (realized variance may be used as well). In contrast, the contemporaneous realized volatility is not an instrument due to the presence of leverage effects, that is, larger increases in volatility associated with negative returns. We also show the idea is more general and applies to other realized measures and related volatility models.

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The orthogonality condition can be used for classical parametric inference as well as for recently developed nonparametric instrumental variable estimation (Ai and Chen (2003)). In the latter case, we propose to summarize the effect of intervention with the average treatment effect. This statistic is also suggested in Fatum and Hutchison (2010), so our nonparametric instrumental variable estimator can be seen as an alternative to their propensity-score matching methodology. The testing framework proposed here is novel and is based on an application of the wild bootstrap to the average treatment effect statistic so as to account for conditional heteroscedasticity.

Realized volatility has been investigated before in the context of official intervention. However, the direction of causality explored in previous paper has been from intervention to the realized measure (Beine et al (2007), Beine et al (2009), Hillebrand et al (2009), Cheng et al (2013)). As far as we can tell, realized volatility is not explored as an identification source for level effects of intervention. In any case, the results from these studies are consistent with the view that official intervention affects realized measures of volatility. This means realized volatility is unlikely to be a weak instrument and therefore supports the approach adopted here. Nonetheless, it remains an empirical question if the instrument is weak in a particular context.

Moving to our empirical application, it is important to mention other papers investigating level effects of spot intervention on the BRL/USD market[1]. Novaes and Oliveira (2005) assume a known generating process for intervention; Meurer (2010) adopts an event study methodology; Wu (2010) assumes structural VAR based on a microstructure model; Kohlscheen (2013) compares intervention and nonintervention samples and applies propensity scores. Only the last two papers use actual intervention data as is the case here. Our dataset is also larger and more recent than the typical one in the literature, with daily information from 2007 to 2011. Although instrumental variable identification is not generally more efficient or transparent than the methods used in these papers, we believe this is the case for our particular instrumental variable estimator. Our approach is also less demanding on the identifying assumptions. As for substantive results, we find very robust evidence of effective intervention regardless of the specific window of events as often emphasized in this literature.

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[1] There are many papers not mentioned here investigating effects of spot intervention on volatility and other features of the market, as well as a few papers studying the effect of swap interventions (e.g. Novaes and Oliveira (2005) and Kohlscheen and Andrade (2013)). This paper considers only spot interventions and level effects, with a robustness exercise with swap interventions.

An important advantage of the dataset used here is the possibility to control for order flow through financial intermediaries. Although order flow is a well-known driver of exchange rate dynamics (e.g. Vitale (2007)), none of the previous papers using an instrumental variable approach controlled for this proximate driver of exchange rate dynamics (e.g. Domingues and Frankel (1993), Galati and Melick (1999), Galati et al (2005), Kearns and Rigobon (2002), Tapia and Tokman (2004)). For the BRL/USD market, Wu (2010) and Kohlscheen (2013) also used order flow information but with other identification strategies. The possibility of nonlinear interactions between order flow and intervention is raised in Kohlscheen (2013), since order flow coefficient is not stable in intervention and nonintervention periods. Recent papers exploring nonlinear level effects of intervention (Taylor (2004, 2005), Reitz and Taylor (2008, 2012) and Beine, Grauwe and Grimaldi (2008)) also do not control for order flow information, and the nonparametric approach adopted here is more flexible than the parametric specifications generally adopted.

The paper is structured as follows. In the section following this introduction, realized volatility is presented as an instrument for intervention policy. Considering the need for robust results, the next section proposes a nonparametric instrumental variable estimator and corresponding test statistic. The fourth section reports the results applying our framework to Brazilian intervention data. The final section offers some conclusions and comments on the general applicability of the method developed in this paper.

2. Realized Volatility as an Instrument

Let $r_{t,i}$ be log return on the foreign exchange rate on tick i of day t such that with n_t ticks available the daily return is $r_t = \sum_{i=0}^{n_t} r_{t,i}$. Define realized variance by $rv_t = \sum_{i=0}^{n_t} r_{t,i}^2$, and realized volatility its square root $rv_t^{1/2}$. If returns are not correlated, it can be shown (e.g. Macleer and Medeiros (2008), under Brownian motion) that realized variance is an unbiased, consistent and asymptotically normal estimator for the conditional variance of the foreign exchange rate $\sigma_t^2 = \text{Var}_t(r_t)..$ The conditional variance is determined by the error process ε_t in the conditional expectation, such that $r_t = E_t(r_t) + \varepsilon_t$. The index t in variance and expectation operators indicate measurability with respect to information known *at the beginning* of period t.

For concreteness, consider the following Log-linear Realized-GARCH model (see Hansen et.al (2011)):

$$\varepsilon_t = \sigma_t \eta_t$$
$$\log \sigma_t^2 = \omega + \delta \log \sigma_{t-1}^2 + \rho \log rv_{t-1} \tag{1}$$
$$\log rv_t = \xi + \varphi \log \sigma_t^2 + \tau(\eta_t) + u_t$$

with $\eta_t \sim iid(0,1)$, $u_t \sim iid(0, \sigma_u^2)$ and $\tau(.)$ a non linear leverage function. The last equation incorporates the fact the realized variance is a consistent estimator of the conditional variance. The second equation incorporates the measurability requirements and induces an autoregressive process in the log conditional variance. These are the measurement and state equations, respectively.

The most significant consequence of this models for our purpose is orthogonality condition: $E\left(\varepsilon_t \middle| rv_{t-1}^{1/2}\right) = 0$. This can be verified by simple algebra, since

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\begin{aligned}
E(\varepsilon_t | rv_{t-1}) &= E\left(\sigma_t \eta_t \middle| rv_{t-1}^{1/2}\right) \\
&= E\left(s(\sigma_t) e^{\frac{\omega}{2}} \sigma_{t-1}^{\delta} rv_{t-1}^{\frac{\rho}{2}} \eta_t \middle| rv_{t-1}^{1/2} \right) \\
&= rv_{t-1}^{\frac{(\varphi\rho+\delta)}{2\varphi}} e^{\frac{\omega}{2} - \frac{\xi\delta}{2\varphi}} E\left(s(\sigma_t) e^{-\frac{u_{t-1}\delta}{2\varphi}} e^{-\frac{\tau(\eta_{t-1})\delta}{2\varphi}} \eta_t \middle| rv_{t-1}^{1/2} \right) = 0,
\end{aligned}
\tag{2}
$$

with $s(.)$ is the sign function. That is, as long as $u_{t-1}, \eta_{t-1}, \eta_t$ are independent conditionally on $s(\sigma_t), rv_{t-1}^{1/2}$, which we shall assume. In this case, in the last step, we may use the law of iterated expectations in the term inside the expectation operator and then use conditional independence. It is interesting to observe that $E\left(\varepsilon_t \middle| rv_t^{1/2}\right)$ is in general different from zero due to the contemporaneous leverage effect in the measurement equation. Also, we may drop the sign condition is σ_t is assumed positive. Finally, note the exact same argument applies to the realized variance, so that the orthogonality condition $E(\varepsilon_t | rv_{t-1}) = 0$ is also available if the error processes are independent conditional on lagged realized variance.

The orthogonality condition with realized volatility is the basis for an instrumental variable estimator. In fact, consider the following model for the conditional expectation of the log exchange rate return

$$E_t(r_t) = \alpha + \theta intv_t + \beta' x_t \tag{3}$$

where the intervention variable $intv_t$ is endogenous and the covariates x_t are exogenous, that is, $E(\varepsilon_t|intv_t) \neq 0$ and $E(\varepsilon_t|x_t) = 0$. If the intervention policy is such that it is correlated with realized volatility as known at the beginning of the period, that is, $Cov\left(rv_{t-1}^{1/2}, intv_t\right) \neq 0$, then realized volatility is a useful instrument. Even if the reaction function actually responds to contemporaneous realized volatility, the autoregressive structure in the state equation along with the measurement equation would imply the necessary correlation. Of course, it will always be an empirical question if the instrument is sufficiently strong for inference. For implementation, one must use a measure of realized volatility obtained from the exchange rate series, since a measure for the residual of the model is not available, assuming both are essentially equal, a sensible proxy variable assumption for exchange rate models.

Note that lagged and contemporaneous intervention could be included in the measurement and state equations, respectively, such that the orthogonality condition would be $E\left(\varepsilon_t|rv_{t-1}^{1/2}, intv_{t-1}\right) = 0$. Again, the adequate condition must be judged empirically, as indicated by over-identification and weak instrument diagnostic tools. As illustrated in the application section below, it is possible to extend the argument for interventions in the futures market, as well as to pool the instrumental variables for both kinds of interventions using the covered parity relation. Also note that other realized measures, such as bipower variation, intraday range and squared return could be used in place of realized volatility or realized variance. However, the measurement equation is probably better specified in the case of realized variance since it is a relatively more efficient estimator of conditional variance. For this reason, in the application to our dataset, we focus on the realized volatility as our observed measure of volatility. Finally, note other conditional volatility models incorporating intraday information would imply similar orthogonality conditions; for instance, Engle and Gallo (2006) estimate a model that has essentially a realized GARCH specification and so similar arguments would apply.

3. Non-parametric estimator and average treatment effect

For robustness, it is interesting to estimate a more general model, such as

$$E_t(r_t) = \alpha + \theta\, intv_t + \beta'(x_{1t}, x_{2t}) + f(intv_t, x_{2t}) \tag{4}$$

for an unknown function $f(.)$ and under the same endogeneity assumption as before, with $x_t = (x_{1t}, x_{2t})$ so as to allow for flexible nonlinear interactions with a subgroup of the control variables. We may consider the non-parametric instrumental variable estimator of Ai and Chen (2003) which is consistent for the real parameters and for the unknown function, as well as asymptotically normal for the real parameters. One may use the wild bootstrap for inference so as to account for conditional heteroscedasticity.

If the intervention is excluded from the nonparametric part of the model, θ continues to summarize the effect from intervention. But such a restriction would be hard to justify. In order to summarize the effect from intervention without arbitrary exclusion restrictions, we may consider the average treatment effect

$$\text{ATE} = T^{-1} \sum \big(E(r_t | x_t, intv_t) - E(r_t | x_t, intv_t = 0) \big) \tag{5}$$

This is a parameter as long as we condition on the sample covariates and intervention policy. Using the estimated conditional expectations instead results in a random variable. As mentioned before, we may test the null of zero average treatment effect by applying the wild bootstrap.

Indeed, consider testing the null that $H_0: ATE \leq 0$ against the alternative that $H_1: ATE > 0$. Let $\delta_t = E(r_t | x_t, intv_t) - E(r_t | x_t, intv_t = 0)$. The test statistic is $t = \sqrt{T}\bar{\delta}_t / Avar(\bar{\delta}_t)$. We propose the following wild bootstrap algorithm

1. Generate the wild bootstrap residuals $\{\varepsilon_t^*\}_{t=1}^T$ from $\varepsilon_t^* = \hat{\varepsilon}_t \eta_t$, where η_t is a sequence of i.i.d. random variables with zero mean and unit variance, $\hat{\varepsilon}_t = r_t - \hat{E}_t(r_t)$, and such that $r_t^* = \hat{E}_t(r_t) + \varepsilon_t^*$.
2. Calculate the bootstrap test statistic t^* on the sample $\{r_t^*, intv_t, x_t\}_{t=1}^T$.

3. Repeat this procedure several times and calculate the p-values for the t statistic with the empirical distribution of the bootstrapped t^* statistics.

Notice we assume the orthogonality condition associated with realized variance is sufficiently strong to result in consistent estimates of the true model. Otherwise, the average treatment effect would have to be estimated by other methods, such as propensity-score matching methodology (e.g. Fatum and Hutchison (2010)).

One may also consider the weighted average treatment effect, perhaps with weights given by the inverse of realized standard deviation. That is,

$$\text{wATE} = T^{-1} \frac{\omega_t}{\sum \omega_t} \sum \left(E(r_t|x_t, \text{intv}_t) - E(r_t|x_t, \text{intv}_t = 0) \right) \tag{6}$$

with $\omega_t = 1/\sqrt{rv_t}$. If the endogeneity problem is particularly severe in high volatility periods, with the intervention failing to completely reverse foreign exchange shocks, then it makes sense to down weight such periods. Although the instrumental variable estimation is consistent, it may not be particularly efficient in finite samples[2]. The weighted average treatment effect imposes a second layer of protection against possible finite sample biases.

Finally, when defining the average treatment effect for period with positive and negative interventions, it is necessary that negative interventions enter with a negative sign, so as to avoid shrinking the average effect to zero. Taking advantage of the nonlinear estimation, it may be also of interest to obtain separate average treatment effects for both positive and negative interventions. We illustrate these possibilities in the application section below.

4. Application: Official Intervention in Brazil

The Central Bank of Brazil has a formal mandate to reduce exchange rate volatility and an official policy of international reserves accumulation. In our sample, there is no announced rule or commitment for intervention policy. Intervention tends to

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[2] Ai and Chen (2003) efficiency results refer only to the finite dimensional parameters and does not allow for time series dependency. Although the estimation of the nonparametric part is consistent in an appropriate metric, there are no results establishing efficiency or finite sample properties.

be correlated with order flow, with the stated purpose of not upsetting underlying market trends (see e.g. Barroso and Sales (2012)). There are large and frequent spot market interventions and occasional interventions in the futures market through derivative instruments with cash settlement (swaps for short).

Data. Our database begins in July the 11th 2007 and ends in November the 30th 2011. The series are sampled at a daily frequency. The BRL/USD foreign exchange rate is measured in domestic currency so that an increase is depreciation. The order flow variable is from the Central Bank of Brazil electronic records of private spot transactions intermediated by financial institutions and covers the entire market; a positive reading means domestic institutions are net buyers of foreign currency against other parties. The actual spot intervention policy of the Central Bank of Brazil is used as a regressor, as compared to a proxy based on international reserves, an a positive number means buying dollars. See Kohlscheen (2012) for further details regarding order flow and spot intervention. In robustness exercises we also consider swap interventions, and the data is publicly available in the Central Bank of Brazil web site. Both interventions are plotted in Figure 1. The realized variance measure is from Bloomberg and is based on 48 intraday measures of return. The set of covariates includes the CRB commodity price index, the implicit volatility index VIX, the dollar index DOL and the emerging market spread index from JPMorgan EMBI+. The interest rate differential measured as the Selic minus the Fed funds rate was considered as a possible covariate.

Parametric. We estimate linear regressions using ordinary least squares, instrumental variable and weighted instrumental variables. In the second and third cases, realized variance is an instrument for spot intervention and identification is exact. In the third case realized variance is used as a consistent estimator for conditional variance in an attempt to obtain more efficient estimators.

The results are summarized in Table 1. There is a clear simultaneity bias in the ordinary least squares estimator for the spot intervention coefficient. The negative coefficient means that the domestic currency depreciates when the Central Bank sells foreign currency, or that it appreciates when the monetary authority is buying dollars. In reality, this only reflects that the monetary authority is leaning against the wind of exogenous variation in the foreign exchange rate. The coefficient on net order flow variable may also be qualified as counterintuitive, since dollar inflows would be associated with depreciation of the domestic currency. The coefficients on the other

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variables are reasonably signed and are highly significant, except for the global risk aversion indicator. Excluding this variable and the net order flow does not change the results on the other variables.

Using realized variance as an instrument for spot intervention leads to completely different results. The spot intervention effect is now estimated to be positive. It is either marginally significant when including all controls and significant at 10% when including only significant controls. For each 1 billion USD buy intervention there is a corresponding depreciation 1.18% of the domestic currency in our preferred model. The test for endogeneity is significant and the Cragg-Donald F statistic from the first stage regression is much larger than Stock-Yogo critical values. Overall, the instrumental variable specification seems appropriate. The net order flow variable shows an inverted sign, although it is no longer significant. The remaining control variables preserve the sign and significance pattern from the ordinary least squares estimation.

These results are similar when using the weighted instrumental variable estimator. The spot intervention is correctly signed and is statistically significant at 10%, at the margin of 5%. For each 1 billion USD buy intervention there is a corresponding depreciation of 0.51% of the domestic currency according to our preferred model. Net order flow continues to show no significance, but the proxy for international risk aversion gains significance with the lower standard errors.

The interest differential variable was not found to be significant in any of the specifications and its exclusion had no impact on the size and significance of other parameters. For this reasons, we reported only results excluding the variable. This is consistent with results from Kohlscheen (2012) using the same dataset.

The instability of the estimated effect of net order flow is also consistent with results from Kohlscheen (2012) according to which this effect is not constant in intervention and nonintervention subsamples. Since order flow has often been found to be one of the best proximate determinants of foreign exchange rates in sample and out of sample, we investigate a more flexible specification allowing for flexible nonlinear interactions between official intervention and selected controls including order flow.

Non-Parametric. We estimate the general model with a linear and nonparametric part defined in equation (4), with x_{2t} set to the net order flow variable so as to focus on possible nonlinear interactions suggested by the literature and by the

results from the linear parametric model. We consider the Ai and Chen (2003) estimator. Accordingly, we use power series sieves to approximate the conditional expectation in a first step using third degree polynomials. The nonparametric part is approximated in a second step with a power series sieve of second degree. The resulting model is used to calculate the average treatment effect defined in equation (5) and the test statistic for such average. The wild bootstrap defined in Section 3 is used to obtain p-values. The effect of negative interventions is multiplied by minus one throughout, so that a positive effect for negative interventions is correctly signed, showing that the domestic currency appreciates when the central bank sells foreign currency.

The results are reported in Table 2. The scaled average treatment effect allows us to think of the average effect of a counterfactual 1 billion USD intervention. For each 1 billion USD acquisition of foreign currency, there is an average depreciation in the range of 0.445% and 0.608% depending on the controls in the model. The effect is significant at 5% in the preferred model including all the controls except for the interest rate differential (model 2 in the table). Moving on, for each 1 billion USD selling of foreign currency, there is an average appreciation in the range of 0.552% and 0.728% depending on the controls in the model. The effect is once again significant at 5% in the preferred model. For the average effect, we obtain the range 0.470% and 0.608% variation, and this is significant at 1% in the preferred model.

The analogous results for the weighted estimator are reported in Table 3. For a counterfactual 1 billion USD acquisition of foreign currency, there is an average depreciation in the range of 0.463% and 0.647%, down weighting volatile episodes, depending on the controls in the model. The effect is significant at 5% in the preferred model including all the controls except for the interest rate differential (again, model 2 in the table). Now, for a counterfactual 1 billion USD selling of foreign currency, there is an average appreciation in the range of 0.508% and 0.636%, down weighting volatile episodes, depending on the controls in the model. The effect is once again significant at 5% in the preferred model. Considering the overall average effect, down weighting volatile episodes, the variation in the corresponding direction of the intervention is in the range 0.487% and 0.660%, and this is significant at 5% in the preferred model.

Overall the average effect or even the conditional effects of sell or buy interventions are close to the 0.51% estimated in the linear framework, which is therefore robust to nonlinear interactions. In any case, in the nonparametric

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framework, the effect of each individual intervention will depend in a very nonlinear way on system conditions and intervention attributes. The effects reported above refer to the estimated average across many different system conditions observed in the sample. It should not be interpreted as a linear coefficient that scales with the size of the intervention. Policy makers and market participants should estimate a similar nonparametric model to forecast the impact of any particular policy in any given system condition. If the conditional expectation were linear, there would be a one to one correspondence between the average effects and the coefficient in the linear model.

Swaps. So far we have not addressed the possible bias coming from the use of other forms of official intervention that might be correlated with spot market intervention. In particular, in our sample, derivative market interventions with cash settlement (swaps for short) correlate positively with spot interventions, introducing the possibility of an upward bias in the results reported above. Our first answer to this is that the results can always be interpreted as the structural impact of spot interventions used in association with swaps as observed in the sample. This is still a relevant structural parameter for the policy maker. The results for this parameter are still a nice illustration of the identification strategy proposed in the paper.

We perform three additional robustness exercises: first, we estimate the effect of spot intervention excluding from the sample the days of swap intervention; second, we estimate on the full sample with additional instruments for the swap operations; third, we estimated a nonparametric instrumental variable model controlling for swaps. In the case of instrumental variables in the linear framework, the instrument list includes (i) a realized variable for the future market, namely the squared variation of the nearest future quotation, and (ii) the net order flow variable. From the covered interest parity, innovations in future and spot exchange rate variation should be close to each other, so that a realized measure in the future could provide additional information. Previous results exclude net order flow from the linear model, and the statements by policy makers suggest order flow is associated with spot market interventions. Both factors suggest net order flow could be used as an instrument. In the nonparametric model, the focus is on neglected nonlinearity in order flow, so we do not include it as an instrument.

The results for the linear robustness exercises are summarized in Table 4. Consider first the no swap sample. As before, there is a clear simultaneity bias in the

ordinary least squares estimator and using realized variance as an instrument for spot intervention inverts the sign of the coefficient. The effect is significant at 10%. Consider now the full sample. Again there is a clear endogeneity bias in spot interventions. With instrumental variable estimation, the effect has the opposite sign, at 0.31% for each 1 USD billion intervention, and is significant at 5%. There is no robust evidence of level effects of swap operations. Moreover, there is no robust evidence of bias in our previous estimates for the effects of spot interventions. The estimated effect in our preferred specification in the last column is lower than the estimates obtained in the previous section, which supports the hypothesis of a positive bias in intervention effects obtained without controlling for swaps.

The results for the nonparametric robustness exercise for swaps are reported in Table 5. Using realized volatility and squared future returns as instruments for both interventions does not result in significant results. We report the regression using only realized volatility to instrument for spot interventions. The scaled average effects are of the order of 0.36% for each 1 USD billion intervention, and this is significant at 1%. This is close to the result from the linear model and lends further support to a small positive bias without controlling for swap operations. We interpret these results as evidence of complementarity of both types of official intervention.

4. Conclusion

This paper contributes to the tradition of instrumental variable estimation of the effect of official intervention. We propose a novel orthogonality condition formally deduced from standard properties of conditional volatility models. In particular, we show that realized variance is orthogonal to the innovation in a Log-linear Realized-GARCH model, as well as argue that it is correlated to intervention by reference to empirical literature relating both variables and to standard policy rationale often presented by monetary policy authorities. We consider both parametric and nonparametric instrumental variable estimation, in the latter case also proposing a statistical test based on the average treatment effect of official intervention.

We apply the proposed instrumental variable approach to a unique dataset for the Brazilian foreign exchange market with full records of official intervention and net order flow intermediated by the financial system. In the linear framework, for each 1

billion USD buy (sell) intervention there is a corresponding depreciation (appreciation) of 0.51% of the domestic currency. In the nonparametric framework incorporating nonlinear interaction between official intervention and the underlying market conditions represented by order flow information, for each 1 billion USD buy (sell) intervention there is a corresponding depreciation (appreciation) of 0.48% (0.57%) of the domestic currency. The effects were significant at 5%. The nonparametric estimates suggest larger effects on sell interventions and point to the relevance of nonlinear interactions. These effects assume swap operations are conducted in the same way as in the sample. Estimated effects of spot interventions are a bit lower controlling for official derivate market interventions, and range from 0.31% to 0.38% in the linear and nonparametric models. This suggests both official intervention policies are complementary.

The deductive reasoning leading to our orthogonality condition may be generalized and adapted in several directions as appropriate for other empirical applications. For example, as illustrated in our robustness exercises involving swap operations, one may consider other realized measures, such as bipower variation, intraday range or the squared return. It is also possible to include the intervention variable in the model equations leading to more general orthogonality conditions. Finally, one may extend the results to other conditional volatility models with intraday information beyond the Log-linear Realized-GARCH model considered in our application. The positive empirical results found here should provide sufficient motivation for such extensions.

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Table 1. Effect of Intervention: linear regression

Dependent variable: d(brl_usd)

	OLS		IV		w-IV	
c	0.02	0.03	-0.15 ^{o}	-0.15 ^{o}	-0.17 ***	-0.16 **
	0.72	0.92	-1.49	-1.53	-2.72	-2.66
spot	-0.33 **	-0.22 **	1.24 ^{o}	1.18 *	0.59 *	0.51 *
	-2.07	-1.52	1.53	1.71	1.63	1.66
d(crb)	-0.39 ***	-0.40 ***	-0.48 ***	-0.47 ***	-0.20 ***	-0.19 ***
	-6.18	-6.31	-6.59	-6.88	-5.16	-5.43
d(dol)	0.35 ***	0.38 ***	0.42 ***	0.41 ***	0.35 ***	0.36 ***
	5.60	5.96	5.66	6.10	7.98	8.82
d(embi)	0.14 ***	0.15 ***	0.16 ***	0.16 ***	0.05 ***	0.07 ***
	9.56	10.07	7.84	8.68	6.41	10.20
d(vix)	0.13		0.34		0.02 ***	
	0.21		0.51		5.14	
netflow	0.15 ***		-0.04		-0.06	
	3.33		-0.43		-0.87	
nobs	973	973	972	972	972	972
R-squared	0.40	0.40	0.27	0.28	0.25	0.26
Endogeneity (dJ)	-	-	17.39	19.44	5.83	4.87
Cragg-Donald (F)	-	-	81.79	106.25	32.69	35.99

Note: t-values bellow estimates; HAC *** 1%, ** 5%, *10%, ^{o}15%

Table 2. Averate Treatment Effect of Intervention: nonparametric estimation

Dependent variable: d(brl_usd)

Model	ATE	Scaled ATE	t-stat	p-value
all/1	0.091	0.608	35.872	0.0234
pos/1	0.114	0.614	32.956	0.0862
neg/1	0.170	0.552	31.941	0.0280
all/2	0.070	0.470	51.649	0.0092
pos/2	0.083	0.445	50.096	0.0440
neg/2	0.224	0.728	32.959	0.0280
all/3	0.079	0.525	45.159	0.0120
pos/3	0.095	0.511	42.105	0.0598
neg/3	0.204	0.665	32.739	0.0202

Notes:
(i) Wild bootstrapp using N(0,1); 5000 replications
(ii) Newey-West variance estimator of asymptotic variance
(iii) Power series sieve; 3rd degree cond.expectation; 2nd degree nonparametric part
Models:
[1] nonlinear: spot, netflow; linear: spot, netflow, d(crb), d(dol), d(embi), d(vix), d(drate)

[2] nonlinear: spot, netflow; linear: spot, netflow, d(crb), d(dol), d(embi), d(vix)

[3] nonlinear: spot, netflow; linear: spot, netflow, d(crb), d(dol), d(embi)

[all] average effect of all interventions; negative interventions x (-1)

[pos] average effect off positive interventions x (+1)

[neg] average effect off negative interventions x (-1)

Table 3. Weighted Averate Treatment Effect of Intervention: nonparametric estimation

Dependent variable: d(brl_usd)

Model	wATE	Scaled wATE	t-stat	p-value
all/1	0.107	0.711	17.564	0.0638
pos/1	0.125	0.676	22.638	0.0592
neg/1	0.145	0.472	30.649	0.0690
all/2	0.076	0.510	32.200	0.0136
pos/2	0.089	0.479	35.067	0.0204
neg/2	0.175	0.569	44.229	0.0290
all/3	0.088	0.589	27.434	0.0226
pos/3	0.103	0.555	28.601	0.0364
neg/3	0.164	0.535	39.020	0.0406

Notes:

(i) Wild bootstrapp using N(0,1); 5000 replications

(ii) Newey-West variance estimator of asymptotic variance

(iii) Power series sieve; 3rd degree cond.expectation; 2nd degree nonparametric part

(iv) Weighted by the inverse of realized standard deviation

Models:

[1] nonlinear: spot, netflow; linear: spot, netflow, d(crb), d(dol), d(embi), d(vix), d(drate)

[2] nonlinear: spot, netflow; linear: spot, netflow, d(crb), d(dol), d(embi), d(vix)

[3] nonlinear: spot, netflow; linear: spot, netflow, d(crb), d(dol), d(embi)

[all] average effect of all interventions; negative interventions x (-1)

[pos] average effect off positive interventions x (+1)

[neg] average effect off negative interventions x (-1)

Table 4. Effect of Intervention: linear regression, robustness to swaps

Dependent variable: d(brl_usd)

	no-swap sample			swap sample		
	OLS	IV	w-IV	OLS	IV	w-IV
c	0.04	-0.12 **	-0.13 **	0.04	-0.11 **	-0.14 ***
	1.07	-2.02	-5.11	1.30	-1.41	-3.85
spot	-0.21 **	0.89 **	0.31 **	-0.27 *	0.90 *	0.31 *
	-1.98	2.25	2.10	-1.83	1.67	1.91
d(crb)	-0.42 ***	-0.44 ***	-0.18 ***	-0.41 ***	-0.47 ***	-0.18 ***
	-6.42	-6.40	-4.37	-6.48	-7.12	-5.10
d(dol)	0.31 ***	0.34 ***	0.35 ***	0.38 ***	0.41 ***	0.36 ***
	4.95	5.31	8.90	5.96	6.20	9.22
d(embi)	0.14 ***	0.14 ***	0.07 ***	0.15 ***	0.16 ***	0.07 ***
	10.45	9.96	8.76	9.96	9.23	10.41
swap				0.16 *	0.11	0.24
				1.90	0.23	0.68
nobs	884	883	883	973	972	972
R-squared	0.41	0.32	0.30	0.39	0.32	0.26
Endogeneity (dJ)	-	20.58	17.63	-	23.73	10.16
Cragg-Donald (F)	-	62.93	121.75	-	8.30	11.02

Note 1: t-values bellow estimates; HAC *** 1%, ** 5%, *10%, °15%

Note 2: sample with or without days of swap operations; instrument list includes lagged realized variance, net order flow and, for the IV-swap sample, squared variation of exchange rate futures; when applicable, overidentifying conditions are not rejected at 5%.

Table 5. Average Treatment Effect of Intervention: nonparametric, robustness to swaps

Dependent variable: d(brl_usd)

Model	ATE	Scaled ATE	t-stat	p-value
all	0.054	0.360	51.243	0.0082
pos	0.063	0.337	48.510	0.0402
neg	0.180	0.586	36.205	0.0128
w-all	0.058	0.385	32.450	0.0124
w-pos	0.067	0.361	35.303	0.0190
w-neg	0.141	0.458	48.064	0.0230

Notes:

(i) Wild bootstrapp using N(0,1); 5000 replications

(ii) Newey-West variance estimator of asymptotic variance

(iii) Power series sieve; 3rd degree cond.expectation; 2nd degree nonparametric part

(iv) nonlinear: spot, netflow; linear: spot, swap, netflow, d(crb), d(dol), d(embi), d(vix)

(v) intervention instrumented by lagged realized volatility

Models:

/all average effect of all interventions; negative interventions x (-1)

/pos average effect of positive interventions x (+1)

/neg average effect of negative interventions x (-1)
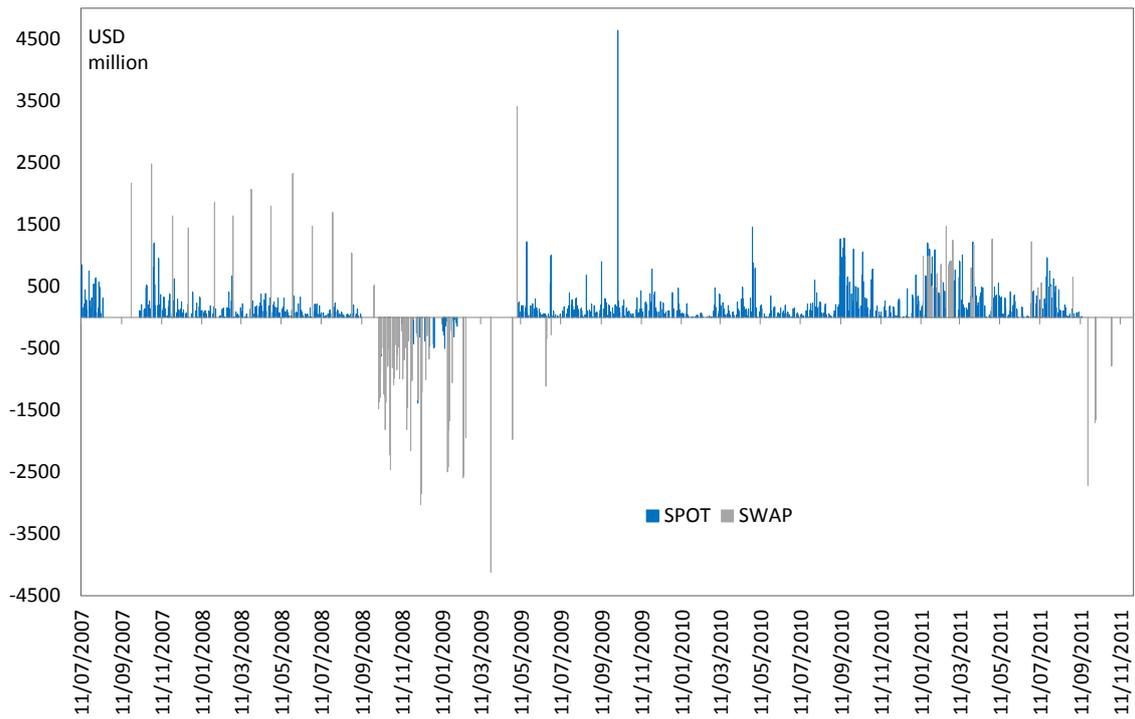
/w- weighted average treatment effect

Figure 1. Official Intervention

Figure 2. Exchange Rate and Realized Variance