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Rationality of announcements, business cycle asymmetry, and predictability of revisions. The case of French GDP

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Résumé

Nous analysons les révisions de la croissance du PIB français et nous étudions la rationalité des premières estimations. Nous incluons des mécanismes non linéaires, à savoir la prise en compte de l'état du cycle économique et la présence de ruptures temporelles, et détaillons leurs effets sur les deux premiers moments de la distribution des révisions et sur les tests de rationalité. Un premier constat intéressant de notre travail est de mettre en avant la présence récurrente de cette non-linéarité dans les résultats. Par ailleurs, nos résultats montrent que les révisions ne sont globalement pas biaisées, mais que les premières estimations de la croissance du PIB n'utilisent pas de façon efficiente toute l'information macroéconomique et financière disponible. Enfin, nous étudions la prévisibilité des révisions de la croissance du PIB en temps réel et développons un modèle de prévision des révisions totales (estimations définitives moins premières estimations), plus précis qu'un modèle de référence autorégressif.

Mots-Clés : Révisons du PIB, Données en temps réel, Efficience, Biais, Prévisions
JEL : C22, C52, E37

Abstract

We analyze French GDP revisions and we investigate the rationality of preliminary announcements of GDP. We consider nonlinearities, taking the form of business cycle asymmetry and time changes, and their effect on both unconditional moments of revisions and the rationality of announcements. We find that nonlinearity represents an interesting feature of French GDP announcements and revisions. Our results suggest that revisions are unbiased, but announcements are overall inefficient, conditionally on a set of macro-financial indicators. Finally, we investigate the forecastability of GDP revisions in real-time and we find out that total revisions are predictable.

Keywords: GDP Revisions, Real-time dataset, Efficiency, Unbiasedness, Forecasting
JEL: C22, C52, E37

Non-technical summary

The economic and econometric literature has been paying increasing attention to the statistical properties of preliminary announcements of key macroeconomic variables, such as GDP, and their subsequent revisions. Monetary and fiscal policy decisions usually depend on preliminary estimates of the state of the economy. This implies that the more accurate the initial announcement for these indicators, the more effective the impact of policies. For this purpose, initial announcements must be "rational", *i.e.* unbiased and efficient predictors of final data releases.

With respect to GDP, recent empirical literature has pointed out that the rationality condition does not hold overall on preliminary announcements for a large group of industrialized countries. This means that early announcements of GDP cannot be considered as rational estimates of revised data. In other words, revisions do not contain news, but reduce statistical noise. However, the inefficiency of early estimates would also mean that revisions might be predictable, using for instance information available at the time of the initial announcement as predictor. For the case of France, Faust et al. (2005) reject the hypothesis of data rationality. Shrestha and Marini (2013) find out that initial announcements of GDP are on average underestimated before the Great Recession episode, but overestimated thereafter. Ahmad et al. (2004) find out significant unconditional bias of GDP announcements, as well as fairly large and persistent revisions.

In this paper, we assess the rationality of preliminary estimates of French GDP and we perform a forecasting analysis of revisions. For this purpose, we use a real-time database including complete vintage estimates of real GDP growth rates spanning from Q1 1991 (released in Q2 1991) to Q4 2014 (released in Q1 2015). The data up to 2012 are provided by the French National Statistical Agency (INSEE) and updated by the authors of the present paper. Compared to the previous studies on France, our dataset has the main advantage of providing a longer-term perspective of GDP announcements and revisions by covering the last 25 years, several base-year changes and annual accounts releases, and a few business cycle phases (including three recessions out of the four identified in the post-war period).

Our first goal is to investigate the main statistical features of GDP revisions, especially their mean and their standard error.[2] We find out that GDP revisions satisfy the two main expected statistical properties: revisions are globally unbiased, meaning that there is no apparent systematic bias in the initial announcement of GDP growth; the volatility of revisions decreases with the order of releases, meaning that for a given quarter, the size of revisions declines progressively and thus the last known GDP value converges towards a stable final value. However, the results also show an influence of the state of the business cycle on the statistical features of revisions. We find out that initial announcements are affected by an overestimation bias during phases of contraction of GDP. We also find out a positive correlation between revisions and the speed of expansion and contraction

[2] The literature usually assumes that revisions are normally distributed. We test this assumption and we cannot reject the hypothesis of normality.

of the economy. The volatility of early GDP revisions is also higher during phases of contraction. Besides, results also point to a decrease in the volatility of revisions overtime.

Our second goal is to provide an assessment of the forecast rationality of GDP announcements, *i.e.* we test whether revisions contain "news" unknown at the date of release of provisional estimates, which in turn implies unpredictable revisions. According to the rational expectation theory, we regress the entire GDP revision history on an information set available at the time of GDP announcements, and we test for the restrictions consistent with a rationality test. The set of additional variables includes accounted GDP revisions and a number of financial indicators standard to this literature, such as stock market returns, oil price, and interest rate spreads (Faust et al., 2005; Swanson and Van Dijk, 2006). We also include survey data on services and construction, because recent contributions to the literature on nowcasting French GDP (Mogliani et al., 2014) pointed out that survey data on these sectors display a fair predictive content when the target variable is implicitly the final GDP growth rate. Our results suggest that early GDP announcements are conditionally unbiased, but overall inefficient. GDP revisions are correlated with indicators available at the date of the estimate, mainly interest rate spread, stock market return and survey indicators. These results are broadly in line with those reported, for instance, by Faust et al. (2005).

Finally, our third goal is to study the forecastability of GDP revisions in real-time. This is particularly meaningful and appealing in the present context of an overall rejection of the efficiency hypothesis.[3] All in all, reasonably accurate predictions for total revisions (*i.e.* the difference between final and initial announcements) can be obtained, although they appear overall quite hard to track in real-time. Several factors, including the predictors entering the model as well as the forecasting model itself, may explain this outcome. This implies that the efficient use of available information in the early stages of GDP measurement could be a rather hard task for data producers, whose primary mission is to provide a consistent and detailed estimate of the economic activity through the system of quarterly National Accounts.

[3]We perform the analysis by implementing a GEneral-To-Specifc (GETS) approach to model selection, where the general unrestricted model includes the set of predictors used to assess the rationality of releases. With respect to total revisions, the selected specifications outperform an ARMA benchmark, over an hold-out sample spanning from 2004 to 2012. In spite of the outcome of rationality tests, the predictive accuracy is not improved when business cycle asymmetry is imposed to model selection.

1. Introduction

The economic and econometric literature has been paying increasing attention to the statistical properties of preliminary announcements of key macroeconomic variables, such as GDP, and their subsequent revisions. For an exhaustive review of this literature, we refer to Mankiw et al. (1984), Mankiw and Shapiro (1986), Mork (1987), Faust et al. (2005), Swanson and Van Dijk (2006), Aruoba (2008), and references contained therein. The rationale of this research activity is substantially threefold. First, monetary and fiscal policy decisions usually depend on preliminary estimates of the state of the economy, represented by standard business cycle indicators. This implies that the more accurate the initial announcement for these indicators, the more effective the impact of policies. For this purpose, initial announcements must be "rational", *i.e.* unbiased and efficient predictors of final (or pseudo-final) data releases, with revisions behaving as errors from rational predictions. The unbiasedness condition states that announcements should not be affected by a systematic prediction error, while the efficiency condition implies that all the available information is efficiently employed in the estimation of these preliminary values, as suggested by the rational expectations theory. Second, understanding the statistical features of initial announcements is a central issue for empirical economists. Indeed, when researchers investigate the effect of policy decisions through econometric models, the data chosen as input should be as close as possible to the data actually observed by policymakers at the time the policy decision was undertaken (Orphanides, 2001). Finally, the construction and evaluation of forecasting models should also account for the presence of data revisions, consistently with the target of the forecaster (Croushore, 2011). Indeed, the forecaster may face the "apples and oranges" problem when producing forecasts based on data that are preliminary or lightly revised in recent periods, but more heavily revised back in time (Koenig et al., 2003; Clements and Galvão, 2013; Carriero et al., 2015). It follows that the analysis of macroeconomic data revisions and the implication for macro-econometric modeling and forecasting are the main issues this literature has recently dealt with.

With respect to GDP, both statistical properties of revisions and rationality of announcements have been intensively investigated by recent empirical literature (Faust et al., 2005; Fixler and Grimm, 2006; Sleeman, 2006; Aruoba, 2008; Jacobs and van Norden, 2011; Giannone et al., 2012; Bishop et al., 2013), often focusing on a bunch of industrialized countries due to limited vintage data availability. In the present paper, we add to this literature by focusing on real GDP for France. This analysis is particularly interesting because previous studies often report conflicting evidence on the rationality of GDP announcements based on different vintage data samples. Here, using a novel dataset of complete vintage estimates with initial announcements spanning from Q1 1991 to Q1 2015, we assess the rationality of preliminary estimates of quarterly real GDP growth rates and we perform a real-time forecasting analysis of revisions. To the best of our knowledge, this is the first study performing this extensive analysis on France. A partial exception is provided by Faust et al. (2005), who report results for France in their study on GDP revisions for the

G7 countries. The authors reject the hypothesis of data rationality for all countries, including France, although only when an extended testing approach is considered. However, the sample of French GDP vintage estimates used in their contribution is quite small (quarterly data from 1987 to 1997). This prevents the authors from investigating crucial features of the data, such as, for instance, the impact of changes in the accounting technology on the evolution (mean and volatility) of revisions and releases, the effect of business cycle phases on the rationality of announcements, and the predictability of revisions. Similarly, Shrestha and Marini (2013) use a relatively short sample of GDP revisions spanning from 2000 to 2012. Focusing on two sub-samples defined by the *Great Recession* episode, the authors find out that the behavior of GDP revisions appear remarkably different across the two periods for a few countries, including France (initial announcements are on average underestimated and then overestimated). However, results suggest that the average size of revisions has not changed after the recession. Further, Ahmad et al. (2004) investigate the moments of GDP revisions for the G7 countries on a sample spanning from 1996 to 2000. The results for France suggest significant unconditional bias of announcements, as well as fairly large and persistent revisions during the period under analysis. The authors argue that the implementation of the new accounting methodology (SNA93) in May 1999 could explain this outcome. Compared to these previous studies, our dataset has the main advantage of providing a longer-term perspective of GDP revisions by covering the last 25 years (96 initial announcements of quarterly GDP growth rates), several base-year changes and annual accounts releases (often used by the Statistical Agency to introduce accounting innovations), and a few business cycle phases (including three recessions out of the four identified in the post-war period).[1]

Looking at the results reported in the existing literature on many OECD countries, it turns out that the unbiasedness and efficiency conditions do not hold overall on preliminary announcements of GDP (see for instance Aruoba, 2008 and Faust et al., 2005). This means that GDP revisions are more consistent with the definition of measurement errors rather than noise from rational forecasts of final values. In other words, revisions would not contain news, but would reduce statistical noise (Croushore, 2011). The evidence of inefficiency also suggests that revisions might be predictable, using for instance information available at the time of the announcement as predictor. This is a crucial point for policymakers, because even if preliminary GDP announcements are irrational, in practice subsequent revisions may be predicted by conditioning on some (neglected) information. However results are rather mixed on this point, suggesting that revisions may be hard to predict when the underlying process is a mix of news and measurement errors (Jacobs and van Norden, 2011). Our contribution to this literature is threefold.

[1] Zwijnenburg (2015) also considers a sample covering about 20 years (1994-2013), but he focuses only on testing for unconditional bias of GDP revisions. Unlike Ahmad et al. (2004), results for France suggest the rejection of the hypothesis of unconditional bias.

First, we investigate the main statistical features of GDP revisions, especially the first two moments of their distribution. We find out that, as expected, revisions are unconditionally unbiased, *i.e* average revisions are not significantly different from zero, and that the volatility of revisions decreases with the order of releases. However, it has been shown in the literature that revision processes may be characterized by nonlinear features that may affect unconditional and conditional moments (Brodsky and Newbold, 1994; Rathjens and Robins, 1995). A reasonable candidate for the underlying nonlinear mechanism is given by the asymmetry of the business cycle (Swanson and Van Dijk, 2006). Indeed, the sign and the volatility of revisions may depend on phases of the business cycle during which initial estimates of GDP are released. For instance, the contraction of activity may be exceptionally fast during recessions, such that the size of the drop could not be correctly evaluated in real-time by National Statistical Agencies (NSAs hereafter). In this example, initial announcements may overstate the economic activity during recessions, but they may also understate it during expansions. In the present paper, we follow the literature and we perform the analysis under the assumption of both symmetric and asymmetric business cycle. We nevertheless additionally assume that alternative nonlinear mechanisms, based on looser definitions of the standard business cycle, may be at play. For instance, revisions may depend on the sign and/or the size of the growth rate of released GDP. Further, revisions may be also correlated with the speed of expansion and contraction of the economy, rather than the (cycle or growth) phase of GDP. Finally, we investigate whether revisions to preliminary announcements do evolve over-time, possibly according to changes in the accounting performance of NSAs. All in all, the results suggest the presence of nonlinear features in both the mean and the volatility of revisions. We find out that initial announcements are affected by an overestimation bias during phases of contraction of GDP. Further, results provide some evidence of an increase in the volatility of total GDP revisions (*i.e.* the difference between final announcements and initial announcements) during phases of contraction, but also to an overall decrease in the volatility over time.

Second, we provide an assessment of the rationality of GDP announcements. We do so by examining the entire revision history for GDP through a sequence of testing procedures based on a standard regression approach. The analysis is performed by including an information set (*e.g.*, past revisions, stock market returns, interest rate spreads, survey data) available at the time of GDP announcements into linear, auxiliary, rationality regressions (see Faust et al., 2005, and Swanson and Van Dijk, 2006). We also assume that rationality may be affected by the phase of the business cycle, by the acceleration of the activity, or by structural changes. For example, during recessions, the data frequently used to compute early estimates may convey information on the severity of the contraction only with some lag. Results suggest that preliminary GDP announcements are conditionally unbiased, but inefficient. The latter can be mainly attributed to the significant correlation between GDP revisions and external economic information.

Finally, we investigate the forecastability of GDP revisions in real-time (Faust et al., 2005; Aruoba, 2008). This is particularly meaningful and appealing in the present context of an overall rejection of the efficiency hypothesis. We perform the analysis by modeling GDP revisions as dynamic linear and nonlinear equations and by implementing a GEneral-To-Specific (GETS) approach to model selection, where the general unrestricted model includes the set of predictors used to assess the rationality of announcements. Evaluation of point and density forecasts suggests that linear models provide reasonable results, while allowing for business cycle asymmetry in forecasting equations does not improve the overall predictive accuracy.

The paper is organized as follows. Section 2 describes the methodology followed in order to test for rationality of preliminary French GDP estimates, as well as for business cycle symmetry and time stability of the revision process. Section 3 presents the real-time dataset used in the present study and the strategy followed to construct GDP revisions. Section 4 reports results on the main statistical features of revisions and the rationality of preliminary GDP estimates. In Section 5, we investigate the predictability of GDP revisions. Finally, Section 6 concludes.

2. The econometric methodology

2.1. GDP revisions

Let us denote y_t^{t+k} the value of the growth rate of real GDP at quarter t, released at quarter $t+k$ (the vintage estimate). Initial announcements are usually released about 30-50 days after the end of the reference quarter, *i.e.* with one quarter lag. Accordingly, the notation used for these values is y_t^{t+1}, with $k = 1$. Final announcements are denoted $y_t^{t+\ell}$, where ℓ is supposed to be a finite value large enough to exclude further revisions. Moreover, we refer to preliminary announcements as the intermediate values of GDP released between $k = 2$ and $k = \ell - 1$. In this general framework, we can define a revision as the difference between two announcements released respectively at time $t + h$ and $t + k$:

$$_{h}y_t^k = y_t^{t+h} - y_t^{t+k} \tag{1}$$

where $h \geq k + 1$. From the general Equation (1), three useful expressions cover a wide space of possible revisions (Swanson and Van Dijk, 2006):

$$_{k+1}y_t^k = y_t^{t+k+1} - y_t^{t+k} \tag{2a}$$
$$_{k+1}y_t^1 = y_t^{t+k+1} - y_t^{t+1} \tag{2b}$$
$$_{\ell}y_t^k = y_t^{t+\ell} - y_t^{t+k} \tag{2c}$$

namely, fixed-width revisions (FXW hereafter) in expression (2a), increasing-width revisions (INW) revisions in (2b), and remaining revisions (REM) in (2c). It is worth noticing that INW revisions are equivalent to FXW revisions for $k = 1$ and to REM revisions for $k = \ell - 1$. The expression $_{\ell}y_t^1 = y_t^{t+\ell} - y_t^{t+1}$ (*i.e.* INW revisions for $k = \ell - 1$ and REM revisions for $k = 1$) denotes the *total* revisions, computed as the difference between final and initial announcements. As noted by Aruoba

(2008), total revisions are expected to have small variance compared to final announcements, *i.e.* $\sigma_{{}_{\ell}y_t^1} < \sigma_{y_t^{t+\ell}}$. The expression ${}_{2}y_t^1 = y_t^{t+2} - y_t^{t+1}$ (*i.e.* FXW and INW revisions for $k = 1$) denotes the *first* revisions. On the other hand, the expression ${}_{\ell}y_t^{\ell-1} = y_t^{t+\ell} - y_t^{t+\ell-1}$ (*i.e.* FXW and REM revisions for $k = \ell - 1$) denotes the *final* revisions. The expressions in (2a)-(2c) will be used to provide a statistical assessment (unconditional moments) of the process governing the revisions of French GDP (see Section 4.1).

2.2. Forecast rationality tests

The literature on testing for rationality of preliminary announcements is broadly based on the framework provided by Mankiw et al. (1984), where preliminary announcements (y_t^{t+k}) are tested for the hypothesis of either noisy estimates or rational forecasts of final announcements ($y_t^{t+\ell}$).[2] The measurement error hypothesis implies that provisional estimates are an observation of later releases, but measured with an error (or noise) that is progressively reduced across subsequent revisions, *i.e.* $y_t^{t+k} = y_t^{t+\ell} + \epsilon_{t+k}$. Under this assumption, noise revisions (ϵ_{t+k}) are orthogonal to final announcements $\left(\mathbb{E}[y_t^{t+\ell}, \epsilon_{t+k}] = 0\right)$, allowing for predictability of revisions. The measurement error hypothesis can be tested through the auxiliary regression

$$y_t^{t+k} = \alpha + y_t^{t+\ell}\beta + \varepsilon_{t+k}, \tag{3}$$

where the test is performed by setting the joint null hypothesis $\alpha = 0$ and $\beta = 1$. On the other hand, the rationality hypothesis implies that revisions contain news unknown at the date of release of provisional estimates, *i.e.* $y_t^{t+\ell} = y_t^{t+k} + \nu_{t+k}$. In this case, news revisions (ν_{t+k}) are uncorrelated with preliminary announcements $\left(\mathbb{E}[y_t^{t+k}, \nu_{t+k}] = 0\right)$ and hence unpredictable. According to the notion of rational expectations, preliminary announcements are rational forecasts of final announcements if and only if

$$y_t^{t+k} = \mathbb{E}\left[y_t^{t+\ell} | \mathbf{\Omega}_{t+k}\right],$$

where $\mathbf{\Omega}_{t+k}$ is the information available at time $t+k$. Thus, the rationality hypothesis can be tested through the auxiliary regression

$$y_t^{t+\ell} = \alpha + y_t^{t+k}\beta + \mathbf{X}'_{t+k}\boldsymbol{\gamma} + \varepsilon_{t+k}, \tag{4}$$

where $\mathbf{X}_{t+k}$ is a vector of indicators representing the information set available at the time of release of the vintage estimate $t + k$, and the test is performed by setting the joint null hypothesis $\alpha = 0$, $\beta = 1$, and $\boldsymbol{\gamma} = \mathbf{0}$.

[2] Noise revisions imply $\sigma_{y_t^{t+1}} > \sigma_{y_t^{t+\ell}}$, while news revisions imply $\sigma_{y_t^{t+1}} < \sigma_{y_t^{t+\ell}}$, because rational forecasts of later releases are expected to be smoother than exactly those later releases to be forecast.

In this paper, we follow Faust et al. (2005) and Swanson and Van Dijk (2006), who propose a general testing approach of the forecast rationality hypothesis based on the regression model

$$ {}_{\ell}y_t^k = \alpha + y_t^{t+k}\beta + \mathbf{X}'_{t+k}\boldsymbol{\gamma} + \varepsilon_{t+k}, \tag{5} $$

and a Mincer-Zarnowitz-type rationality test (Mincer and Zarnowitz, 1969) on the joint null hypothesis $\alpha = \beta = 0$ and $\boldsymbol{\gamma} = \mathbf{0}$.[3] Following Keane and Runkle (1990), the test of rationality in the context of Equation (5) can be broken down in an unbiasedness hypothesis ($\alpha = \beta = 0$, with the imposed restriction $\boldsymbol{\gamma} = \mathbf{0}$) and an efficiency hypothesis ($\alpha = \beta = 0$ and $\boldsymbol{\gamma} = \mathbf{0}$, with no imposed restrictions). By the means of these two sub-hypothesis, we can readily ascertain whether the rejection of the hypothesis of data rationality depends on the presence of either a prediction bias or incomplete information when constructing preliminary announcements, or both. Further, performing the test on different releases (y_t^{t+k}) may reveal whether irrationality (if any) is a special feature of earlier GDP estimates, which are usually constructed based on partial information and on equations bridging available provisional indicators over national accounts.[4]

2.3. Real-time peak-and-trough business cycle asymmetry (BC I)

Business cycle fluctuations may affect the efficiency of NSAs, in the sense that the statistical features of preliminary announcements and revisions may depend on the phase of the cycle. Indeed, changes in activity during recession may be exceptionally fast and thus hard to be estimated in real-time. Furthermore, data related to the manufacturing sector (survey data and industrial production index) may be more efficient than data related to other sectors of the economy to forecast initial announcements of GDP (Mogliani et al., 2014). However, during recessions, the contraction is often sharper in the former than in the latter. For that reasons, preliminary announcements may overstate the economic activity during contraction periods and understate it during expansion periods. In the present framework, we can test for rationality and business cycle symmetry by the means of the following model:

$$ {}_{\ell}y_t^k = \left(\alpha_1 + y_t^{t+k}\beta_1 + \mathbf{X}'_{t+k}\boldsymbol{\gamma}_1\right)\mathbb{I}(s_t) + \left(\alpha_2 + y_t^{t+k}\beta_2 + \mathbf{X}'_{t+k}\boldsymbol{\gamma}_2\right)[1 - \mathbb{I}(s_t)] + \varepsilon_{t+k}, \tag{6} $$

where $\mathbb{I}(s_t)$ is an indicator function, taking value of 1 if period t is part of a GDP expansion and 0 otherwise. Phases of expansion and contraction are here estimated through a Bry-Boschan quarterly algorithm (Harding and Pagan, 2002), implemented recursively on GDP vintage estimates

[3] Equation (5) is obtained by subtracting y_t^{t+k} from both sides of Equation (4), such that the dependent variable is the vector of k-th REM revisions defined in (2c). Hence, the βs in the two equations have a different interpretation.

[4] It is nevertheless worth noticing that rejecting the hypothesis of "news" revisions in this framework does not necessarily imply accepting the hypothesis of "noise" revisions. This is because the two null hypotheses are mutually exclusive but not collectively exhaustive, meaning that both may be simultaneously rejected (see Aruoba, 2008, and Jacobs and van Norden, 2011).

y_t^{T+k} available at the time of release of announcements y_t^{t+k}. The procedure is based on the detection of turning points in real-time, and it is hence consistent with a forecast rationality testing approach, because estimated business cycle phases enter the information set of agents at time $t + k$.[5]

Business cycle symmetry in the revision process can be easily tested through a Wald test on parameters restrictions by setting the null hypothesis $\alpha_1 = \alpha_2$, with $\beta_1 = \beta_2 = 0$ and $\boldsymbol{\gamma}_1 = \boldsymbol{\gamma}_2 = 0$ imposed. When testing for the rationality of announcements, we proceed sequentially as follows: first, we test for business cycle symmetry; second, if the symmetry hypothesis can be rejected, we test for unbiasedness and efficiency while imposing the non-linearity implied by Equation (6), *i.e.* we set respectively the joint null hypotheses $\alpha_1 = \alpha_2 = \beta_1 = \beta_2 = 0$, with the imposed restriction that $\boldsymbol{\gamma}_1 = \boldsymbol{\gamma}_2 = \mathbf{0}$, and $\alpha_1 = \alpha_2 = \beta_1 = \beta_2 = 0$ and $\boldsymbol{\gamma}_1 = \boldsymbol{\gamma}_2 = \mathbf{0}$, with no imposed restrictions. This procedure should allow us to ascertain whether the rejection of the hypothesis of data rationality (if any) depends on systematic predictive bias and/or inefficiency during a specific phase of the cycle. Further, the sign and the magnitude of estimated coefficients should allow us to explore the importance of asymmetry in the data, as well as to assess whether the French NSA do overstate or understate the activity in the business cycle.[6]

2.4. Threshold business cycle asymmetry (BC II)

One drawback of the business cycle analysis above is that the sample of expansion and contraction periods is usually unbalanced towards the former (see also footnote 5 on the issue of real-time detection of turning points through the Bry-Boschan quarterly algorithm). One way to address this issue is to investigate the revision process and test for data rationality conditionally on the speed, rather than the phase, of the business cycle. This analysis should shed light on whether preliminary overstate or understate the activity when GDP growth is positive or negative. More formally, we can check for the effect of the speed of business cycle on data rationality by the means

[5] The definition of turning points follows here the standard peak-and-trough characterization of the business cycle (Burns and Mitchell, 1946). Estimated business cycle phases are quite stable across vintages and broadly consistent with to those provided by alternative sources (for instance, the CEPR Dating Committee and Cotis and Coppel, 2005). However, turning points estimated in real-time may differ overtime from official dates. This is because the latter are not usually known until the end of a contraction period. Further, backcasts of GDP series following base-year changes and the introduction of major accounting innovations (such as with the introduction of the ESA 2010 in 2014) may also alter the detection of previously estimated turning points. An example is given by the contraction phase in Q4 2002-Q2 2003 (reported, for instance, by Cotis and Coppel, 2005), which disappeared from recent vintages of GDP. Finally, given the definition of business cycle employed here, our real-time approach may have the drawback of detecting recessions with one quarter lag. Alternative approaches for detecting turning points in real-time (Markov switching models, current-depth-of-recession indicators) are available in the literature but not considered for ease of analysis.

[6] Roughly speaking, if β_1 is positive (negative) and β_2 is negative (positive), this means that preliminary announcements understate (overstate) GDP growth during expansion periods and conversely overstate (understate) GDP growth during contraction periods. Of course, preliminary announcements can also systematically understate (overstate) GDP if the sign of both β_1 and β_2 is positive (negative) and the size of these coefficients differs across the business cycle.

of the following model:

$$
{}_{\ell}y_t^k = \left(\alpha_1 + y_t^{t+k}\beta_1 + \mathbf{X}'_{t+k}\boldsymbol{\gamma}_1\right)\mathbb{I}(y_t^{T+k};c) + \left(\alpha_2 + y_t^{t+k}\beta_2 + \mathbf{X}'_{t+k}\boldsymbol{\gamma}_2\right)\left[1 - \mathbb{I}(y_t^{T+k};c)\right] + \varepsilon_{t+k}, \quad (7)
$$

where $\mathbb{I}(y_t^{T+k};c)$ is an indicator function, taking value of 1 if the vintage estimate $y_t^{T+k} > c$ and 0 otherwise, with $c = 0$ imposed. The testing strategy (symmetry, rationality) is then implemented as described in Section 2.3.

2.5. Business cycle acceleration (BC III)

Alternatively, business cycle features may be accounted for by testing for rationality conditionally on the acceleration of GDP. For this aim, we use a transformation of y_t^{t+k} (the acceleration factor) supposed to capture the speed at which the economy is expanding or contracting (Dynan and Elmendorf, 2001; Bishop et al., 2013):

$$
\Phi(y_t^{T+k}) = y_t^{T+k} - \left(y_{t-1}^{T+k}\right)^{1/2}\left(y_{t-2}^{T+k}\right)^{1/2},
$$

where $\Phi(y_t^{T+k})$ is estimated recursively on GDP vintage estimates y_t^{T+k} available at the time of release of announcements y_t^{t+k} and involves a weighted geometric average, intended to smooth past growth rates of GDP. Finally, $\Phi(y_t^{T+k})$ is recoded as an indicator function $\left(\tilde{\Phi}(y_t^{t+k})\right)$, taking value of 1 if $\Phi(y_t^{T+k}) > 0$ and 0 otherwise. Finally, we can check for the effect of the acceleration of business cycle on data rationality by the means of the following model:

$$
{}_{\ell}y_t^k = \left(\alpha_1 + y_t^{t+k}\beta_1 + \mathbf{X}'_{t+k}\boldsymbol{\gamma}_1\right)\tilde{\Phi}(y_t^{T+k}) + \left(\alpha_2 + y_t^{t+k}\beta_2 + \mathbf{X}'_{t+k}\boldsymbol{\gamma}_2\right)\left[1 - \tilde{\Phi}(y_t^{T+k})\right] + \varepsilon_{t+k}. \quad (8)
$$

The testing strategy (symmetry, rationality) is then implemented as described in Section 2.3.

2.6. Testing for changes in the French NSA performance

Revisions process and rationality features may evolve over-time according to changes in the accounting performance of NSAs, such as improvements in the collection and processing of input data for the construction of quarterly National Accounts. Examples of these improvements may be found in the refining of seasonal adjustment procedures, inclusion of new early indicators conveying information about current activity in specific sectors, and reshaping of the accounting methodology. Changes can be abrupt and/or smooth, depending on whether they take place, for instance, in the context of (infrequent) introductions of major accounting innovations or progressively through benchmark revisions. The former can be compared to breaks in the revision process and in the forecast rationality, while the latter is consistent with time-variation. The presence of breaks in our data is investigated by the means of a test for multiple structural changes, where the number of breaks is estimated by implementing the sequential Sup$F$ test procedure suggested by Bai and

Perron (1998, 2003). More formally, for m estimated breaks and $m+1$ partitions of the data, the auxiliary forecast rationality regression takes the following form:

$$ {}_{\ell}y_t^k = \left(\alpha_1 + y_{t,1}^{t+k}\beta_1 + \mathbf{X}'_{t+k,1}\boldsymbol{\gamma}_1\right) + \cdots + \left(\alpha_{m+1} + y_{t,m+1}^{t+k}\beta_{m+1} + \mathbf{X}'_{t+k,,m+1}\boldsymbol{\gamma}_{m+1}\right) + \varepsilon_{t+k}. \quad (9) $$

If the null hypothesis of no structural changes is rejected, the testing strategy (rationality) is implemented as described is Section 2.3 using Equation (9).

Smooth changes may imply a gradual increase in the efficiency of announcements over-time. This hypothesis is investigated here by the means of the joint test for parameter instability in Equation (5) proposed by Nyblom (1989) and Hansen (1992) (the L_C test statistic). We then take the result of this test as a rough evidence of the presence of parameters instability.[7]

2.7. Robust testing and nonlinear dependence

A crucial issue in the analysis described above is the construction of robust test statistics for the relevant parameters (t-statistic) and the restrictions of the rational forecast hypothesis (F-statistic). Since GDP revisions often display apparent autocorrelated structure and changing volatility (Aruoba, 2008), it seems reasonable to compute heteroskedasticity and autocorrelation consistent (HAC) estimates of the residuals covariance matrix. However, as pointed out by Andrews (1991), Andrews and Monahan (1992) and Den Haan and Levin (1997), the use of HAC estimators can lead the tests to substantial size distortions in finite samples. To attenuate this issue, we compute HAC estimates of the covariance matrix following the approach suggested by Kiefer et al. (2000) and Kiefer and Vogelsang (2002a,b). This approach consists in computing HAC standard errors using a Bartlett kernel without truncation, with bandwidth equal to the sample size (see Kiefer and Vogelsang, 2005, for a more general discussion on the underlying fixed-b asymptotic theory). The resulting test statistics t^* and F^* are asymptotically invariant to nuisance parameters, but have non-standard distributions that depend on the number of restrictions q being tested:

$$ t^* \Rightarrow W_1(1)\left[2\int_0^1 B_1(r)^2 \mathrm{d}r\right]^{-\frac{1}{2}} \qquad \text{and} \qquad F^* \Rightarrow W_q(1)'\left[2\int_0^1 B_q(r)B_q(r)'\mathrm{d}r\right]^{-1} W_q(1)/q, $$

where $B_q(r) = W_q(r) - rW_q(1)$ and $W_q(r)$ is a $q \times 1$ vector of independent Brownian motions. We use standard Monte Carlo techniques to simulate these distributions and to compute critical and probability values.

Finally, we point out that, in the present linear regression framework, failures to reject the null hypothesis of rationality only imply the absence of *linear* correlation in Equation (5), because the testing approach described in Section 2.2 does not rule out the presence of *nonlinear* dependence.

[7] The test is valid against the alternative of a martingale process. An alternative approach based on recursive regressions and the class of CUSUM tests is not considered here, mainly because of the low power of these tests well documented in the literature.

Cognizant of this issue, we compare our results with those of alternative rationality tests consistent against generic nonlinearity by implementing the testing procedure described by Corradi et al. (2009). The proposed test statistic takes the form:

$$M_T = \sup_{\boldsymbol{\lambda}\in\Lambda} |m_T(\boldsymbol{\lambda})| \,,$$

where $m_T(\boldsymbol{\lambda}) = \frac{1}{\sqrt{T}} \sum_{t=1}^{T-2} {}_\ell y_t^k \times w \left(\sum_{j=0}^{t-1} \boldsymbol{\lambda}_j' \Psi(\mathbf{Z}_{t+k}) \right)$ and $\mathbf{Z}_{t+k} = \left(y_t^{t+k}, \mathbf{X}_{t+k} \right)'$. Following Corradi and Swanson (2002) and Corradi et al. (2009), we set $\lambda_{i,j} = \lambda_i (j+1)^{-2}$, $w(\cdot)$ as the exponential function, and $\Psi(\cdot)$ as the inverse tangent function. The limiting distribution of this statistic depends on the vector of nuisance parameters $\boldsymbol{\lambda} \in \Lambda$ (the set of model parameters spanning a multidimensional grid), and thus standard critical values are not available. Corradi et al. (2009) suggest to implement a bootstrap procedure to obtain critical and probability values, leading to a bootstrap analog of M_T, namely $M_T^* = \sup_{\boldsymbol{\lambda}\in\Lambda} |m_T^*(\boldsymbol{\lambda})|$, where $m_T^*(\boldsymbol{\lambda}) = \frac{1}{\sqrt{T}} \sum_{t=1}^{T-2} \left({}_\ell y_t^{k*} \times w^*(\boldsymbol{\lambda}) - {}_\ell y_t^k \times w(\boldsymbol{\lambda}) \right)$, and ${}_\ell y_t^{k*}$ and $w^*(\boldsymbol{\lambda})$ are bootstrap resampled series.

3. The data

Seasonally-adjusted vintages of quarterly French GDP growth rates are collected in a real-time data trapezoid, with first observation in Q1 1980 and initial announcements spanning from Q1 1991 to Q1 2015 (97 observations). The French NSA (INSEE hereafter) releases two initial announcements of GDP within the same quarter: the first one (named "preliminary estimates") provides an early estimate of transactions on goods and services, while the second one (named "detailed estimates") provides an early estimate of agent accounts. Since GDP values are usually only slightly revised between these two initial releases, in what follows we consider exclusively vintages of "preliminary estimates". The subsequent analysis is then carried out on announcements and revisions located in the upper-triangular part of the square matrix obtained by dropping observations anterior to Q1 1991 from this adjusted trapezoid.[8]

Vintage GDP estimates are periodically affected by a few structural revisions, the so-called "benchmark revisions". Unlike regular revisions, which add news or reduce noise, benchmark revisions usually involve base-year changes, weighting changes, seasonal adjustment refining, introduction of new indicators for the calibration of quarterly measures, and general reshaping of the accounting methodology. These revisions are unpredictable by construction and they do not represent any news or noise component, so that they should be offset by the econometrician. The solution proposed in this paper is to identify benchmark revisions endogenously and directly into the auxiliary regressions estimated to perform the analysis described in Section 2. To do so, we rely

[8] It is worth noticing that in 2016 INSEE has started to release three initial announcements of GDP, similarly to the BEA (US) and the ONS (UK): "flash estimates" about 30 days after the end of the quarter, followed by "preliminary estimates" and "detailed estimates", respectively about 60 and 90 days after the end of the quarter.

on the recent literature on impulse dummy saturation (Santos et al., 2008; Johansen and Nielsen, 2009; Castle et al., 2012), implemented in the *Autometrics* algorithm (Doornik, 2009).[9] In a nutshell, the impulse dummy saturation approach is implemented as follows: the sample is split in multiple blocks of observations, and an indicator dummy variable is added for each observation in each block; blocks are analyzed sequentially and significant dummy variables are retained for each block separately; finally, blocks are merged and the selected dummies retained in the previous step are tested again for statistical significance. In the present paper we chose the significance level of 1%, in order to avoid overselection of impulse dummies. The selected dummies are expected to capture significant benchmark revisions, as well as those extreme revisions ("irregular revisions") that are strictly related to specific GDP growth episodes we can consider as outliers, such as the general strike in 1995 and the *Great Recession* in 2008-2009. However, in order to robustify our analysis, we check manually the output of the automatic selection procedure and we discard those dummy variables that are not consistent with the recorded benchmark and irregular revisions.

In the literature, the series of final announcements ($y_t^{t+\ell}$) is usually defined as either a specific vintage estimate, such as the latest available vintage (where $\ell = \infty$), or alternatively a series constructed in such a way that there are no further revisions (excluding benchmark revisions) after a finite number of periods. While the first option is not necessarily optimal, due precisely to benchmark revisions, the second one is more appealing because ℓ can be set as to replicate the calendar of revisions followed by NSAs (Aruoba, 2008).[10] For this reason, it is often reasonable to set a value for ℓ ranging between 12 and 14. An interesting but problematic issue with French GDP vintages is that this definition of final announcements, *i.e.* a vintage immune from further (regular) revisions, does not seem to apply to actual data. This means that values released in old vintages keep being substantially revised even in more recent vintages. Causes for such behavior of the data can be found, for instance, in the continuous updating of the seasonal-adjustment procedures and of the backcasting methodology. To address this issue, we propose to compute the vintage of final announcements by following the official calendar of regular revisions employed by INSEE: quarterly GDP values released in year T can be considered as fully revised when the quarterly vintage series consistent with "final" annual accounts for that year is released. However, the timing of release of these final annual accounts-consistent quarterly vintages has substantially changed overtime, leading to a mismatch in our sample: between 1991 and 1999, in the second quarter of year $T + 4$ with the release of the "detailed estimates" of GDP for Q4 of year $T + 3$; between 1999 and 2005, in the second quarter of year $T + 3$ with the release of the "detailed estimates" for Q4 of year $T + 2$; and from 2005 onward, in the second quarter of year $T + 3$ with the release of the "preliminary estimates" for Q1 of year $T + 3$. Hence, an additional issue arises, because scheduled final announcements have been historically released with either preliminary or detailed estimates

[9] A similar approach is implemented by Hecq et al. (2016).

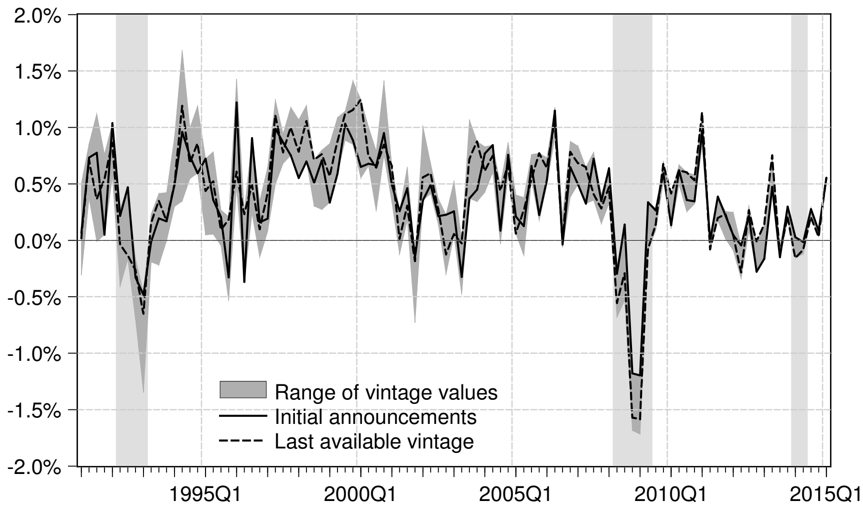
[10] For instance, the BEA follows a schedule of 13 revisions, so that a GDP value becomes "final" after 14 releases.

of quarterly GDP, depending on the year. In order to circumvent the practical problem associated with mixing two different databases (preliminary and detailed estimates), we address this issue by considering only final announcements released with preliminary estimates. This is done by replacing final announcements released with detailed estimates between 1991 and 2005 with their respective values obtained from the vintage series released with the preliminary estimates of the following quarter. It is worth noticing that following the official calendar to set ℓ implies that the number of recorded revisions between initial and final announcements varies across years and quarters: between 1991 and 1995, 16 for Q1, 15 for Q2, 14 for Q3, and 13 for Q4; from 1996 onward, 12 for Q1, 11 for Q2, 10 for Q3, and 9 for Q4. In the subsequent analysis we thus set ℓ consistently with this schedule, also meaning that the last observation available for the sample of final announcements $y_t^{t+\ell}$ is set to Q4 2012.

Figure 1 reports initial announcements of GDP and the last available vintage series (released in Q2 2015), as well as the range of historical vintage values. A visual inspection of the series suggests that, excepting for a few observations (mostly concentrated at the beginning of the sample and related to the general strike episode at the end of 1995, as well as during the *Great Recession* episode), initial announcements and last vintage data are relatively close. However, the range of historical values appears quite large, such as for the 1992-1993 recession, and points to a pattern of substantial variation across vintages. Figure 2 reports total GDP revisions (${}_{\ell}y_t^1$) computed as the difference between the initial announcements and either the last available vintage series ($\ell = \infty$) or the series of final announcements constructed using the revision schedule described above. These two series have overall similar unconditional moments (tests do not reject the hypotheses of equal mean and variance), but display sometimes noticeable differences in terms of both size and sign.

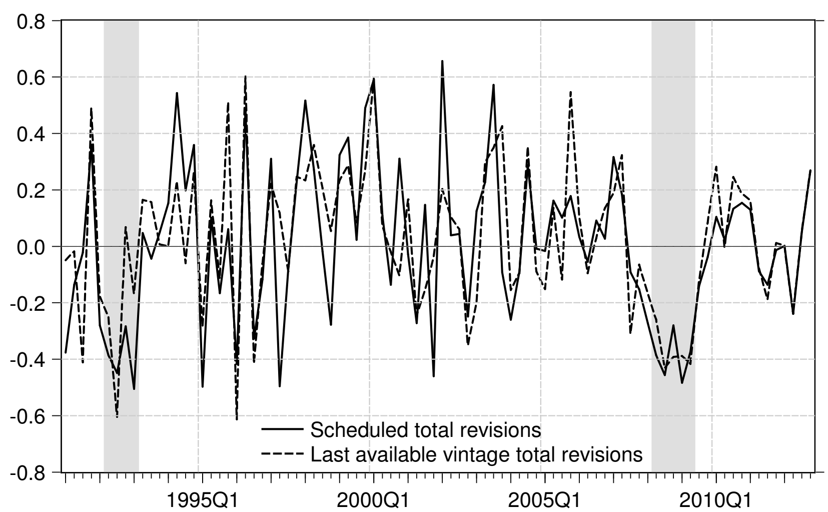
The path of the main k-th revisions considered alongside the present paper ($k = 1, 2, 3, 4, 8, \ell-1$) can be roughly explored in Figure 3, which reports boxplots of the distribution of FXW, INW and REM revisions, respectively, described by Equations (2a), (2b) and (2c). From the boxplots, we can notice that the mean and the median of revisions are usually close to zero, while the variance tends to decrease or increase consistently with the order and the nature of revisions. For instance, the size of FXW revisions is quite small (ranging between ± 0.2) and the variance seems to decrease progressively, since the accuracy of the k-th announcements with respect to final announcements is expected to increase. An intriguing exception is represented by final revisions, for which the dispersion seems to increase compared to the previous steps. Preliminary results (not reported) revealed that this finding can be attributed to our choice of ℓ, which is set to change across years and quarters. Fixing ℓ to, say, 13 is sufficient to attenuate this issue, although final revisions keep displaying an unexpected large volatility. On the other hand, as expected the variance of INW revisions tends to increase (see Figure 3b), while that of REM revisions tends to decrease (see Figure 3c). Further, the empirical distribution of ${}_{\ell}y_t^{t+1}$ indicates that total revisions range in the interval ± 0.6 percentage points, which is quite large compared to the average of initial announcements (about 0.4%).

Figure 1: Initial announcements, last available vintage, and range of vintage values for the French real GDP quarterly growth rate (Q1 1991-Q1 2015)



Notes: vertical shaded areas denote official recessions dates.

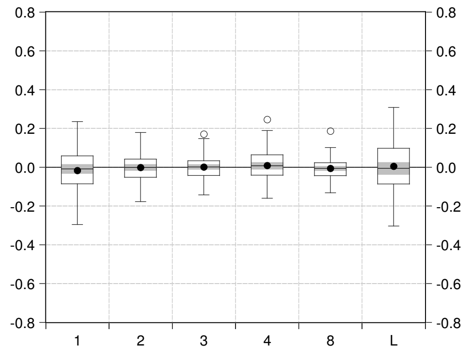
Figure 2: Total revisions (${}_{\ell}y_t^1$) of French real GDP quarterly growth rate (Q1 1991-Q4 2012; in percentage points): scheduled and last available vintage ($\ell = \infty$)



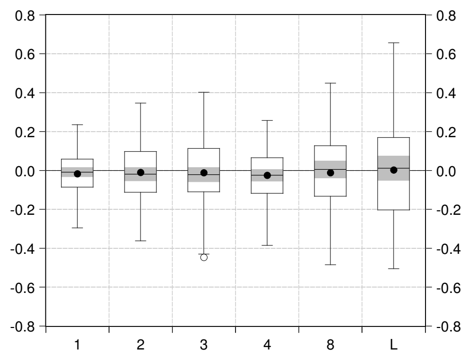
Notes: vertical shaded areas denote official recessions dates.

Figure 3: Boxplot of French real GDP quarterly growth rate revisions

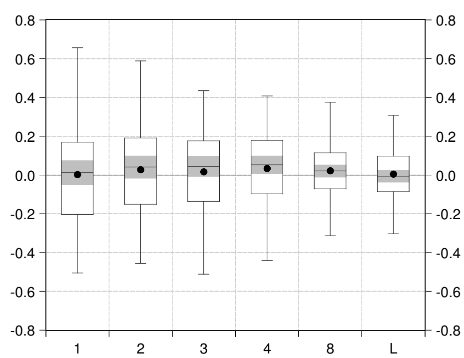
(a) Fixed-width revisions ($_{k+1}y_t^k$)



(b) Increasing-width revisions ($_{k+1}y_t^1$)



(c) Remaining revisions ($_{\ell}y_t^k$)



Notes: Data are adjusted for benchmark and irregular revisions (see Section 3 for details). The box denotes the first and third quartiles. The staples denote the last data point within (or equal to) these quartiles ±1.5 IQR, respectively. Shaded areas denote approximate 95% confidence intervals for the median (the line inside the box). Full dot denotes the mean, empty dots denote the outliers. The x-axis denotes the k-th revision, with $\mathrm{L} = \ell - 1$.

4. Empirical results

4.1. GDP Revisions: main statistical features

We start our empirical analysis by investigating the main statistical properties of French GDP revisions. In what follows, we focus on total revisions (${}_{\ell}y_t^1$), first revisions (${}_{2}y_t^1$), final revisions (${}_{\ell}y_t^{\ell-1}$), and one-step FXW revisions for $k = 2, 3, 4, 8$. The whole set of results (including REM and INW revisions for $k = 1, 2, 3, 4, 8, \ell-1$) are reported in an online appendix available on the web-page of the authors. Table 1 reports main descriptive statistics. Average revisions (μ) appear overall small and statistically not different from zero, which means that revisions are broadly unconditionally unbiased. As expected, volatility (σ) and mean absolute revisions ($|\mu|$) are quite high for total revisions (respectively, 0.28 and 0.22), but they are a decreasing function of k for FXW revisions (except for $k = \ell - 1$; see the discussion in Section 3). These findings are consistent with the increasing rate of accuracy of later announcements, as new available data convey progressively less information for the refining of GDP estimates (see also Figure 3). Further, according to the Jarque-Bera test, revisions appear overall normally distributed.

Table 1: Main descriptive statistics: mean (μ) and volatility (σ) of GDP revisions

	${}_{\ell}y_t^1$	${}_{k+1}y_t^k$					
		$k=1$	$k=2$	$k=3$	$k=4$	$k=8$	$k=\ell-1$
T	88	96	95	94	93	89	88
μ	0.00 (0.02)	-0.02 (0.01)	0.00 (0.01)	0.00 (0.00)	0.01 (0.01)	-0.01 (0.00)	0.01 (0.01)
σ	0.28	0.12	0.07	0.07	0.08	0.06	0.13
$\lvert\mu\rvert$	0.22	0.09	0.06	0.05	0.06	0.05	0.10
JB	0.56	0.77	0.97	0.81	0.78	0.10	0.88
$\mu/\mu_{y_t^{t+k+1}}$	0.01	-0.05	0.00	0.00	0.03	-0.02	0.01
$\sigma/\sigma_{y_t^{t+k+1}}$	0.50	0.25	0.15	0.14	0.15	0.10	0.23

Notes: JB denotes p-values for the Jarque-Bera test of normality. HAC standard errors in parentheses. ***, ** and * denote statistical significance at the 1, 5 and 10% levels, respectively, for the t^* statistics.

Since the unconditional moments of revisions may not be very informative *per se*, we also report revisions-to-announcements and noise-to-signal ratios, computed respectively as the mean/standard deviation of revisions divided by the mean/standard deviation of announcements. We propose these statistics to shed light on the relative size of revisions (in terms of first two moments) compared to the underlying original data (Aruoba, 2008). As expected, results point to negligible average revisions compared to mean announcements. However, noise-to-signal ratios are more substantial: 0.50 for total revisions, 0.25 for first revisions, and decreasing slowly at 0.10 up to $k = 8$ (final

Table 2: Mean GDP revisions and the business cycle

	${}_{\ell}y_t^1$	${}_{k+1}y_t^k$					
		$k=1$	$k=2$	$k=3$	$k=4$	$k=8$	$k=\ell-1$
		Business Cycle Asymmetry I					
$F^*(\mu_1=\mu_2)$	0.09	0.99	0.58	0.39	0.41	0.00	0.66
μ_1	0.01 (0.01)	–	–	–	–	0.00 (0.00)	–
μ_2	-0.22 * (0.05)	–	–	–	–	-0.06 *** (0.01)	–
		Business Cycle Asymmetry II					
$F^*(\mu_1=\mu_2)$	0.09	0.45	0.73	0.22	0.05	0.17	0.21
μ_1	0.02 (0.02)	–	–	–	0.01 (0.01)	–	–
μ_2	-0.11 (0.03)	–	–	–	-0.01 (0.01)	–	–
		Business Cycle Asymmetry III					
$F^*(\mu_1=\mu_2)$	0.90	0.67	0.08	0.50	0.13	0.99	0.74
μ_1	–	–	-0.01 (0.01)	–	–	–	–
μ_2	–	–	0.01 (0.01)	–	–	–	–

Notes: HAC standard errors in parentheses. F^* denotes p-values of the indicated null hypothesis. ***, ** and * denote statistical significance at the 1, 5 and 10% levels, respectively, for the t^* statistics.

revisions display an excess noise-to-signal ratio).[11] Hence, findings suggest that the volatility of revisions may be sizeable compared to that of announcements, which implies that one desirable property of revisions (σ small) is broadly not supported by our data.

Table 2 reports results on the correlation of revisions with the business cycle. According to the Wald test for the hypothesis of equal mean revisions across standard business cycle phases (Business Cycle I in the table), the null can be rejected (at 10% confidence level) for total revisions and for eight revisions. Results point to zero mean revisions in phases of expansion (μ_1) and to negative mean revisions in phases of contraction (μ_2), the latter suggesting an overestimate of GDP during recessions. In particular, the mean bias during recessions is particularly strong for total revisions (-0.22), suggesting that in these phases initial announcements may suffer from a systematic overestimation compared to their final announcements. We nevertheless show in the appendix that the bias disappears after the initial announcement. Turning to a threshold definition of the business cycle (Business Cycle II in the table), the hypothesis of equal mean revisions across positive and negative growth phases can be rejected again for total revisions and for fourth revisions. Similarly to recessions, results point to overestimates of GDP during phases of negative growth rates, but now the estimated coefficients are not statistically significant at any standard confidence level.

[11] Since $\sigma_{{}_h y_t^k}/\sigma_{y_t^{t+h}} = \sqrt{1-\left[\sigma^2_{y_t^{t+k}}\left(1-2\rho\sigma^{-1}_{y_t^{t+k}}\sigma_{{}_h y_t^k}\right)/\sigma^2_{y_t^{t+h}}\right]}$, the noise-to-signal ratio is bounded below by zero, but not necessarily bounded above by unity due to the sign of the coefficient ρ, that denotes the correlation between y_t^{t+k} and ${}_h y_t^k$.

Table 3: Volatility of GDP revisions and the business cycle

	${}_{\ell}y_t^1$	${}_{k+1}y_t^k$					
		$k=1$	$k=2$	$k=3$	$k=4$	$k=8$	$k=\ell-1$
			Business Cycle Asymmetry I				
$F^*(\sigma_1=\sigma_2)$	0.12	0.42	0.09	0.69	0.50	0.27	0.61
σ_1	–	–	0.08	–	–	–	–
σ_2	–	–	0.05	–	–	–	–
			Business Cycle Asymmetry II				
$F^*(\sigma_1=\sigma_2)$	0.08	0.30	0.01	0.62	0.43	0.16	0.43
σ_1	0.27	–	0.08	–	–	–	–
σ_2	0.32	–	0.04	–	–	–	–
			Business Cycle Asymmetry III				
$F^*(\sigma_1=\sigma_2)$	0.42	0.52	0.09	0.58	0.53	0.59	0.10
σ_1	–	–	0.08	–	–	–	–
σ_2	–	–	0.07	–	–	–	–

Notes: HAC standard errors in parentheses. F^* denotes p-values of the indicated null hypothesis. ***, ** and * denote statistical significance at the 1, 5 and 10% levels, respectively, for the t^* statistics.

Finally, when we consider the acceleration of GDP as a definition of the business cycle (Business Cycle III in the table), we only find weak evidence of asymmetry for third revisions, with estimated unconditional means statistically not different from zero.

Table 3 reports results on the correlation of the volatility of revisions with the business cycle. According to Wald tests, the hypothesis of equal volatility across the business cycle (BC I) can be rejected (at 10% level) for the second revisions, with slightly larger volatility during phases of expansion (σ_1). When a threshold definition of the business cycle is considered (BC II), the null can be also rejected for total revisions. For the latter, the results suggest higher volatility of revisions during phases of negative GDP growth (σ_2). When acceleration is considered (BC III), results do not clearly suggest any significant asymmetry in volatility. All in all, combining these results with those reported in Table 2, total revisions appear characterized by higher volatility, in addition to overestimation bias, when the initial announcement is a negative growth rate.

Finally, results for the stability of the mean and volatility of revisions are reported in Table 4. According to structural changes test results, the former hypothesis cannot be rejected for all revisions (The maximum number of breaks is set to $M = 3$). These findings would suggest that the unconditional mean of revisions does not seem affected by the numerous recorded methodological changes undergone by National Accounts overtime. However, evidence of structural changes is substantially stronger for the volatility of revisions. In particular, first and third revisions appear substantially less volatile since the recover from the *Great Recession* episode (σ_2). Total revisions appear also less volatile since the beginning of the 2000s. Interestingly, final revisions display two breaks located in the mid-90s, with a second regime substantially more volatile. This finding may be in part attributed to the correction of the sizeable GDP growth swings reported in preliminary

Table 4: GDP revisions and structural changes

	${}_{\ell}y_t^1$	${}_{k+1}y_t^k$					
		$k=1$	$k=2$	$k=3$	$k=4$	$k=8$	$k=\ell-1$
Structural changes test							
Sup$F(\mu)$	–	–	–	–	–	–	–
Sup$F(\sigma)$	1 [03Q4]	1 [09Q2]	–	1 [10Q3]	–	–	2 [95Q1,98Q2]
σ_1	0.32	0.13	–	0.07	–	–	0.09
σ_2	0.21	0.07	–	0.04	–	–	0.20
σ_3	–	–	–	–	–	–	0.12
Time-varying test							
L_C	0.03 [σ]	0.02 [σ]	0.11	0.16	0.10	0.08 [σ]	0.08 [σ]

Notes: Sup$F$ denotes the estimated number of breaks (dates are reported in brackets). L_C denotes p-values for the test of the joint null hypothesis of parameters stability (in brackets the parameters for which stability is rejected individually).

announcements and related to the general strike at the end of 1995. The large volatility of final revisions observed in Figure 3a may be hence also explained by this particular feature of the data in the mid-90s. Turning to time-varying parameters tests, the results are broadly consistent with the findings reported above: the hypothesis of stability can be more frequently rejected for the volatility of revisions than for mean revisions. All in all, results suggest that the volatility of revisions has decreased during the last decade, supporting the hypothesis of a gradual improvement in the accuracy of GDP announcements released by INSEE.

4.2. Data rationality

As discussed in Section 2.2, we break down the hypothesis of forecast rationality into a conditional unbiasedness hypothesis and an efficiency hypothesis. We start with the test of conditional unbiasedness of announcements, by estimating Equation (5) with the imposed restriction $\boldsymbol{\gamma} = \mathbf{0}$ and setting the joint null hypothesis $\alpha = \beta = 0$. Wald test results, reported in Table 5, suggest that the null hypothesis cannot be rejected overall. This finding can be attributed to the absence of both systematic prediction errors (*i.e.* $\alpha = 0$) and significant correlation between GDP revisions and announcements (*i.e.* $\beta = 0$). Business cycle symmetry is tested as described in Sections 2.3, 2.4, and 2.5. The null hypothesis of absence of peak-and-through asymmetry, $F^*(s)$, can be rejected (at 10% level) for $k = 8$. When the asymmetry is imposed, unbiasedness can be rejected, with some evidence of conditional bias in the expansion phase. When a threshold definition of the business cycle is considered, the null hypothesis of symmetry can be rejected for $k = 1$ and $k = 4$. However, when the asymmetry is imposed, unbiasedness can be strongly rejected as a result of conditional bias in the contraction regime. The null hypothesis of symmetry can also be rejected for $k = 1$ against the alternative of correlation with the acceleration of the business cycle. When the asymmetry is imposed, unbiasedness can be rejected as a result of conditional bias during phases of deceleration of GDP. Structural changes tests provide evidence of parameters instability for $k = 3$,

Table 5: Forecast rationality: unbiasedness $\left({}_{\ell}y_t^k = \alpha + y_t^{t+k}\beta + \varepsilon_{t+k}\right)$

	$k=1$	$k=2$	$k=3$	$k=4$	$k=8$	$k=\ell-1$
			Unbiasedness Regressions			
T	88	88	88	88	88	88
α	-0.03 (0.03)	0.00 (0.02)	0.00 (0.02)	0.03 (0.01)	0.02 (0.01)	0.01 (0.01)
β	0.09 (0.05)	0.09* (0.02)	0.06 (0.03)	0.04 (0.03)	0.00 (0.02)	-0.01 (0.01)
$F^*(\alpha,\beta=0)$	0.67	0.21	0.66	0.35	0.45	0.80
			Business Cycle Asymmetry I			
$F^*(s)$	0.47	0.12	0.41	0.32	0.06	0.99
$F^*(\alpha,\beta=0)$	–	–	–	–	0.03	–
$F^*(\alpha_1,\beta_1=0)$	–	–	–	–	0.09	–
$F^*(\alpha_2,\beta_2=0)$	–	–	–	–	0.14	–
			Business Cycle Asymmetry II			
$F^*(s)$	0.08	0.69	0.14	0.07	0.23	0.34
$F^*(\alpha,\beta=0)$	0.03	–	–	0.02	–	–
$F^*(\alpha_1,\beta_1=0)$	0.74	–	–	0.20	–	–
$F^*(\alpha_2,\beta_2=0)$	0.02	–	–	0.01	–	–
			Business Cycle Asymmetry III			
$F^*(s)$	0.06	0.19	0.41	0.56	0.81	0.26
$F^*(\alpha,\beta=0)$	0.07	–	–	–	–	–
$F^*(\alpha_1,\beta_1=0)$	0.76	–	–	–	–	–
$F^*(\alpha_2,\beta_2=0)$	0.03	–	–	–	–	–
			Structural changes tests			
Sup$F$	–	–	1 [97Q4]	–	–	–
$F^*(\alpha,\beta=0)$	–	–	0.02	–	–	–
$F^*(\alpha_1,\beta_1=0)$	–	–	0.07	–	–	–
$F^*(\alpha_2,\beta_2=0)$	–	–	0.00	–	–	–
			Time-varying tests			
L_C	0.02 [σ]	0.09 [σ]	0.02 [α,β,σ]	0.22	0.25	0.12

Notes: HAC standard errors in parentheses. F^* denotes p-values for the indicated null hypothesis. ***, ** and * denote statistical significance at the 1, 5 and 10% levels, respectively, for the t^* statistic. Sup$F$ denotes the estimated number of breaks (dates are reported in brackets). L_C denotes p-values for the test of the joint null hypothesis of parameters stability (in brackets the parameters for which stability is rejected individually).

with one estimated break date at Q4 1997. When the regime change is imposed, unbiasedness can be rejected, mainly because of conditional bias in the second regime. Time-varying tests also reject the null of stability of regression parameters at $k=3$, as well as at $k=1,2$ due to the volatility of regression residuals.

Testing for efficiency implies relaxing the restriction on $\boldsymbol{\gamma} = \mathbf{0}$. For ease of analysis, we split the vector $\mathbf{X}_{t+k}$, representing the information available at time $t+k$, into two groups of indicators. The first one includes the accounted revisions up to the k-th announcement (*i.e.* the k-th INW revision ${}_k y_t^1$, for $k>1$), with the aim of testing for the rationality hypothesis using the information on the own pattern of revisions. We call this approach a test for *weak efficiency.* The second group ($\mathbf{x}_{t+k}$) includes external indicators known at the time of announcements y_t^{t+k}, such as the spread between yields on long- and short-term Government bonds, the quarterly return on stock market index (SBF 250) and the quarterly change of crude oil (Brent) prices (see, for instance, Faust et al., 2005, and Swanson and Van Dijk, 2006). We call this approach a test for *strong efficiency*. Recent contributions to the literature on nowcasting French GDP pointed out that survey data on services and construction sectors display a fair predictive content when the target variable is implicitly the fully-revised GDP growth rates, while they appear far less useful for forecasting purposes than survey data on manufacturing sector when the target variable is explicitly the initial announcements of GDP growth rates (see Mogliani et al., 2014, and references cited therein for a discussion). It follows that these indicators might carry valuable information on the revision process. We hence test for this assumption by including business indicators on services and construction sectors released by INSEE in the information set $\mathbf{x}_{t+k}$. The data described above are sampled at monthly frequency, with small publication lags (usually, a few days). For a sake of consistency with the timing of the analysis (information available at $t+k$), these variables are measured at the end of quarter $t+k-1$, by taking quarterly averages. Since the INSEE surveys are designed to collect opinions over the past and following three months, we only consider values released at the end of each quarter. Finally, we include in the efficiency regressions a set of centered seasonal dummies $\mathbf{D}^*$, where $D^*_{s,t} = D_{s,t} - D_{4,t}$ and $D_{s,t} = 1$ if time period t corresponds to quarter s and 0 otherwise. We follow Swanson and Van Dijk (2006) and we use estimated seasonal coefficients ($\boldsymbol{\delta}$) to compute a measure of the importance of seasonal effects, which is given by $\delta^* = \sqrt{\sum_{s=1}^{4} \delta_s^2}$ with $\delta_4 = -\sum_{s=1}^{3} \delta_s$.

Results based on efficiency regressions including information on accounted revisions, $\mathbf{X}_{t+k} = {}_k y_t^1$, are reported in Table 6. It is worth noticing that for $k = 1$, INW and REM revisions coincide, so that testing for weak efficiency would be equivalent to testing for conditional bias, and hence results are not reported. For $k > 1$, Wald test results suggest that announcements are overall (weakly) efficient: the null hypothesis $\alpha = \beta = \gamma = 0$ cannot be rejected for all k. This result can be attributed to both unbiasedness of announcements (*i.e.* $\alpha = \beta = 0$) and the absence of significant correlation between GDP revisions and the information conveyed by accounted revisions (*i.e.* $\gamma = 0$). The null hypothesis of symmetry in the efficiency regressions can be rejected for $k = 2, 8$ against peak-and-through business cycle and for $k = 8$ against acceleration business cycle. When asymmetry is imposed, weak efficiency can be rejected, mainly as a result of inefficiency in the contraction regime or phases of deceleration of GDP. The null hypothesis of stability in the regression coefficients can be rejected for $k = 3$ against the alternative of two breaks in Q2 1994 and Q4 1997. When changes are imposed, weak efficiency can be strongly rejected overall, as well as over the three estimated regimes. However, these results should be interpreted with care, because the analysis could be affected by the short length of the sub-samples. Finally, time-varying tests also point to instability in the regression coefficients and volatility of residuals for $k = 3$.

Results based on efficiency regressions including information on external indicators, $\mathbf{X}_{t+k} = \mathbf{x}_{t+k}$, are reported in Table 7. Unlike the findings described above, test results suggest the rejection of the strong efficiency hypothesis. Indeed, the null hypothesis of $\alpha = \beta = \boldsymbol{\gamma} = 0$ can be strongly rejected for all k, except for $k = \ell - 1$, as a result of significant correlation between GDP revisions

Table 6: Forecast rationality: weak efficiency $\left({}_{\ell}y_t^k = \alpha + y_t^{t+k}\beta + {}_{k}y_t^1\gamma + \varepsilon_{t+k}\right)$

	$k=1$	$k=2$	$k=3$	$k=4$	$k=8$	$k=\ell-1$
			Efficiency Regressions			
T		88	88	88	88	88
α		0.00	0.00	0.02	0.02	0.01
		(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
β		0.09	0.06	0.06	0.00	-0.01
		(0.03)	(0.03)	(0.03)	(0.03)	(0.02)
γ		0.00	-0.01	-0.02	0.00	0.00
		(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
$F^*(\alpha,\beta=0)$		0.34	0.65	0.27	0.43	0.80
$F^*(\alpha,\beta,\gamma=0)$		0.37	0.86	0.33	0.66	0.94
			Business Cycle Asymmetry I			
$F^*(s)$		0.09	0.66	0.64	0.00	0.99
$F^*(\alpha,\beta,\gamma=0)$		0.00	–	–	0.00	–
$F^*(\alpha_1,\beta_1,\gamma_1=0)$		0.68	–	–	0.17	–
$F^*(\alpha_2,\beta_2,\gamma_2=0)$		0.05	–	–	0.00	–
			Business Cycle Asymmetry II			
$F^*(s)$		0.86	0.24	0.20	0.14	0.40
$F^*(\alpha,\beta,\gamma=0)$		–	–	–	–	–
$F^*(\alpha_1,\beta_1,\gamma_1=0)$		–	–	–	–	–
$F^*(\alpha_2,\beta_2,\gamma_2=0)$		–	–	–	–	–
			Business Cycle Asymmetry III			
$F^*(s)$		0.34	0.67	0.60	0.02	0.36
$F^*(\alpha,\beta,\gamma=0)$		–	–	–	0.01	–
$F^*(\alpha_1,\beta_1,\gamma_1=0)$		–	–	–	0.23	–
$F^*(\alpha_2,\beta_2,\gamma_2=0)$		–	–	–	0.00	–
			Structural change tests			
Sup$F$		–	2	–	–	–
			$[94Q2,97Q4]$			
$F^*(\alpha,\beta,\gamma=0)$		–	0.00	–	–	–
$F^*(\alpha_1,\beta_1,\gamma_1=0)$		–	0.00	–	–	–
$F^*(\alpha_2,\beta_2,\gamma_2=0)$		–	0.00	–	–	–
$F^*(\alpha_3,\beta_3,\gamma_3=0)$		–	0.01	–	–	–
			Time-varying tests			
L_C		0.12	0.04	0.42	0.44	0.24
			$[\alpha,\beta,\sigma]$			

Notes: See Table 5.

and the selected indicators. Detailed results (not reported) reveal that failure to reject inefficiency can be mainly attributed to the interest rate spread, the quarterly return on stock market, and the survey indicator on services. Furthermore, revisions do not seem to be affected by seasonal patterns, as δ^* is not sizeable compared to the conditional mean. The null hypothesis of business cycle symmetry can be rejected for $k = 2,3,4$ against the alternative of peak-and-through asymmetry. When the asymmetry is imposed, efficiency can be strongly rejected, as a result of inefficiency in both expansion and contraction phases. Further, symmetry can be strongly rejected for all k against the alternative of threshold business cycle. When asymmetry is imposed, the efficiency hypothesis can be systematically rejected, mainly due to inefficiency in negative growth phases. Finally, symmetry can be also rejected for $k = 3,4,8,\ell-1$ against the alternative of acceleration business cycle. When asymmetry is imposed, the efficiency hypothesis can be rejected for $k = 3,4,8,$

Table 7: Forecast rationality: strong efficiency $\left({}_{\ell}y_t^k = \alpha + y_t^{t+k}\beta + \mathbf{x}_{t+k}\boldsymbol{\gamma} + \mathbf{D}_t^{*\prime}\boldsymbol{\delta} + \varepsilon_{t+k}\right)$

	$k=1$	$k=2$	$k=3$	$k=4$	$k=8$	$k=\ell-1$
			Efficiency Regressions			
T	88	88	88	88	88	88
α	0.06 (0.03)	0.06 * (0.01)	0.05 (0.02)	0.07 (0.02)	0.07 ** (0.01)	0.03 (0.01)
β	-0.18 * (0.04)	-0.08 (0.02)	-0.07 (0.05)	-0.09 (0.05)	-0.12 * (0.03)	-0.07 * (0.02)
δ^*	0.08	0.06	0.05	0.05	0.02	0.07
$F^*(\alpha,\beta=0)$	0.09	0.23	0.43	0.14	0.14	0.33
$F^*(\boldsymbol{\gamma}=0)$	0.00	0.00	0.01	0.00	0.04	0.09
$F^*(\alpha,\beta,\boldsymbol{\gamma}=0)$	0.00	0.00	0.01	0.00	0.01	0.15
			Business Cycle Asymmetry I			
$F^*(s)$	0.18	0.03	0.00	0.00	0.11	0.11
$F^*(\alpha,\beta,\boldsymbol{\gamma}=0)$	–	0.00	0.00	0.00	–	–
$F^*(\alpha_1,\beta_1,\boldsymbol{\gamma}_1=0)$	–	0.01	0.00	0.00	–	–
$F^*(\alpha_2,\beta_2,\boldsymbol{\gamma}_2=0)$	–	0.00	0.00	0.00	–	–
			Business Cycle Asymmetry II			
$F^*(s)$	0.09	0.03	0.04	0.01	0.05	0.01
$F^*(\alpha,\beta,\boldsymbol{\gamma}=0)$	0.00	0.00	0.00	0.00	0.00	0.00
$F^*(\alpha_1,\beta_1,\boldsymbol{\gamma}_1=0)$	0.00	0.01	0.13	0.02	0.19	0.21
$F^*(\alpha_2,\beta_2,\boldsymbol{\gamma}_2=0)$	0.00	0.00	0.00	0.00	0.01	0.00
			Business Cycle Asymmetry III			
$F^*(s)$	0.14	0.66	0.01	0.04	0.01	0.04
$F^*(\alpha,\beta,\boldsymbol{\gamma}=0)$	–	–	0.00	0.00	0.01	0.51
$F^*(\alpha_1,\beta_1,\boldsymbol{\gamma}_1=0)$	–	–	0.00	0.00	0.06	–
$F^*(\alpha_2,\beta_2,\boldsymbol{\gamma}_2=0)$	–	–	0.00	0.00	0.04	–
			Structural change tests			
Sup$F$	–	1 [96Q1]	–	2 [00Q1,09Q4]	3 [96Q1,00Q2,03Q3]	2 [01Q3,09Q4]
$F^*(\alpha,\beta,\boldsymbol{\gamma}=0)$	–	0.00	–	0.00	0.00	0.00
$F^*(\alpha_1,\beta_1,\boldsymbol{\gamma}_1=0)$	–	0.00	–	0.03	0.00	0.00
$F^*(\alpha_2,\beta_2,\boldsymbol{\gamma}_2=0)$	–	0.17	–	0.00	0.00	0.00
$F^*(\alpha_3,\beta_3,\boldsymbol{\gamma}_3=0)$	–	–	–	0.00	0.00	0.00
$F^*(\alpha_4,\beta_4,\boldsymbol{\gamma}_4=0)$	–	–	–	–	0.06	–
			Time-varying tests			
L_C	0.35	0.52	0.38	0.75	0.30	0.17

Notes: See Table 5.

again as a result of inefficiency in deceleration phases. Structural change tests provide evidence of parameters instability for $k = 1, 4, 8, \ell - 1$, with the number of breaks ranging between 1 and 3. When regime changes are imposed, strong efficiency can be rejected. However, as before, the size of the sub-samples considered may lead to results that need to be interpreted with care. Conversely, time-varying tests do not provide any significant evidence of parameters instability.

These findings are broadly consistent with those provided by the testing approach proposed by Corradi et al. (2009) and reported in Table 8. Although these results must be considered with some caution (mainly because of the limited number of both nodes in the grid search for $\boldsymbol{\lambda}$ and bootstrap replications, as a consequence of the computational burden growing exponentially with the number of variables in $\mathbf{Z}_{t+k}$), they suggest that the null hypothesis of generic nonlinear weak efficiency

Table 8: Corradi et al. (2009) forecast rationality tests

	$k=1$	$k=2$	$k=3$	$k=4$	$k=8$	$k=\ell-1$
			$\mathbf{Z}_{t+k}=\left(y_t^{t+k},{}_k y_t^1\right)'$			
M_T^*	0.37	0.05	0.16	0.06	0.34	0.35
			$\mathbf{Z}_{t+k}=\left(y_t^{t+k},\mathbf{x}_{t+k}\right)'$			
M_T^*	0.06	0.03	0.03	0.03	0.04	0.04

Notes: M_T^* denotes bootstrap p-values obtained after 999 replications.

cannot be rejected overall (for $k=1$, the null hypothesis is nonlinear unbiasedness). However, the hypothesis of generic nonlinear strong efficiency can be rejected for all k.

All in all, the results reported above suggest that GDP announcements are conditionally unbiased, but inefficient when conditioned on external indicators. Results provide evidence of strong correlation of revisions with macroeconomic and financial indicators, as well as of asymmetry with respect to phases of the business cycle. The failure to reject inefficiency suggests that revisions may reduce statistical noise, rather than contain news. This also means that GDP revisions are technically predictable. In the next section, we investigate this issue by building forecasting models of GDP revisions and performing an evaluation analysis of point and density forecasts.

5. Forecasting GDP revisions in real-time

5.1. *The design of the forecasting experiment*

The out-of-sample analysis is based on recursive regressions over a hold-out sample spanning from T_0=Q1 2004 to T_f=Q4 2012, the latter being consistent with the latest final announcement available. We compute h-step ahead forecasts of GDP revisions using the following generalized forecast rationality specification:

$$
{}_\ell y^k_{t+h} = \alpha + \sum_{j=0}^{4} \beta_j y^{t+k}_{t+h-j} + \sum_{j=0}^{4} \mathbf{X}'_{t+h-j+k}\boldsymbol{\gamma}_j + \mathbf{D}^{*\prime}_t\boldsymbol{\delta} + \mathbf{I}'_t\boldsymbol{\iota} + \varepsilon_{t+k}, \tag{10}
$$

where $\mathbf{I}_t$ denotes a vector of indicator variables capturing benchmark and irregular revisions and estimated recursively through the indicator saturation approach (see Section 3). Further, for ease of notation, $\mathbf{X}_{t+h+k}$ denotes the set of external predictors plus the accounted INW revisions (for $k>1$). This distributed-lag specification has the advantage of providing a more flexible representation of GDP revisions than the rationality regression described in Section 2.2. However, it has the drawback of being heavily parameterized (large number of parameters to be estimated relative to the number of observations), leading potentially to in-sample overfit and poor out-of-sample performance compared to more parsimonious models. To address this issue, we implement a shrinkage method to model selection based on the GEneral-To-Specific (GETS) approach popularized by Krolzig and Hendry (2001) and implemented here through the *Autometrics* algorithm (Doornik, 2009). This model reduction algorithm can deal with the 2^N paths generated by Equation (10), where N is the number regressors (including the deterministic terms), focusing efficiently on a subselection of paths only. Model selection is thus obtained through simplification of the general model by sequentially eliminating those variables failing a battery of forward and backward tests of inclusion in the parsimonious final model. A training sample starting in Q1 1991 is used to select the best model specifications. However, to avoid misspecification issues related to this initial model selection, the specifications are allowed to evolve overtime through a sequential updating every time new final annual accounts are released.

Consistently with the publication lags of the dependent variable implied by our definition of final GDP values (see Section 3), we can show that the real-time (direct) forecast horizon h crucially depends on k. To see why, let us consider the case with $k = 1$. In order to forecast revisions for, say, the year 2004, we have timely information on the whole set of predictors $\mathbf{X}_{t+k}$, but the latest observation available for ${}_{\ell}y_t^k$ is at t=Q4 2001, because only final annual accounts for the year 2001 are known at the time of the release of preliminary announcements for 2004. This implies that the specification of the forecasting model (10) can be performed limiting the training sample to Q4 2001, but the direct forecast horizon h obviously varies across the out-of-sample: from $h = 9$ for Q1 to $h = 12$ for Q4. Let us now consider the case with $k = 2$. When forecasting Q1, Q2, and Q3 we face exactly the same constraints as in the case with $k = 1$. However, it turns out that final annual accounts for year 2002 are known at the time of the second release of Q4 2004, such that the latest observation available for ${}_{\ell}y_t^k$ jumps to Q4 2002. This implies that $h = 9$ for Q1, $h = 10$ for Q2, and $h = 11$ for Q3, but $h = 8$ for Q4. Similar reasoning applies to $k = 3$ and beyond, leading to a sophisticated structure of forecast horizons that we take into account in the subsequent analysis.

5.2. Evaluation

Point and density forecasts are evaluated relatively to the performance of a benchmark ARMA(p,q) model, where the autoregressive and moving-average orders are set by optimizing the BIC criterion and predictions are obtained through iterated forecasts, with h depending on k as described above.[12] Let us denote $\tilde{e}_{t+h+k} = {}_{\ell}y_{t+h}^k - {}_{\ell}\tilde{y}_{t+h}^k$ the forecast errors of the benchmark ARMA model, and $\hat{e}_{t+h+k} = {}_{\ell}y_{t+h}^k - {}_{\ell}\hat{y}_{t+h}^k$ the forecast errors of the selected competing model. The out-of-sample predictive accuracy is then measured in terms of relative root mean squared forecast error (RMSFE):

$$\text{RMSFE}_k = \sqrt{\sum_{\tau=T_0}^{T_f} (\hat{e}_{\tau+h+k})^2 \bigg/ \sum_{\tau=T_0}^{T_f} (\tilde{e}_{\tau+h+k})^2}, \tag{11}$$

[12] We set the maximum AR and MA orders to 4. Further, consistently with the design of the real-time forecasting experiment, the selected ARMA specifications are updated sequentially.

where lower values suggest that the selected model outperforms the benchmark model. Mean squared error differences are evaluated through the Diebold and Mariano (1995) and West (1996) test (DMW hereafter) for unconditional equal predictive accuracy, with a small-sample adjustment to the consistent estimate of the variance proposed by Harvey et al. (1997). It is also interesting to investigate the ability of our models to predict the sign of GDP revisions, other than their values. For this purpose we implement the Pesaran and Timmermann (1992, 2009) sign test (PT hereafter).

Density forecasts $g^k_{t+h}\left({}_\ell y^k_{t+h}\right)$ are computed under the assumption of normality of the probability distribution of predictions. From the sequence of predictive densities, we compute the mean log-predictive score (MLPS) as the realization of the variable evaluated at the out-turn of the probability densities:

$$\text{MLPS}_{y,k} = (T_f - T_0)^{-1} \sum_{\tau=T_0}^{T_f} \log S\left({}_\ell y^k_{\tau+h}\right),$$

where $\log S\left({}_\ell y^k_{t+h}\right) = \log g^k_{t+h}\left({}_\ell y^k_{t+h}\right)$. For our set of predictions ${}_\ell\hat{y}^k_{t+h}$ and ${}_\ell\tilde{y}^k_{t+h}$, we report the relative density performance, $\Delta\text{MLPS}_{y,k} = \text{MLPS}_{\hat{y},k} - \text{MLPS}_{\tilde{y},k}$, such that positive values suggest that the competing model outperforms the benchmark. Further, equal density forecast accuracy is here investigated by the means of a DMW-type test, in the lines of Mitchell and Hall (2005) and Amisano and Giacomini (2007). Density forecasts are also evaluated individually over their probability integral transform (PIT) values:

$$z^k_{t+h} = \int_{-\infty}^{{}_\ell y^k_{t+h}} g^k_{t+h}(u)\mathrm{d}u.$$

If g^k_{t+h} coincides with the sequence of true densities $\bar{g}^k_{t+h}$, then the sequence of PITs z^k_{t+h} is uniformally distributed (Diebold et al., 1998). In this case, the inverse normal transformation of PITs, $z^{k,*}_{t+h} = \Phi^{-1}(z^k_{t+h})$, is normally distributed. To evaluate the calibration of density forecasts, we implement the Doornik and Hansen (2008) test (DH hereafter) to check for normality of the CDF of $z^{k,*}_{t+h}$.

5.3. Results

Results for $k = 1, 2, \ldots, \ell - 1$ are presented in Table 9. It is nevertheless worth noticing that, from the point of view of the policy-maker, results for the case $k = 1$ are certainly more relevant for the conduct of monetary and/or fiscal policy than for $k > 1$. Hence, in what follows we mainly focus our discussion on the former case.

Forecast results for the baseline model presented in Equation (10) are reported in Panel A of Table 9. With respect to point forecasts, we find that the selected specification for $k = 1$ significantly outperforms the ARMA benchmark, with a predictive gain of about 20% (see Figure 4). The PT test rejects at 1% significance level the hypothesis of distributional independence between (the

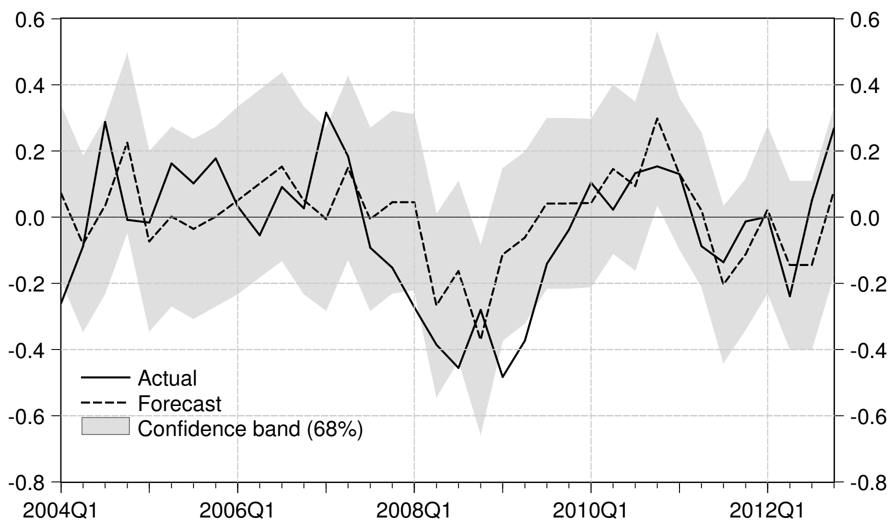
Table 9: Forecast results

	$k=1$	$k=2$	$k=3$	$k=4$	$k=8$	$k=\ell-1$
			A. Baseline model			
RMSFE	**0.82**	0.94	0.88	1.13	1.12	1.19
PT	0.00	0.02	0.11	0.23	0.07	0.61
ΔMPLS	**0.20**	**0.10**	**0.21**	-0.01	-0.05	-0.04
DH	0.86	0.34	0.69	0.04	0.34	0.20
			B. Business Cycle I Asymmetric model			
RMSFE	1.54	0.96	1.23	1.34	1.18	0.99
PT	0.04	0.13	0.19	0.19	0.00	0.99
ΔMPLS	-0.21	0.04	-0.18	-0.21	-0.09	**0.06**
DH	0.00	0.70	0.04	0.00	0.07	0.89
			C. Business Cycle II Asymmetric model			
RMSFE	0.93	1.05	1.09	1.22	1.04	1.26
PT	0.05	0.14	0.33	0.59	0.65	0.71
ΔMPLS	**0.12**	0.04	-0.01	-0.11	0.00	-0.07
DH	0.87	0.90	0.07	0.18	0.48	0.13
			D. Business Cycle III Asymmetric model			
RMSFE	1.05	0.93	0.94	1.16	1.09	1.16
PT	0.00	0.00	0.03	0.93	0.10	0.21
ΔMPLS	-0.01	**0.12**	**0.15**	-0.03	0.04	0.00
DH	0.24	0.78	0.71	0.00	0.39	0.93
			E. Baseline model with AveW forecasts			
RMSFE	**0.84**	0.95	0.90	1.13	1.14	1.21
PT	0.00	0.04	0.12	0.28	0.00	0.61
ΔMPLS	**0.18**	**0.10**	**0.20**	-0.01	-0.06	-0.06
DH	0.93	0.43	0.59	0.05	0.27	0.10

Notes: RMSFE denotes relative root mean squared forecast error (<1 means outperformance of the benchmark model). ΔMPLS denotes relative mean log predictive score (>0 means outperformance of the benchmark model). Bold values denote rejection of the null hypothesis of equal predictive accuracy at 10% level according to the one-sided t-statistic version of the DMW test. PT and DH denote p-values for the Pesaran and Timmermann (1992, 2009) and Doornik and Hansen (2008) tests, respectively.

binary transformation of) ${}_{\ell}y^1_{t+h}$ and ${}_{\ell}\hat{y}^1_{t+h}$, so that the model also displays an interesting predictive power for the sign of total GDP revisions. Good results in terms of RMSFE are also obtained for $k=2,3$, but the DMW test does not reject the hypothesis of equal predictive accuracy with the benchmark. PT test results suggest a significant sign predictive accuracy for $k=2$. For the cases $k=4,8,\ell-1$, results are overall disappointing on the side of both point and sign accuracy. This means that, in spite of the efficiency results reported in the first panel of Table 7, the indicators selected among the vector $\mathbf{X}_{t+h+k}$ have low predictive power for the latest revisions. This is also suggestive of the presence of a large share of news revisions in late announcements of French GDP. With respect to density forecasts, results for relative accuracy suggest a substantial outperformance of the specification at $k=1$. Predictive densities appear also well calibrated according to the DH

Figure 4: Actual and predicted French real GDP quarterly growth rate total revisions ($_{\ell}y_t^1$)



test. The specifications for $k = 2, 3$ also outperform the benchmark in terms of density accuracy, with statistically significant log-predictive score differences. Further, predictive densities appear well calibrated.

All in all, the findings reported above suggest that revisions for $k = 1, 2, 3$ can be predicted using a simple linear model. However, from the results reported in Section 4.2, the hypothesis of efficiency can be also strongly rejected when business cycle asymmetry is imposed to the rationality testing regressions. We can hence exploit this feature and extend the forecasting model in (10) to a non-linear specification. Results are reported in Panel B, C, and D of Table 9. Compared to the findings above, the selected specifications do not significantly improve the forecasting performance of the model for all k: the RMSFE appears often above unity, while the few observed predictive gains are quite small and not statistically significant. Further, the evidence of sign accuracy is not clear-cut. With respect to density forecasts, evidence of relative density outperformance of the benchmark can be found for $k = 1$ when the imposed asymmetry takes the form of a threshold model. Overall, these results point to a poor predictive power for the selected forecasting equations with business cycle asymmetric specifications, with somewhat less disappointing performance provided by the acceleration asymmetric specifications (BC III). A deeper analysis reveals that this outcome can be mainly attributed to large forecast errors in 2008-2009, meaning that the asymmetric specifications cannot accurately predict the dynamic of revisions during the contraction and bounce-back phases of GDP.

Finally, we reported in Section 4.2 evidence of both multiple structural breaks in the parameters of the rationality regressions and rejection of the null of efficiency under the hypothesis of regime changes. As stated by Pesaran and Timmermann (2007), using pre-break observations in the training sample biases the forecast, but may reduce the MSFE. To compute forecasts in real-time while allowing for the presence of structural breaks in the hold-in sample, we implement the AveW procedure suggested by Pesaran and Pick (2011). This consists in averaging forecasts from the same model, but each computed over different estimation windows. Here we combine forecasts from the baseline model (10) estimated over 8 rolling sub-windows within the expanding window described in Section 5.1, with rolling steps of four quarters. The design is consistent with the limited number of observations in the training sample and the evidence of breaks in the first part of the sample. Results, reported in Panel E of Table 9, suggest that the implementation of the AveW procedure does not lead to a quantitatively different predictive (point and density) accuracy compared to the single-window baseline model. Hence, even though the hypothesis of structural changes in the efficiency regressions cannot be rejected (except for $k = 1$), forecast results appear insensitive to the choice of the in-sample window. Reasons for this outcome could be found in the size of the breaks or the design of the AveW procedure, but we do not investigate this issue here.

To summarize, forecast evaluation results suggest that a linear model provide a reasonable predictive accuracy compared to a simple benchmark, mainly for total revision ($k = 1$). However, in spite of what the output of inefficiency tests would have suggested, we show that accounting for business cycle asymmetry in the forecasting equations, as well as for the presence of structural breaks in training samples, does not significantly and systematically improve these results.

6. Concluding remarks

We performed a statistical analysis of French real GDP announcements and revisions, based on quarterly data spanning from 1991 to 2015. Concerning GDP revisions, the results point to a bunch of interesting features. First, mean revisions are not statistically different from zero, meaning that GDP announcements are overall unconditionally unbiased. Second, the volatility of revisions decreases with the order of announcements, which is consistent with the idea that an increasing rate of accuracy of later GDP releases may be driven by either additional news or reduced measurement errors, or even both, conveyed by revisions. Third, we tested for business cycle symmetry of the revision process (mean and variance), using three alternative definition of business cycle, and the hypothesis was often rejected. When asymmetry is imposed, we found out that the statistical properties of total revisions (the difference between final announcements and initial announcements) are substantially different across business cycle regimes, notably during phases of GDP contraction or slowdown. In that case, initial GDP announcements appear overestimated and more volatile. We then performed a further analysis in order to assess the rationality of GDP announcements. Following Faust et al. (2005) and Swanson and Van Dijk (2006), the hypothesis of rationality was broken down into two sub-hypotheses: conditional unbiasedness (revisions are mean-zero and uncorrelated with GDP announcements) and efficiency (revisions are uncorrelated with information available at the time of the announcements). The results point to the unbiasedness of preliminary announcements, but suggest the rejection of the efficiency hypothesis. Accordingly, we performed a real-time forecasting analysis of GDP revisions. The results suggest that reasonably accurate predictions for total revisions can be obtained, although they appear overall quite hard to track in real-time. Several factors, including the predictors entering the model as well as the forecasting model itself, may explain this outcome. This implies that the efficient use of available information in the early stages of GDP measurement could be a rather hard task for data producers, whose primary mission is to provide a consistent and detailed estimate of the economic activity through the system of quarterly National Accounts. We nevertheless believe that our study could stimulate future research on the side of increasing the efficiency of preliminary GDP announcements.

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