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How to treat benchmark revisions? The case of German production and orders statistics

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Abstract

Elements of an econometric examination of benchmark revisions in real-time data are suggested. Structural break tests may be applied to detect heterogeneities within vintages. Systems cointegration tests are helpful to reveal inconsistencies across vintages. Differencing and rebasing, often used to adjust for benchmark revisions, are generally not sufficient to ensure consistent real-time macroeconomic data. Vintage transformation functions estimated by cointegrating regressions are more flexible. Inappropriate conversion may cause observed revision statistics to be affected by nuisance parameters. In German industrial production and orders statistics, remaining revisions are generally biased and serially correlated.

Keywords: real-time data, benchmark revisions; industrial production, orders.

JEL classification: C22, C32, C82.

Non-Technical Summary

Macroeconomic time series are revised for several reasons. Revisions are regularly made when additional information is available. The importance of these regular revision has been extensively discussed in the economic literature. However, time series are also subject to benchmark revisions because new conventions, measurement concepts and survey methods are introduced in the statistical accounting systems. The impact of benchmark revisions on the modeling of macroeconomic time series and on the analysis of regular revisions has been studied to a lesser extent. By three examples of the German statistics on production and orders received (reclassification of economic sectors and products, innovation in the revision procedure, base year changeover), the paper reveals that econometric methods such as structural break tests and cointegration tests as well as cointegration regressions are appropriate to analyze benchmark revisions. In particular, it is proven that differencing and rebasing, albeit often applied, are generally not able to completely adjust real-time data for benchmark revisions.

The European harmonization of sector and production classifications which were implemented in German short-term economic statistics in early 1995 harms the intertemporal comparison of index values before and after the revision because lacking detailed information prevented the statistical authorities from consistently converting the figures which had been published before 1995. Statistical tests provide evidence for a structural break in the time series at this date, affecting the (joint) econometric modeling of industrial production and orders received.

Base year changeovers generally imply that the time series released before and after this benchmark revision are not comparable directly. Simple transformations such as differencing and rebasing are frequently applied to account for such effects in real-time data sets. However, statistical tests reveal that these conversion methods are not appropriate to the statistics under study. Hence, affine transformation functions are estimated by cointegration regressions. The fact that the transformation functions estimated for production and orders differ greatly affect the cointegrating relation between them, suggesting that (pure) base year changeovers, which are typically regarded as a comparably less problematic benchmark revision, may even alter the estimation results of long-run economic relationships.

The goal to closely approximate the transformation functions is also essential for the study of regular revisions. A theoretical analysis proves that revision statistics may be distorted if an inappropriate conversion method is applied. Empirical results on the basis of the real-time data sets of production and orders reaching back to the introduction of the new survey methodology in 1999 confirm the impact of the chosen transformation function on revision statistics. Especially in the case of industrial production where the affine transformation function greatly differ from rebasing, the estimates of the revision mean and the revision variance strongly depend upon the choice of the conversion method.

Furthermore, the empirical results let us conclude that the preliminary releases of production and orders tend to underestimate the final index values. The regular revisions of both indices are well behaved in the sense that the volatility of remaining revisions decreases in the revision number. Finally, some correlations between production and orders revisions are found to be statistically significant.

Nicht technische Zusammenfassung

Makroökonomische Zeitreihen werden aus verschiedenen Gründen revidiert. Revisionen werden regelmäßig dann vorgenommen, wenn zusätzliche Informationen verfügbar sind. Die Bedeutung solcher laufenden Revisionen ist in der wirtschaftswissenschaftlichen Literatur bereits ausführlich diskutiert worden. Zeitreihen unterliegen aber auch Generalrevisionen, weil neue Konventionen, Messkonzepte und Erhebungsverfahren in die statistischen Rechenwerke eingeführt werden. Die Auswirkungen von Generalrevisionen auf die Modellierung makroökonomischer Zeitreihen und auf die Analyse laufender Revisionen sind demgegenüber weit weniger intensiv untersucht worden. Das vorliegende Papier zeigt an drei Beispielen der deutschen Produktions- und Auftragseingangsstatisik (Neufassung der Sektor- und Güterklassifikationen, Änderung der Datenerhebungsmethodik, Basisjahrumstellungen), dass übliche Verfahren der Zeitreihenanalyse wie Strukturbruch- und Kointegrationstests sowie Kointegrationsregressionen geeignet sind, die Wirkungen von Generalrevisionen zu studieren. Insbesondere wird nachgewiesen, dass Differenzierung und Umbasierung – obschon häufig angewendet – grundsätzlich nicht in der Lage sind, Echtzeitdatensätze um Generalrevisionen vollständig bereinigen.

Die europäische Harmonisierung der Sektor- und Güterklassifikationen, die in den deutschen Konjunkturstatistiken zu Beginn des Jahres 1995 umgesetzt wurde, führt zu einer Beeinträchtigung des intertemporalen Vergleichs der Indexwerte vor und nach der Revision, weil es aufgrund fehlenden Datenmaterials nicht gelang, die bis 1994 publizierten Werte vollkommen konsistent auf die neuen Konzeptionen umzurechnen. Statistische Tests belegen die Evidenz für einen Strukturbruch in den Zeitreihen zu diesem Zeitpunkt, was die (gemeinsame) ökonometrische Modellierung von Produktion und Auftragseingang im industriellen Sektor beeinflusst.

Basisjahrumstellungen haben grundsätzlich zur Folge, dass die veröffentlichten Zeitreihen vor und nach dem Revisionszeitpunkt nicht direkt miteinander vergleichbar sind. Um in Echtzeitdatensätzen für solche Effekte zu korrigieren, werden häufig einfache Transformationen wie Differenzierung und Umbasierung angewandt. Wie statistische Tests zeigen, sind diese Methoden für das vorliegende Datenmaterial jedoch ungeeignet. Stattdessen werden mit Hilfe von Kointegrationsregressionen affine Transformationsfunktionen geschätzt. Dass sich für Produktion und Auftragseingang sehr unterschiedliche Schätzungen ergeben, hat Einfluss auf die Parameterwerte der Kointegrationsbeziehung zwischen Produktion und Auftragseingang. Hieran zeigt sich, dass eine (reine) Basisjahrumstellung, die gemeinhin als vergleichsweise unproblematische Generalrevision angesehen wird, die Schätzergebnisse von ökonomischen Strukturbeziehungen verändern kann.

Das Ziel, Transformationsfunktionen möglichst gut zu approximieren, ist auch für die Analyse laufender Revisionen essentiell. Im Rahmen einer theoretischen Analyse wird nämlich nachgewiesen, dass Revisionstatistiken verzerrt sein können, wenn ein unangemessenes Konvertierungsverfahren angewandt wird. Empirische Untersuchungen auf Grundlage der Echtzeitdatensätze von Produktion und Auftragseingang seit Einführung einer neuen Erhebungsmethodik im Jahre 1999 bestätigen den Einfluss der gewählten Transformationsfunktion auf übliche Revisionsstatistiken. Insbesondere im Fall der Industrieproduktion, wo sich affine Transformationsfunktion und einfache Umbasierungsmethode erheblich unterscheiden, sind die Schätzungen für das Revisionsmittel und die Revisionsvarianz stark von der Wahl der Konvertierungsmethode abhängig.

Die empirischen Resultate lassen ferner schließen, dass die vorläufigen Veröffentlichungen von Produktion und Auftragseingang die endgültigen Werte tendenziell unterschätzen. Die laufenden Revisionen der beiden Indexreihen entsprechen insoweit der Erwartung, dass die verbleibende Revisionsvolatilität mit zunehmender Revision abnehmen. Schließlich lassen sich vereinzelt statistisch signifikante Korrelationen zwischen Produktions- und Auftragseingangsrevisionen nachweisen.

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1 Introduction

The statistics of production and orders received are the main source of timely information about the German economic situation. The system of monthly production indices, depicting industrial activity in disaggregated form, was (re)established in the early days of the west German monetary union in 1948. Since then, these statistics have been subjected to a number of benchmark revisions, which cover, for example, base year conversions, changes in sector classifications and index formula as well as innovations in survey methodologies.[1] The last decade alone has seen more than five revisions of this kind. Apart from the obvious need to integrate the East German economy, further changes were caused by the harmonization of statistics within the European Union and by the pressure to relieve the enterprises' statistical workload.

In detail, the first half of the year 1995 saw the implementation of the European NACE and the PRODCOM regulations in the German production and orders statistics.[2] The reform also entailed changing the base year and, for the first time, seasonally adjusted indicators were available for Germany as a whole. After another base year changeover at the beginning of 1998, a more fundamental benchmark revision was made one year later. The survey methodology was refined, creating the existing system of four statistically relevant releases of the monthly production index. The base year 2000 was introduced in the orders statistics in the publication month of March 2003, while the production statistics followed in February 2004.

The paper shows that the consequences of benchmark revisions are diverse and may affect both econometric modeling and the analysis of regular revisions.[3] Quite obviously, economic time series measured at distinct publication dates are not directly comparable when a benchmark revision has occurred in the intervening period. The empirical analysis of regular revision processes may thus be distorted. But even at a single publication date,

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[1]Mankiw and Shapiro (1986) and Mork (1987) provide early examples of subsuming this sort of fundamental changes in statistical conventions under the heading "benchmark revision".

[2]NACE is the acronym for *Nomenclature général des activités économiques dans les Communautés européennes* and PRODCOM is the abbreviation for *Products of the European Community*. The former defines an EU-harmonized classification for economic sectors, while the latter does the same for products.

[3]To clarify notation, regular revisions mean periodic revisions replacing components needed to be estimated in provisional figures by information gathered from statistical sources, which are only available with a time lag. Formally, regular revisions capture all changes in the published quantity less those ascribed to benchmark revisions.

a time series could still be heterogeneous if the historical data originally published according to the standards prior to the benchmark revision cannot be converted ensuring full congruency. Using the example of the German production and orders statistics, this paper argues that a number of statistical tests, predominantly developed to analyze macroeconomic time series (e.g. structural break tests, cointegration tests), can also be beneficial when studying the effects of benchmark revisions. In order to derive hypotheses to be tested, real-time data sets must be supplemented by detailed information about statistical measurement conventions.

The analysis of benchmark revisions is generally twofold.[4] First, whenever documentation gives rise to conjecture that benchmark revisions may affect econometric modeling, structural break tests are natural means to detect heterogeneities *within* vintages. The problem of incongruent time series data might be tackled by accounting for different regimes, in the simplest case by using dummy variables only. Second, in order to ensure consistency *across* vintages, the real-time data base, or at least subsets of it, must be transformed.[5] Simple vintage transformation methods are differencing and rebasing.[6] The paper, however, recommends starting with an affine transformation function which relates the final releases of the two measurement regimes, i.e. before and after the benchmark revision. During the specification step of the transformation function, the parameter constellations, under which the simple methods may work well, can be checked by hypothesis tests.

The consideration of affine transformation functions is a natural generalization when benchmark revisions are supposed to affect not only the level but also the growth rate of a time series. It is worth emphasizing that such an effect does not require major definitional changes in the accounting system. Even a pure base year shift may systematically alter the growth rates of real variables if measured under the fixed-weight methodology because the price structure of the base year defines the weighting scheme of the aggregates (see Croushore and Stark, 2001, p.117-118). In general, it should therefore not be expected that differencing and rebasing are valid conversion methods in the case of real (and not chain-weighted) variables.

[4]At this stage, it is useful to clarify the use of the terms "vintage" and "release" in this paper. A time series measured at a specific publication date is called vintage. The release, however, denotes the issue of the quantity ascribed to a specific reporting period. In the matrix scheme usually used to store the real-time data of an economic variable, reporting periods are in rows and vintages in columns, while releases correspond to diagonals. See Golinelli and Parigi (2005, p.3-4) for an overview of terminology used in this context.

[5]Transformation can be avoided by construing the last vintage, which is prior to a benchmark revision, as final release (see e.g. Keane and Runkle, 1990; Howrey, 1996; Swanson and van Dijk, 2006).

[6]Swanson (1996, p.50) presents rebasing as an alternative to differencing. Rebasing essentially means that all vintages are transformed to the same base year by a deterministic conversion factor extracted from an overlapping period. Rebasing is equal to Golinelli and Parigi's (2005) "rescaling" and looks, at least, rather similar to what Mork (1987) obscurely terms "normalization".

In the vast majority of empirical studies using real-time data, macroeconomic variables are transformed into growth rates because they are mostly nonstationary.[7] However, some papers also put forward the argument that the use of growth rates could avoid, or at least diminish, the problem of benchmark revisions (see e.g. Mankiw et al., 1984; Swanson et al., 1999; Garratt and Vahey, 2006; Swanson and van Dijk, 2006). In contrast, there are only a few studies treating the variables in levels; amongst them are Siklos (1996), Gallo and Marcellino (1999) as well as Patterson (2000, 2002, 2003). Especially the latter author applies the full capabilities of the cointegration methodology (including exogeneity and separation issues) to revision analysis. However, by modeling many releases simultaneously, he risks causing problems typically linked with high-dimensional vector autoregressions (e.g. power erosion in systems cointegration tests). Furthermore, at least in this series of papers, no particular attention is paid to benchmark revisions.

This paper argues that cointegration approaches are especially helpful for the analysis of benchmark revisions, as they can be carried out in rather small systems. In particular, we propose estimating vintage transformation functions as bivariate cointegration regressions. In this regard, the paper is related to Patterson and Heravi (1991) who have already advocated the regression approach in order to achieve consistent real-time data in the context of benchmark revisions. In addition to the concrete econometric setup proposed to estimate the transformation function, we deviate from their contribution by developing theoretical arguments underpinning the convenience of the regression approach. Precisely, we prove that observed revision statistics may not (only) measure properties of the regular revision process if differencing or rebasing, albeit inappropriate, are employed to adjust real-time data for benchmark revisions.

A central element of the paper is to study the consequences on revision analysis when the data generating processes of economic variables possess a common source. In fact, when time series are cointegrated, their dynamic representations include relationships between levels. For revision analysis, this fact has at least two implications. First, it is necessary to characterize the revision processes in levels rather than in growth rates. Second, revision processes may not be analyzed in isolation. With the exception of Patterson (2003), however, the issue is typically not tackled in this comprehensive form.

The revision process of U.S. industrial production is studied by Kennedy (1993), Swanson et al. (1999) and Swanson and van Dijk (2006), for instance. In an application to German data, Jacobs and Sturm (2005) try to circumvent the problem of benchmark revisions by transforming the vintages since 1995 into growth rates. However, our study suggests that this simple measure does not work in this context. In particular, the econometric investigation shows that the effects of benchmark revisions vary not only due to their type but also with respect to the economic variable. The European harmonization of sector and product classifications in 1995 affected production and orders statistics asymmetrically, creating a mean shift in the cointegrating relationship between production and

[7] The extensive literature on the news-or-noise issue originated by Mankiw et al. (1984) and ranging to Faust et al. (2005) and Swanson and van Dijk (2006) have had to use growth rates because test equations would not be balanced otherwise.

the orders received. Even the changeover to the base year 2000 has distinct consequences on the two economic variables. In order to correct for this benchmark revision, it suffices to use the rebasing method in the case of the orders received but not in the case of production. An unpleasant corollary of this statistical result is that the parameters of the long-run economic relationship between the two variables changed simply due to the base year conversion. Regarding the properties of regular production and orders revisions, the results indicate that provisional announcements systematically underestimate final figures. With few exceptions, revisions are found to be serially correlated. Moreover, there is significant positive correlation between the first-versus-final revisions of production and orders.

The more general conclusion of our study may be that real-time data analysis should incorporate an initial step consisting of tests of benchmark revisions. With respect to econometric modeling, an understanding of them may avoid misspecification and, concerning the analysis of regular revisions, it may insure the researcher against the risk that results are adversely affected by an incongruent real-time data set.

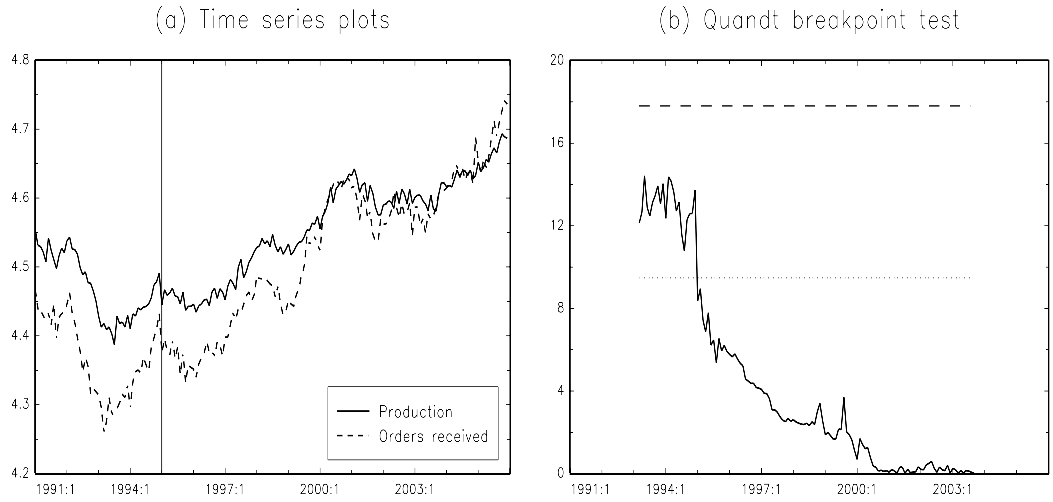
The remainder of this paper is organized as follows. Section 2 analyzes the effect of the sector and product reclassifications in 1995 on the (joint) econometric modeling of industrial production and orders. In Section 3, the properties of the changeover to base year 2000 are investigated and vintage transformation functions are estimated. Section 4 is devoted to the revision procedure established in 1999. Before the empirical results are presented, some formal analysis is carried out, proving the existence of nuisance parameters in observed revision statistics when an inappropriate conversion method is applied for adjusting for benchmark revisions. Section 5 concludes.

2 The sector and product reclassifications in 1995

In 1995, a number of innovations were introduced in the German production and orders statistics (see Nowack and Weisbrod, 1995, for details). The most substantial revision referred to the introduction of new sector and product classifications. The existing German system was replaced by an economic sector classification implementing the pan-European NACE standard. The switch to this new sector classification also involved a changeover to a new product classification derived from the European PRODCOM regulation. In addition, the German industry statistics were affected by two further changes. First, indicators were converted to the base year 1991. Second, the Deutsche Bundesbank started publishing seasonally adjusted indicators for Germany as a whole. The latter is particularly important in our context, as the following econometric analysis uses seasonally adjusted figures of industrial production and orders.

Although industrial indicators were back-calculated to January 1991 according to the new standards, the reorganization of economic sectors and products caused a lack of comparability between figures released before and after January 1995. Hence, this benchmark revision is likely to affect the econometric modeling of industrial production and orders. In order to check this conjecture formally, it is useful to carry out structural break tests. More precisely, the question is whether the data generating processes of industrial production and orders are affected by the benchmark revision in January 1995. This hypothesis

Figure 1: Comovement between industrial production and orders



In Figure (a), the plots depict the time series of industrial production and orders, both transformed into logs. The vertical line shows January 1995, for which the index values were first published according to the new classifications. Figure (b) displays the results of the Quandt breakpoint test. The dashed line represents the 5% critical value of the supremum test, the dotted line the 5% critical value of the χ^2 distribution applicable in cases where the breakpoint is known.

could generally be tested in every data vintage comprising enough data before and after the potential break. For reasons of statistical efficiency, the sample should be as long as possible, but it should include only final data because the issue here refers to the data generating process. Thus, we study the data from January 1991 through December 2005 (as released in July 2006).

In Figure 1(a), the time series of industrial production and orders are plotted. At first glance, both series grow over time with a high degree of commonality. Thus, production and orders might be integrated and cointegrated. In the second place, however, production growth turns out to be, on average, somewhat weaker than the growth of the orders received. Owing to their representation as indices normalized to 100 in the base year 2000, this observation implies that the variables differ with respect to the levels they take in the first half of the sample. Regarding the impact of the reclassifications, there is no obvious evidence for any specialty of the pre-1995 period, but visual inspection alone may sometimes fail. Hansen (1992) provides a set of structural break tests which suits the application at hand. Concretely, the null of cointegration can be tested against the hypothesis that the long-run relationship is different before and since the 1995 revision. Hence, if the null is accepted, the series of production and orders can be modeled by a bivariate error correction model ignoring the benchmark revision. If the null is rejected and, in addition, the Quandt (1960) test statistic does not find strong evidence for an

Table 1: VAR of production and orders

information criterion	lag order	autocorrelation test[a]		SL test[a]
		LM(1)	LM(4)	LR($r = 0$)
AIC	3	0.75 [0.613]	0.57 [0.860]	11.03 [0.030]
HQ	3			
SC	2	2.42 [0.029]	1.01 [0.441]	14.13 [0.008]

[a] p-values are reported in brackets.

alternative break date, we may conclude that the benchmark revision is the most likely cause of the structural break.

Figure 1(b) shows that the sequence of F statistics calculated over the middle 70% range of the sample does not surpass the 5% critical value of the supremum version of the test, suggesting that there is no evidence for a statistical break ignoring information about the date of its (potential) occurrence. However, by testing for a break in January 1995, the F statistic is 13.72, exceeding the 5% critical value of the $\chi^2(4)$ distribution. Hence, the cointegrating relation turns out to be different before and after January 1995, implying that the insufficient back-calculation of data published prior to the sector and product reclassifications matters for the econometric modeling of industrial production and orders.

In order to detect the nature of the structural break, it is not feasible to consider the two regimes separately because the resulting subsamples would be too short for cointegration analyses. A viable modeling strategy of the 1995 benchmark revision would maintain the assumption of cointegration between production and orders, but allow for a shift in the deterministic part of the model. Test procedures for this case are developed by Johansen et al. (2000) and by Saikkonen and Lütkepohl (2000). However, only the latter approach (henceforth referred to as the SL test) provides a framework in which the hypothesis of interest can be tested directly, namely that production and orders are deterministically cointegrated with a shift in the cointegrating mean in January 1995.[8]

Like any systems cointegration test, the SL test requires the lag order of the underlying vector autoregression (VAR) be determined a priori. As standard for this purpose, we apply information criteria such as the Akaike (AIC), the Hannan-Quinn (HQ) and the Schwarz criterion (SC) (see e.g. Lütkepohl, 2005, Chapter 4). Table 1 reports the lag orders which minimize the criteria for unrestricted VARs in levels taking the lag orders $p = 1, ..., 10$ and including a constant, a linear trend and a step dummy variable which is zero from January 1991 through December 1994 and unity otherwise. While the AIC and the HQ suggest

[8]Deterministic cointegration means that there is a linear trend in the variables but not in the cointegrating relation. Whereas the SL test statistic converges to a nonstandard distribution which is free of nuisance parameters under the null hypothesis of interest, this is not so for the traditional Johansen approach. The way out proposed by Johansen et al. (2000) is a two-step procedure which, however, turns out to be less attractive when a direct test is available.

lag order 3, the SC opts for 2. For the more parsimonious choice, however, the systems tests on residuals detect autocorrelation of order 1 at the 10% level.[9] Hence, the following analysis will be based on a VAR(3).

As Table 1 shows further, the SL test rejects the null of no cointegration at the 5% level in the preferred specification. By applying the reduced rank regression technique suggested by Johansen (1991), we are therefore able to estimate the following valid long-run relationship between production and orders (standard errors in parentheses):

$$\mathsf{pr}(t) - \underset{(0.01)}{0.69}\,\mathsf{or}(t) + \underset{(0.004)}{0.013}\,\mathsf{S}(t) \;\sim\; \mathrm{I}(0) \tag{1}$$

where pr and or denote the log levels of industrial production and orders, respectively, and S is the above-mentioned step dummy variable. $\mathrm{I}(d)$ means "integrated of order d".

In sum, the 1995 revision of sector and product classifications caused heterogeneity in the time series of industrial production and orders. This benchmark revision affects econometric modeling because there is evidence of a structural break in the cointegrating relation between these series. The break can be modeled by a step dummy variable, establishing cointegration between production and orders subject to a shift in the mean of the long-run relationship in January 1995.

3 The changeover to base year 2000

Starting with the reporting month of March 2003, the orders statistics were converted to base year 2000. Production indices followed in February 2004. If the base year changeover is implemented in the whole series of interest, the general model structure should not be affected. In real-time data sets, however, this benchmark revision induces a statistical break between vintages before and after the date of conversion. As base years have changed regularly,[10] real-time data research has developed strategies to account for this issue. While differencing and rebasing are straightforwardly applicable, the regression approach demands some preparation. This section is devoted to specifying and estimating vintage transformation functions for industrial production and orders.

The transformation function is modeled as the affine relationship

$$x(t) = a_0 + a_1\bar{x}(t) + \xi(t) \tag{2}$$

where $x(t)$ and $\bar{x}(t)$ are, respectively, the post-conversion and the pre-conversion figures in logs, a_0 and a_1 are scalars and $\xi(t)$ is a residual process with mean zero and variance σ_ξ^2 which, for instance, accounts for measurement errors. It is worth stressing that (2) is a cointegration regression because the series of interest are I(1). $x(t)$ and $\bar{x}(t)$ are both final releases, only differing with respect to the base year. Thus, $\xi(t)$ is supposed to be near

[9] Concretely, the VAR is tested for residual autocorrelation by Doornik's approximate F version of the Breusch-Godfrey LM test (see e.g. Lütkepohl, 2005, Section 4.4.4).

[10] For instance, according to the agreed EU standards, a new base year has to be introduced every five years, i.e. at latest three years after the end of a year ending in a zero or a five.

Table 2: Econometric analysis of the changeover to base year 2000

system	information criterion	lag order[a]	autocorrelation test[b] LM(1)	LM(4)	SL test[b] LR($r = 0$)	unit elasticity[b] LR($a_1' = 1$)
pr, $\overline{\mathsf{pr}}$	AIC	3	0.98 [0.443]	0.74 [0.708]	2.42 [0.730]	
	HQ	1	0.80 [0.569]	0.98 [0.470]	25.02 [0.000]	38.94 [0.000]
	SC	1				
or, $\overline{\mathsf{or}}$	AIC	10	0.44 [0.847]	1.48 [0.144]	2.89 [0.647]	
	HQ	1	1.26 [0.280]	0.70 [0.750]	11.90 [0.020]	11.97 [0.000]
	SC	1				

[a] of the unrestricted VAR in levels.
[b] p-values are reported in brackets.

white noise. Furthermore, the rebasing method implies unit elasticity of the transformation function ($a_1 = 1$), a hypothesis which can be tested empirically. Whenever this restriction is supported, a_0 represents the rebasing factor in log.

Given that the long-run relationship between industrial production and orders is subject to a structural break in January 1995, one should not exclude the possibility that the 1995 benchmark revision has an impact on (2). As an alternative, the transformation function could take the form

$$x(t) = a_0' + a_0^b \mathsf{S}(t) + a_1' \bar{x}(t) + \xi'(t) \tag{2'}$$

where a_0' and a_1' are scalars and $\xi'(t)$ is a residual process. $\mathsf{S}(t)$ is defined as in (1), meaning that a_0^b accounts for the fact that the conversion factor might be different in the pre-1995 and the post-1995 period. As in the previous section, structural break tests can be employed to decide whether (2) or (2′) better represents the data.

Let $\bar{x}(t)$ be the final figures available in the last vintage published with base year 1995. For production, this is the period from January 1991 through December 2002 as released in February 2004. For the orders received, the sample ends one year earlier because the last old-base-year vintage was published in April 2003. As in the previous section, $x(t)$ represents the final figures of the latest vintage which is measured using base year 2000. In the sequel, the series mentioned are denoted by $\overline{\mathsf{pr}}$ and $\overline{\mathsf{or}}$ as well as pr and or.

We start the analysis by asking whether (2) or (2′) is the right specification of the transformation function. The F statistics of the Hansen (1992) test are 16.52 for production and 13.77 for the orders received. As these numbers are clearly higher than the 5% critical value of the $\chi^2(4)$ distribution which is relevant in case of a known break, we conclude that the transformation functions are subject to a structural break induced by the 1995 reclassifications of economic sectors and products. With the SL approach, it can then be tested whether (2′) is a valid cointegration regression. But prior to that, the lag orders of the underlying VARs have to be selected. The AIC chooses comparably long lag lengths, leading to overparametrized models, especially in the case of orders. The HQ and the SC, however, opt for the theoretically appealing lag order 1, implying $\xi'(t)$ to be white noise. The latter choice is confirmed by diagnostic checks which indicate the absence of residual autocorrelation of orders 1 and 4. The SL test statistics reported in Table 2 point to a clear acceptance of this hypothesis for both variables because the absence of cointegration can be rejected for the relevant lag order 1. Power erosion might be the reason why the SL test is not able to reject the null for the long AIC lag lengths.

For both production and orders, (2′) is a valid specification converting vintages of base year 1995 into series which are consistent with those of base year 2000. The transformation function has to be estimated by a cointegration regression. By applying the fully modified (FM) estimator proposed by Phillips and Hansen (1990),[11] we obtain the following results (ignoring error terms):

$$\mathsf{pr}(t) = \underset{(0.038)}{0.386} - \underset{(0.0014)}{0.0084}\,\mathsf{S}(t) + \underset{(0.008)}{0.884}\,\overline{\mathsf{pr}}(t), \tag{3}$$

$$\mathsf{or}(t) = -\underset{(0.023)}{0.332} - \underset{(0.0010)}{0.0048}\,\mathsf{S}(t) + \underset{(0.005)}{1.020}\,\overline{\mathsf{or}}(t). \tag{4}$$

As production and orders are closely connected economically, it is surprising that their transformation functions differ greatly. The estimates of a_0 possess different signs and those of a_1 lie below and above unity respectively. As $a_0 = 0$ and $a_1 = 1$ are reference values, the differences are substantial in qualitative terms. To both variables, however, is common that rebasing is not appropriate from a statistical point of view. In the case of production, this finding is evident, not only owing to the reported FM estimate, but also because the LR test of the unit elasticity restriction is clearly rejected in the systems approach (see Table 2). In the case of the orders received, the LR test does also indicate rejection at conventional significance levels, but the FM estimate is so close to unity that the rebasing method could nonetheless work well in practice.

If $a_1 = 1$, the sign of a_0 is expected to be negative. The reason is that the conversion factor is usually smaller than unity when index series are governed by a trend. The negative intercept documented in (4) complies with the theoretical guess. The positive intercept found in the transformation equation of production must be explained in the context of $a_1 < 1$, which implies that the level of the converted $\overline{\mathsf{pr}}$ is reduced relative to the original. By this effect alone, the extent needed to scale down the old-base-year vintages is obviously exceeded, calling for an adjustment in the opposite direction. The coefficients attached to the step dummy variable have equal signs, but differ clearly with respect to their magnitude.

The distinct vintage transformation functions found for industrial production and orders possess a corollary on the estimated cointegrating relation. The benchmark revision

[11] In contrast to the cointegrating relation (1) which describes a long-run economic relationship between endogenous variables, the transformation function is a technical relationship, clearly determining which variable is regressand and which are regressors. The FM technique is appropriate to this setup because it estimates this structure directly, taking into account that the stochastic regressor is endogenous. The covariance parameters necessary to perform the semiparametric corrections are estimated as proposed in Hansen (1992). It is worth noting that, with reference to Engle and Granger (1987), Patterson and Heravi (1991) estimate conceptually rather similar vintage transformation functions by ordinary least squares.

alters its parameter estimates. As economic interpretation is often concentrated on cointegrating relations, this result is rather unpleasant. By substituting the transformation functions (3) and (4) into the cointegrating relation (1), we end up with $\overline{\mathsf{pr}}(t) - 0.80\,\overline{\mathsf{or}}(t) + 0.009\,\mathsf{S}(t)$, which would be the long-run economic relationship between production and orders before the base year changeover if the post-benchmark revision estimate of the cointegrating relation were true. On the basis of the final figures with base year 1995, it is estimated as

$$\overline{\mathsf{pr}}(t) - \underset{(0.03)}{0.82}\,\overline{\mathsf{or}}(t) + \underset{(0.005)}{0.008}\,\mathsf{S}(t) \;\sim\; \mathrm{I}(0), \tag{5}$$

which, in fact, only differs marginally from the expression derived arithmetically.[12]

In general, a test of the hypothesis that a benchmark revision is innocuous for long-run economic relationships can be constructed in a system comprising the time series of interest in final releases both before and after the benchmark revision. To derive the test idea formally, let K be the number of economic variables and $0 < r < K$ the number of long-run relationships between them. The cointegrating space of the enlarged system with $2K$ time series can be identified by r long-run economic relationships and K transformation functions. If the latter are equal, the benchmark revision will not change the former. The innocuity hypothesis can therefore be evaluated by an LR test, which is asymptotically $\chi^2(K-1)$ distributed (ignoring deterministic elements restricted to the cointegrating space). As the restrictions are linear but span across cointegrating vectors, Boswijk's switching algorithm has to be applied (see Boswijk and Doornik, 2004, Section 4.4).

In the present context, the enlarged system is four-dimensional with the specialty that a step dummy variable has to be considered in both the long-run economic relationship and the vintage transformation functions. Hence, the equality requirement concerns the coefficients attached to $\bar{x}(t)$ and $\mathsf{S}(t)$ in $(2')$, suggesting that the respective LR test is asymptotically $\chi^2(2)$ distributed. The test statistic taking the value 34.93 clearly surpasses the critical values of all conventional significance levels.[13] As expected from the above analysis, we infer from the formal test, too, that the transformation functions of production and orders are not equal in the case of the base year changeover under review. The empirical measurement of the long-run economic relationship between the two variables is therefore significantly affected by a pure statistical phenomenon.

4 The revision procedure introduced in 1999

In 1999, a new survey method was introduced in the production statistics (see Herbel and Weisbrod, 1999, for details). The existing system was refined to relieve the enterprises' sta-

[12] The estimation presented in (5) results from the reduced rank regression of a bivariate error correction model with lag order 3, which is the same specification as used in Section 2. The likely reason for the observed difference is therefore the sample used to estimate the post-benchmark revision cointegrating relation (1).

[13] The LR test is carried out on the basis of a vector error correction model including no lagged differences. This specification is supported by the lag order choices of consistent information criteria. In concrete terms, the HQ and the SC opt for lag length 1 in the corresponding unrestricted VAR in levels.

tistical workload and, in particular, to avoid duplicating reports. Under the new concept, the reporting sample of the production survey is divided into mutually exclusive quarterly and monthly reporting parties. In each of the German *Länder*, the largest units of the economic sectors, covering at least 75% of the sectoral output produced by firms with 20 or more employees, are obliged to submit a monthly production report, suggesting a national coverage in excess of 80%.

The publication dates of the monthly indices are embedded in the International Monetary Fund's reference indicators and are fixed one year in advance. As it is regularly the case that not all monthly reporting parties submit a report on the set date (t + 37 days), the output data for the current latest month is provisional. The **first revision** takes place promptly after further monthly reports have been received in the same publication month (i.e. t + 57 to 62 days). As part of the quarterly production surveys, the monthly reports of the larger enterprises are collated with the quarterly reports of the smaller units, with the extrapolated monthly data being aligned with the quarterly figure using the same percentage rate.[14] This **quarterly revision** takes place roughly two and a half months after the end of the reporting quarter. After the conclusion of the quarterly report for the final quarter of the year, an **annual revision** give the indices final status.[15]

In this section, we will analyze the statistical properties of the revision procedure introduced in 1999. The changeover to base year 2000, affecting the set of data vintages considered for this purpose, requires applying methods discussed in the previous section. A special focus is to study the consequences of using the different conversion methods in circumstances where the transformation function does or does not meet the restrictions of the simple methods. The first part of this section develops some theoretical results on this issue, while the second part comprises the empirical examples based on the real-time data sets at hand.

4.1 Revision statistics in the context of benchmark revisions

In order to derive theoretically which consequences a possibly misapplied conversion method may have on observed revision statistics, let the v th release of the value a nonstationary macroeconomic variable $\bar{x}$ takes in period t be described by

$$\bar{x}(t,v) = m\,t + \sum_{i=1}^{t} \varepsilon(i) + \mu(L)\,\varepsilon(t) + \xi_v(t). \tag{6}$$

The first three elements describe the data-generating process of the economic variable as a Beveridge-Nelson (1981) decomposition where the disturbance term $\varepsilon \sim \text{iid.}(0, \sigma^2_\varepsilon)$ is assumed to possess permanent and transitory impact on $\bar{x}$, $m > 0$ is a drift parameter and

[14]It should also be noted that the monthly figures which have not yet been aligned with the quarterly survey contain an estimated quarterly adjustment. After every quarterly revision, the adjustment factor is recalculated. Since November 1999, the estimated provisional adjustment of monthly industrial production has been stated in a footnote in the Bundesbank publication *Saisonbereinigte Wirtschaftszahlen.*

[15]Consequently, the quarterly and the annual revision coincide for the indices of the final quarters.

$\mu(L)$ is a lag polynomial satisfying the stability condition, i.e. $|1 - \mu(z)| = 0$ for $|z| > 1$. The revision process is modeled by a release-dependent fourth element

$$\xi_v(t) = \delta^v \, [1 + \theta_v(L)] \, \xi(t, v), \tag{7}$$

with $0 < \delta < 1$, $\theta_v(L)$ invertible for all releases v, i.e. $|1 + \theta(z)| = 0$ for $|z| > 1$, and $\xi \sim$ iid.$(0, \sigma_\xi^2)$. In general, ξ_v is I(0) with mean zero, a release-dependent variance $\sigma_v^2 = \delta^{2v}[1 + \theta_v^2(1)]\sigma_\xi^2$ and autocorrelation pattern characterized by $\theta_v(L)$. By the factor δ^v, the revision volatility tends to decrease in the release.[16] Furthermore, the final release ($v = \infty$) is supposed to be measured without revision uncertainty; thus, $\bar{x}(t) \equiv \bar{x}(t, \infty)$ is written for brevity. Finally, while ε and ξ are independent of each other, there might be contemporaneous correlation between the revision processes at some releases v and w, denoted by $\rho_{v,w}$.

Revision statistics are usually calculated for neighboring releases and with a fixed reference release. Swanson et al. (1999) called the former fixed-width revisions and the latter increasing-width revisions when the reference was chosen to be the first release. We adopt the second concept but choose the final release as reference. In accordance with Swanson and van Dijk (2006), this type of revision is named "remaining revision". In our context, it has the advantage that the theoretical revision process (7) is explicit for each v because $\bar{x}(t, v) - \bar{x}(t) = \xi_v(t)$.

The researcher's objective is to study the stochastic properties of the revision process which can be consistently estimated by the observed revision $\bar{x}(t, v) - \bar{x}(t)$ under ergodicity. The specialty of the present investigation, however, is that the v_0 early releases are assumed to be measured according to statistical conventions which are different from those of the final release. In general, the benchmark revision is assumed to affect both the level and the growth rate of the variable, i.e.

$$x(t, v) = a_0 + a_1 \bar{x}(t, v) \quad \text{for} \quad v \leq v_0. \tag{8}$$

In this case, the observed revision is given by

$$\bar{x}(t, v) - x(t) = -a_0 + (1 - a_1) \Big[m\, t + \sum_{i=1}^{t} \varepsilon(i) + \mu(L) \, \varepsilon(t) \Big] + \xi_v(t). \tag{9}$$

In contrast to the scenario without benchmark revision, $\bar{x}(t, v) - x(t)$ does not represent the revision process solely. Apart from the latter, the observed revision depends on parameters characterizing the data-generating process of the economic variable. Thus, the revision statistics do not generally measure the stochastic properties of the revision process as formally stated in the following proposition.[17]

[16] The general decreasing tendency in revision volatilities is reasoned by the result that $\sigma_{v+i} < \sigma_v$ if $\delta^i < [1 + \theta_v(1)]/[1 + \theta_{v+i}(1)]$ which is surely satisfied for some integer i. At neighboring releases, however, there might be parameter constellations satisfying $\delta > [1 + \theta_v(1)]/[1 + \theta_{v+1}(1)]$ such that the revision volatility increases.

[17] The proofs of this and the following propositions are to be found in the appendix.

Proposition 1 If a revision-prone economic variable described by (6) is subject to the benchmark revision (8) which is ignored in the empirical analysis, the observed revision mean and the observed revision variance are given by

$$\begin{aligned}\mathsf{E}[\bar{x}(t,v)-x(t)] &= -a_0+(1-a_1)\,(m\,t),\\ \mathsf{Var}[\bar{x}(t,v)-x(t)] &= (1-a_1)^2[\mu^2(1)+t]\,\sigma_\varepsilon^2+\sigma_v^2.\end{aligned}$$

The evaluation of the observed revisions let the ignorant researcher conclude that the revision process is biased, although it is actually not. The observed revision variance is greater than the true one. In the case of an I(1) variable, the revision bias and the revision variance are increasing over time.

Of course, ignorance is a pure hypothetical case. It is interesting, however, that the results are rather similar for the rebasing method. Since it also imposes $a_1 = 1$, the observed revision statistics only differ with respect to the intercept. Formally, let $\bar{a}_0$ denote the logged conversion factor and, thus, $\mathsf{E}[\bar{x}(t,v)-x(t)] = (\bar{a}_0-a_0)+(1-a_1)\,(m\,t)$ and $\mathsf{Var}[\bar{x}(t,v)-x(t)] = (\bar{a}_0-a_0)^2+(1-a_1)^2[\mu^2(1)+t]\,\sigma_\varepsilon^2+\sigma_v^2$. Only if $a_1 = 1$, rebasing will never make the observed revision mean be more strongly distorted because $\bar{a}_0$ should never be a worse predictor of a_0 than it is zero. However, the observed revision variance will only coincide if the conversion factor is known, i.e. $\bar{a}_0 = a_0$. Otherwise, the variance is higher for rebasing owing to estimation uncertainty with regard to the conversion factor.

Let us now check the effects of a benchmark revision when the regression approach is applied to transform the old-base-year releases $v \leq v_0$. Let $\hat{a}_0$ and $\hat{a}_1$ represent the estimated parameters of the vintage transformation function, and let $\hat{x}(t,v)$ denote the converted $x(t,v)$. In this case, the observed revision is

$$\hat{x}(t,v)-x(t) = (\hat{a}_0-a_0)+(\hat{a}_1-a_1)\Big[m\,t+\sum_{i=1}^{t}\varepsilon(i)+\mu(L)\,\varepsilon(t)\Big]+\xi_v(t). \qquad (10)$$

The expression will reduce to the true value if the coefficients of the transformation function are consistently estimated. The revision mean will be zero, in particular. Owing to estimation uncertainty, however, the revision variance will be greater than the true one. The following proposition states these results formally.

Proposition 2 If the pre-benchmark revision vintages of a revision-prone economic variable described by (6) are converted by a vintage transformation function which is consistently estimated by a regression approach, the observed revision mean is zero and the observed revision variance is $\sigma_v^2+\Lambda_t$ with $\Lambda_t \approx \mathsf{Var}(\hat{a}_0)+\{(m\,t)^2+[\mu^2(1)+t]\sigma_\varepsilon^2\}\,\mathsf{Var}(\hat{a}_1)+2\,(m\,t)\,\mathsf{Cov}(\hat{a}_0,\hat{a}_1)$.

From a theoretical point of view, it remains an open issue as to whether the regression approach or the rebasing method ends up with the lower observed revision variance. From the analytical variance expressions in Propositions 1 and 2, we can draw two conclusions. First, the more precise the transformation function is estimated, the better the regression approach is. Second, the lower the (absolute) t statistic of the null hypothesis $a_1 = 1$ is, the better regression approach is relative to rebasing.[18]

Let us also analyze the effects of differencing on the revision mean and the revision variance. This transformation is worth considering as the major part of the empirical literature on data revisions is based on growth rates. In the present setup, the first difference of $\bar{x}(t,v)$ is given by

$$\begin{aligned}\Delta\bar{x}(t,v) &\equiv \bar{x}(t,v) - \bar{x}(t-1,v+1) \\ &= m + [1 + (1-L)\mu(L)]\,\varepsilon(t) + [\xi_v(t) - \xi_{v+1}(t-1)]. \end{aligned} \tag{11}$$

The revision process is $\Delta\bar{x}(t,v) - \Delta\bar{x}(t) = \xi_v(t) - \xi_{v+1}(t-1)$ when the statistical measurement system remains unchanged. In the case of the benchmark revision (8), however, the revision process is

$$\Delta\bar{x}(t,v) - \Delta x(t) = (1-a_1)\big\{m + [1 + (1-L)\mu(L)]\,\varepsilon(t)\big\} + [\xi_v(t) - \xi_{v+1}(t-1)]. \tag{12}$$

The effects of the benchmark revision on the observed revision mean and the observed revision variance after differencing are summarized in the following proposition.

Proposition 3 If a revision-prone economic variable described by (6) is subject to the benchmark revision (8), the observed revision mean and the observed revision variance of its first difference are given by

$$\begin{aligned}\mathsf{E}[\Delta\bar{x}(t,v) - \Delta x(t)] &= (1-a_1)\,m, \\ \mathsf{Var}[\Delta\bar{x}(t,v) - \Delta x(t)] &= (1-a_1)^2[1 + 2\mu^2(1)]\,\sigma_\varepsilon^2 + \lambda\,\delta^{2v}[1 + \theta_v^2(1)]\sigma_\xi^2\end{aligned}$$

with $\lambda \equiv 1 + \delta^2[1 + \theta_{v+1}^2(1)]/[1 + \theta_v^2(1)] - 2\delta\left\{\theta_v(1)[1 + \theta_{v+1}(1)]/[1 + \theta_v^2(1)]\right\}\rho_{v,v+1}$.

The main advantage of differencing is that the revision mean is unaffected by a_0. Hence, the benchmark revision can be ignored as long as $a_1 = 1$ is fulfilled. The disadvantage is that under this condition (but also in the absence of benchmark revisions), the observed revision variance of the differenced series does not proxy the theoretical variance of the revision component. To be precise, it is multiplied by the factor λ which may be greater or smaller than unity.[19] Whenever $a_1 \neq 1$, differencing faces problems similar to rebasing. First, the observed revision mean differs from zero and, second, the observed revision variance includes volatility components ascribed to ε. However, it may be registered on the positive side that these nuisance terms do not increase over time as they do in the case of rebasing.

In the context of cointegration, the effects of benchmark revisions on statistics of revision correlation are particularly interesting. Let us consider the process $\bar{y}(t,v)$, which is

[18] Since transformation functions are cointegrating relations and, thus, a_1 is estimated superconsistently, it is very likely that the (absolute) t statistics take large values.

[19] Differencing implies a greater variance if $\delta[1 + \theta_{v+1}(1)] > 2\theta_v(1)\rho_{v,v+1}$ which is always satisfied if, for instance, the revision process of the vth release is white noise, i.e. $\theta_v(L) = 0$, or neighboring releases are uncorrelated, i.e. $\rho_{v,v+1} = 0$.

cointegrated with $\bar{x}(t,v)$. Under the simplifying assumption that the long-run economic relationship is $(1,-1)'$, the process can be written as

$$\bar{y}(t,v) = m\,t + \sum_{i=1}^{t} \varepsilon(i) + \kappa(L)\,\varepsilon(t) + \eta_v(t) \tag{13}$$

where m and ε are the same entities as defined in (6), $\kappa(L)$ is a stable lag polynomial and η_v is a release-dependent I(0) revision process analogously modeled as ξ_v in (7). Furthermore, the benchmark revision is described by

$$y(t,v) = b_0 + b_1\bar{y}(t,v) \quad \text{for} \quad v \leq v_0. \tag{14}$$

The following proposition states theoretical results concerning the observed revision covariance between the two variables in the case of different conversion methods.

Proposition 4 If revision-prone economic variables described by (6) and (13) are subject to the benchmark revisions (8) and (14) respectively, the observed revision covariance between them is $\mathsf{Cov}[\xi_v(t), \eta_v(t)]$, provided that the vintage transformation functions are consistently estimated in separate regressions. In the case of rebasing, the observed revision covariance is $\mathsf{Cov}[\xi_v(t), \eta_v(t)] + (\bar{a}_0 - a_0)(\bar{b}_0 - b_0) + (1-a_1)(1-b_1)\,[\mu(1)\kappa(1) + t]\,\sigma_\varepsilon^2$ where $\bar{a}_0$ and $\bar{b}_0$ denote the conversion factors. In the case of differencing, the observed revision covariance is $\mathsf{Cov}[\Delta\xi_v(t), \Delta\eta_v(t)] + (1-a_1)(1-b_1)\,[1+2\mu(1)][1+2\kappa(1)]\,\sigma_\varepsilon^2$.

The observed revision covariance coincides with the theoretical one in the case of the regression approach. For this result, it is crucial that the vintage transformation functions are estimated separately, warranting the independence of regression coefficients across equations. In contrast, the observed revision covariance is affected by nuisance parameters when the standard methods are applied without ensuring for, at least, one variable that the unit elasticity condition is met and, additionally required for rebasing, that the conversion factor is known. The nuisance parameter is traced back to the existence of cointegration because, under inappropriate conversion, the common stochastic component passes through the observed revisions of the two variables.

In conclusion, serious consideration should be given as to whether the unit elasticity restriction holds for the vintage transformation functions before the standard approaches, rebasing and differencing, are applied to adjust real-time data for benchmark revisions. Otherwise, the researcher risks calculating misleading revision statistics. The regression approach is generally more flexible, but it is not recommended for use in any case because its observed revision variance is adversely affected by estimation uncertainty.

4.2 Revision statistics of industrial production and orders

In this section, we verify empirically the prediction of the theoretical analysis on nuisance parameters affecting the revision statistics when real-time data are adjusted for benchmark revisions by misapplied conversion methods. The empirical examination is carried out for industrial production and orders on the basis of the revision procedure established in 1999. From the real-time data sets available for these variables, we extract the first, second, quarterly and final (or annual) releases since the reporting month of January 1999 and compute remaining revision statistics. To abbreviate the revision series under study, let us use 1–F, 2–F and Q–F as acronyms, in which “1” denotes the first, “2” the second, “Q” the quarterly and “F” the final release.[20] Since the latest annual revision finalized the index values of the year 2005, a maximum of 84 observations are available.

The specialty of the real-time data under consideration is the base year changeover analyzed in Section 3, requiring the old-base-year vintages be converted by the methods discussed above. Table 3 summarizes the empirical revision statistics. With respect to levels, the item “original” means the purely hypothetical case where the benchmark revision is ignored. The same column heading, however, sorted in the category “first difference” reports the results of the differencing method.

Let us first consider the case in which the benchmark revision is ignored. Among the poor results reported for both variables, those of orders are comparably worse. This observation might point to the fact that the errors caused by ignorance turn out to depend, to a larger extent, on the intercept term than on the elasticity parameter. By comparing the conversion factors in rebasing, which are 0.86 for production and 0.79 for orders, it is evident that the need to downshift the old-base-year vintages is stronger for orders. Since all conversion methods deal with the shift in some form or another, revision statistics improve considerably relative to no adjustment.

In the case of production levels, the first releases seem to systematically underestimate the final index values. This conclusion is drawn from significantly negative revision means reported for the regression approach.[21] When vintages were rebased instead, the results would suggest the first releases to be unbiased predictors of the final release. While no bias (truly) exists between the quarterly and the final releases, rebasing induces a weakly significant positive revision mean. These results clearly indicate that wrong conclusions may be drawn if the unit elasticity condition is not met. Regarding revision volatilities, we observe a falling trend from the 1–F through the 2–F and the Q–F revisions. This pattern is fully consistent with the model of the revision process presented in (7). It is not puzzling that this property is found for all conversion methods, taking into account the fact that nuisance parameters do not depend on the revision number. In relative terms, the observed revision variances are clearly higher for rebasing than for the regression approach.

For the levels of orders received, all provisional releases are biased predictors of the final release because the regression approach comes out with significantly negative revision means. The minor differences existing in relation to the rebasing method suggest that the statistical rejection of the unit elasticity condition turns out to be largely irrelevant from an empirical point of view. Compared with the regression approach, however, rebasing pro-

[20] In the present setup, the quarterly and final releases are not diagonals in strict form. In both cases, the patterns tailored out of the real-time databases rather resemble step functions.

[21] The theoretical considerations of the previous section have shown that the regression approach does not imply nuisance parameters affecting the revision mean. Hence, the outcomes of this conversion method are generally seen to best proxy true revision biases.

Table 3: Revision statistics

A. Production

		level			first difference	
statistic	revision	original	rebasing	regression	original	regression
mean[a,b]	1–F	1.94 (1.28)	−0.08 (0.27)	−0.35 (0.15)	−0.13 (0.07)	−0.12 (0.06)
	2–F	1.83 (1.21)	0.03 (0.25)	−0.22 (0.14)	−0.04 (0.05)	−0.04 (0.05)
	Q–F	1.91 (1.13)	0.29 (0.19)	0.04 (0.05)	0.01 (0.04)	0.01 (0.03)
std. deviation	1–F	6.25	1.46	0.95	0.85	0.77
	2–F	5.95	1.32	0.78	0.63	0.55
	Q–F	5.58	0.97	0.34	0.43	0.38
variance ratio[c]	1–F	6.58	1.54	1.00	1.10	1.00
	2–F	7.60	1.68	1.00	1.15	1.00
	Q–F	16.37	2.85	1.00	1.13	1.00

B. Orders received

		level			first difference	
statistic	revision	original	rebasing	regression	original	regression
mean[a,b]	1–F	3.06 (1.68)	−0.35 (0.07)	−0.28 (0.07)	−0.14 (0.05)	−0.14 (0.05)
	2–F	3.14 (1.66)	−0.22 (0.06)	−0.15 (0.05)	0.00 (0.03)	−0.01 (0.04)
	Q–F	2.34 (1.44)	−0.19 (0.06)	−0.13 (0.05)	−0.03 (0.03)	−0.03 (0.03)
std. deviation	1–F	8.12	0.75	0.74	0.82	0.83
	2–F	8.06	0.43	0.39	0.50	0.51
	Q–F	7.14	0.39	0.36	0.43	0.44
variance ratio[c]	1–F	11.02	1.02	1.00	0.98	1.00
	2–F	20.81	1.10	1.00	0.98	1.00
	Q–F	19.78	1.08	1.00	0.98	1.00

C. Interrelation

		level			first difference	
statistic	revision	original	rebasing	regression	original	regression
correlation[a,b]	1–F	−0.14 (0.10)	0.24 (0.10)	0.25 (0.09)	0.43 (0.09)	0.44 (0.09)
	1–F	−0.17 (0.11)	0.08 (0.12)	0.01 (0.11)	0.05 (0.10)	0.05 (0.10)
	Q–F	−0.12 (0.09)	0.03 (0.10)	0.11 (0.13)	0.31 (0.17)	0.30 (0.17)

[a] The statistic is measured as a percentage of the final index value.
[b] Heteroskedasticity and autocorrelation consistent (HAC) standard errors compiled due to Newey and West (1987) with lag truncation 4 are reported in parentheses.
[c] The regression approach serves as reference.

vides greater revision variances. The variance ratios are considerably smaller in magnitude than those reported in Panel A. This observation can also be regarded as a consequence of the near fulfilment of the unit elasticity condition.

The results are linked to the news-or-noise discussion of revision processes. Under the news view, revision means must be zero, while they need not be so if revisions are regarded as noise (see Faust et al., 2005, p.406). Since production and orders revisions mostly possess significant biases, the news view can be rejected for the revision procedures of these variables without further testing. Instead, they turn out to be affected by a substantial proportion of noise. The declining trend found in the volatilities of the remaining revisions is a piece of evidence pointing towards the hypothesis that revisions are noisy.

Regarding the empirical association between production and orders revisions in Panel C, we observe a significantly positive correlation between their 1–F revisions . The 2–F and the Q–F revisions, however, affect production and orders statistics in an unrelated manner. In general, the outcomes of rebasing do not differ much from those of the regression approach. This result comes as no surprise because theory suggests that the unit elasticity condition needs to be met for one variable only.

The properties of differencing as a standard conversion method can be checked by comparing the revision statistics observed for this method with those resulting from the real-time data which are first converted by the regression approach and then transformed into first differences. Regarding revision means and correlations, the two approaches only differ marginally. In comparison with the clear superiority featuring the regression approach vis-à-vis the standard method in the level case, this result suggests that the effect of imposing the unit elasticity restriction is markedly less detrimental when the real-time data are transformed into first differences. The theoretical underpinning may be given by the fact that the nuisance parameter is not time-dependent in the case of differencing.

Relative to its regression-based counterparts, differencing leads to higher revision variances in Panel A, while they are marginally lower in Panel B. Hence, for production where the unit elasticity restriction is clearly rejected, the nuisance parameter induced by differencing is comparably more important. For orders with $a_1 \approx 1$, however, it is virtually zero, whereas the estimation uncertainty brought with the regression approach lifts the empirical revision variances. As regards revision volatilities, a final observation is that production and orders revisions differ with respect to the variance shift parameter λ which stems from differencing the real-time data. By considering the two columns headed by "regression", Panel A shows that the standard deviations are smaller when the revision processes of production are studied in first differences than in levels. As depicted in Panel B, the opposite is true for orders.

4.3 Modeling industrial production and orders revisions

The analytical aim of this section is to check the hypothesis that remaining revisions are serially correlated and, if so, whether the autocorrelation pattern can be suitably proxied by an autoregressive (AR) model. For this issue, we first study the time series properties of the remaining revision series of industrial production and orders by their autocorrelation and partial autocorrelation functions (henceforth abbreviated by ACF and PACF respectively). On the basis of these results, AR models are then fitted to the revision series.

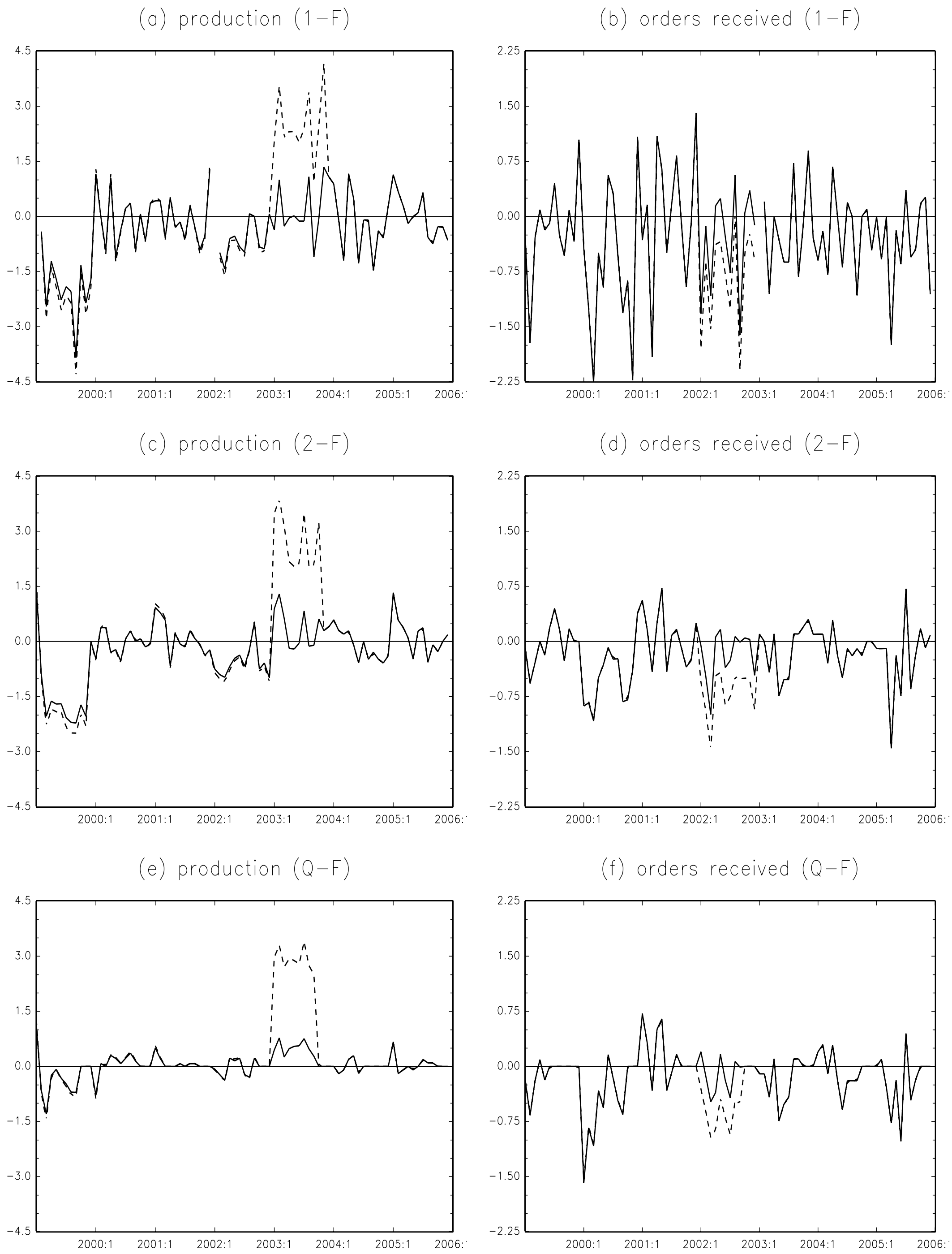
Real-time data of production and orders indices comprise all vintages published since March 1999. Revision series generally start in January 1999. In Figure 2, their plots are displayed in two versions concerning the treatment of the base year changeover. The regression approach serves as reference and provides the series relevant for the subsequent analysis. The reason for showing the rebased revision series, too, is to highlight that misapplied conversion methods may induce strong deficiencies in the interim period where the final release is published with the new base year, while the previous releases are not. Taking into account the previous results regarding the unit elasticity condition, it comes as no surprise that the errors caused by rebasing are more pronounced for production than for orders.

Missing values are observed for January 1999 and January 2002 in Figure 2(a) and for January 2003 in Figure 2(b). In all instances, a delayed announcement was responsible for the non-availability of a first release.[22] Moreover, it is conspicuous that production revisions were comparably large in 1999. In the first year of the new survey method, there was a lack of experience on how to match the reports of the mutually exclusive groups reporting either monthly or quarterly (see also Jung, 2003). At least since 2000, however, the revision series reveal properties of stationary processes. Serial correlation is obvious in the 2–F revisions of both variables, while it also seems present in the 1–F revisions. The Q–F revisions, however, are of minor importance, not at least against the background that they are, by construction of the revision procedure, always zero in fourth quarters.

Table 4 reports the ACFs and the PACFs of the revision series under review. In the case of production revisions, the ACFs decay slowly, while the PACFs only exhibit significant spikes at some lags of low order. This observation is consistent with an autoregressive structure. While the PACF of the 1–F revisions has significant spikes at lag 1 and 3, it is only the first lag which is significant for the 2–F and the Q–F revisions. Instead, the first-order partial autocorrelation of the 2–F revisions is 0.64, suggesting a high persistence. The orders revisions show similar autocorrelation structures, as far as the 2–F and the Q–F revisions are concerned. The degree of persistence, however, is generally lower because the spikes, which are found to be statistically significant in the ACFs and PACFs, take definitely lower values compared with their production counterparts. The major difference between the two variables refers to their 1–F revisions where, in the case of orders, both the ACF and the PACF do not exhibit significant spikes at low lags. Hence, the 1–F revisions of orders turn out to be (near) white noise.

[22] The notion "first release" has to be understood in the context of the publication pattern established in 1999. When the index is not announced at about t+37 days, there is no first release in our understanding, even though there would be, of course, a first announcement of the index at some point later. The missing January 1999 first release of production is explained by adjustment problems during the conversion of the survey method. The base year changeover is responsible for the missing first release of the orders received in January 2003. No reason of this kind, however, can be found to explain why the January 2002 production index was not published in time.

Figure 2: Revision plots



The remaining revisions resulting from the regression approach are depicted by solid lines, the remaining revisions resulting from rebasing by the dashed lines. Note that the scale of production revisions is doubled compared with that of orders. The horizontal axes display the reporting month.

Table 4: Revision autocorrelation patterns

A. Production

statistic	revision	lag 1	2	3	4	5	6
ACF[a]	1–F	0.40 (0.11)	0.29 (0.13)	0.40 (0.13)	0.22 (0.15)	0.15 (0.15)	0.28 (0.15)
	2–F	0.64 (0.11)	0.45 (0.15)	0.34 (0.16)	0.30 (0.17)	0.29 (0.18)	0.19 (0.18)
	Q–F	0.39 (0.11)	0.10 (0.12)	0.18 (0.13)	0.28 (0.13)	0.29 (0.14)	0.15 (0.14)
PACF[b]	1–F	0.40 (0.11)	0.15 (0.11)	0.29 (0.11)	−0.04 (0.11)	−0.01 (0.11)	0.15 (0.11)
	2–F	0.64 (0.11)	0.07 (0.11)	0.05 (0.11)	0.08 (0.11)	0.10 (0.11)	−0.11 (0.11)
	Q–F	0.39 (0.11)	−0.06 (0.11)	0.19 (0.11)	0.18 (0.11)	0.15 (0.11)	−0.01 (0.11)

B. Orders received

statistic	revision	lag 1	2	3	4	5	6
ACF[a]	1–F	−0.15 (0.11)	−0.04 (0.11)	−0.10 (0.11)	0.07 (0.11)	−0.18 (0.11)	0.20 (0.12)
	2–F	0.21 (0.11)	0.10 (0.11)	−0.06 (0.11)	0.06 (0.12)	−0.02 (0.12)	−0.11 (0.12)
	Q–F	0.26 (0.11)	0.21 (0.12)	0.11 (0.12)	0.05 (0.12)	−0.13 (0.12)	−0.02 (0.12)
PACF[b]	1–F	−0.15 (0.11)	−0.07 (0.11)	−0.12 (0.11)	0.04 (0.11)	−0.18 (0.11)	0.15 (0.11)
	2–F	0.21 (0.11)	0.06 (0.11)	−0.09 (0.11)	0.09 (0.11)	−0.04 (0.11)	−0.13 (0.11)
	Q–F	0.26 (0.11)	0.16 (0.11)	0.03 (0.11)	−0.01 (0.11)	−0.17 (0.11)	0.03 (0.11)

[a] Standard errors (imposing Bartlett's moving average assumption) are reported in parentheses.
[b] Standard errors are reported in parentheses.

Table 5: AR revision models

	production			orders received		
	1–F	2–F	Q–F	1–F	2–F	Q–F
const.	−0.10	−0.10	0.02	−0.28	−0.12	−0.10
	(0.10)	(0.07)	(0.03)	(0.09)	(0.05)	(0.04)
AR(1)	0.31	0.64	0.43		0.30	0.35
	(0.10)	(0.08)	(0.10)		(0.11)	(0.11)
AR(3)	0.32					
	(0.10)					
AR(4)			0.23			
			(0.09)			
R^2	0.27	0.44	0.33	0.00	0.09	0.11
AC LM(2)	0.14	1.37	1.63	1.93	0.01	1.01
	[0.93]	[0.50]	[0.44]	[0.38]	[1.00]	[0.60]
AC LM(4)	3.52	2.86	0.60	2.81	0.43	4.64
	[0.47]	[0.58]	[0.96]	[0.59]	[0.98]	[0.33]
AC LM(12)	16.62	5.64	11.66	13.11	5.05	7.43
	[0.16]	[0.93]	[0.47]	[0.36]	[0.96]	[0.83]
ARCH LM(2)	0.65	19.67	2.73	3.53	0.12	0.42
	[0.42]	[0.00]	[0.10]	[0.06]	[0.72]	[0.52]

Standard errors of estimates are reported in parentheses, *p*-values of diagnostic checks in brackets.

Table 5 shows the AR models fitted to the revision series of industrial production and orders. Allowing for lag length 6 at most, redundant lags are sequentially dropped from the equations by using the AIC. The parsimonious AR(1) is found to be the best approximation to one half of the series. This group is therefore characterized by some degree of persistence which is definitely strongest in the case of the 2–F production revisions. In contrast, the estimated AR(1) coefficients of the 1–F and the Q–F orders revisions take comparably smaller values. As expected, the 1–F production revisions resemble an AR(3) process, whereas an AR(4) is found for the Q–F production revisions. In the latter cases, the dominating oscillation is enriched by minor fluctuations of high frequency. In the equation for the 1–F orders revisions, no stochastic regressor survives, confirming the view that these are best approximated by a white noise process. Furthermore, all equations except that of the Q–F production revision possess a negative intercept term, mirroring the observation of Table 3 that provisional releases tend to underestimate the final index values.

Finally, diagnostic checks do not point to misspecification in neither case. In particular, the lag structures seem to be appropriately chosen as the LM tests do not reject the hypothesis that autocorrelation of orders 2, 4 and 12 is absent in all residual series. However, there is a problem of ARCH effects in the AR(1) model for the 2–F production revisions. This deficiency is mainly due to the year 1999, when revisions were, for reasons already mentioned, exceptionally large.

In sum, the results of this section indicate that the remaining revisions of industrial production and orders (mostly) possess autoregressive patterns. This evidence confirms the assumption imposed on the revision process in the theoretical framework of Section 4.1.

5 Conclusion

The paper has laid emphasis on benchmark revisions in real-time data. The example of the German production and orders statistics has shown that both econometric modeling and the analysis of regular revisions might be affected considerably, suggesting the usefulness of first checking the nature of benchmark revisions. While structural break tests are able to detect heterogeneities *within* vintages, systems cointegration tests may be the first step in overcoming inconsistencies *across* vintages. In fact, hypothesis tests may diagnose that differencing and rebasing are inadequate methods for adjusting real-time data for benchmark revisions. According to theoretical arguments as well as the empirical evidence from the application at hand, vintage transformation functions estimated by cointegrating regressions between final data under different measurement systems have been proven to be flexible means of creating congruent real-time data sets.

The consequences of three benchmark revisions on the real-time data of industrial production and orders have been studied. First, structural break tests have shown that the 1995 reclassifications of economic sectors and products cause a mean shift in the cointegrating relation between production and orders at the beginning of 1995. Second, the estimated vintage transformation functions modeling the changeover to base year 2000 are significantly different for production and orders, altering the empirical measurement of the long-run relationship between the two variables. Third, on the basis of real-time data starting in 1999, provisional figures of production and orders have been proven to underestimate the final index values, at least as far as the first release is concerned. Moreover, remaining revisions are generally not free of serial correlation.

Knowledge of systematic components in regular revisions helps to assess the true state of current industrial activity by provisional announcements and may improve forecasts. However, it was beyond the focus of this paper to propose a concrete forecasting model on the basis of the obtained results. In order to reach this goal, the predictability of regular revisions would have to, of course, be analyzed more thoroughly, including their dependence on business survey indicators (see Jacobs and Sturm, 2005) and financial indicators like monetary aggregates or interest rate spreads.

In conceptual terms, it would certainly be interesting to examine the consequences of benchmark revisions in other statistical measurement systems. In particular, it would be worthwhile considering whether base year changeovers possess similarly marked effects in national accounts. As theory suggests that growth rates of nominal or chained-weighted real variables should not be affected by pure base year shifts, existing real-time data include a number of candidates for which the unit elasticity condition should be fulfilled.

A Proofs

Proof of Proposition 1

The revision mean follows immediately from using the expectation operator in (9) with $\mathsf{E}[\varepsilon(t)] = 0$ and $\mathsf{E}[\xi_v(t)] = 0$.

In order to prove the second result, consider

$$[\bar{x}(t,v) - x(t)] - \mathsf{E}[\bar{x}(t,v) - x(t)] = (1-a_1)\Big[\sum_{i=1}^{t} \varepsilon(i) + \mu(L)\,\varepsilon(t)\Big] + \xi_v(t).$$

The revision variance results from squaring the equation and taking expectations

$$\mathsf{E}\Big[[\bar{x}(t,v) - x(t)] - \mathsf{E}[\bar{x}(t,v) - x(t)]\Big]^2 = (1-a_1)^2\Big[\sum_{i=1}^{t} \mathsf{E}[\varepsilon(i)]^2 + \mu^2(1)\mathsf{E}[\varepsilon(t)]^2\Big] + \\ + \mathsf{E}[\xi_v(t)]^2$$

with $\mathsf{E}[\varepsilon(t)]^2 = \sigma_\varepsilon^2$, $\mathsf{E}[\xi_v(t)]^2 = \sigma_v^2$ and $\mathsf{E}[\varepsilon(t)\xi_v(t+i)] = 0 \;\forall\, i$. ■

Proof of Proposition 2

The use of the expectation operator in (10) gives

$$\mathsf{E}[\hat{x}(t,v) - x(t)] = [\mathsf{E}(\hat{a}_0) - a_0] + [\mathsf{E}(\hat{a}_1) - a_1]\, m\, t$$

because $\mathsf{E}[\varepsilon(t)] = 0$ and $\mathsf{E}[\xi_v(t)] = 0$ and by independence of $\hat{a}_1$ and ε. The consistency of the estimates, i.e. $\mathsf{E}(\hat{a}_0) = a_0$ and $\mathsf{E}(\hat{a}_1) = a_1$, in turn implies $\mathsf{E}[\hat{x}(t,v) - x(t)] = 0$.

To derive the expression for the revision variance, square (10) and take expectations. This yields

$$\begin{aligned} \mathsf{E}[\hat{x}(t,v) - x(t)]^2 &= \sigma_v^2 + \mathsf{Var}(\hat{a}_0) + \mathsf{E}\left[(\hat{a}_1 - a_1)\Big[m\,t + \sum_{i=1}^{t} \varepsilon(i) + \mu(L)\,\varepsilon(t)\Big]\right]^2 + \\ &\quad + 2\mathsf{E}\left[(\hat{a}_0 - a_0)(\hat{a}_1 - a_1)\Big[m\,t + \sum_{i=1}^{t} \varepsilon(i) + \mu(L)\,\varepsilon(t)\Big]\right] \end{aligned}$$

because $\hat{a}_0$ and $\hat{a}_1$ are independent of ε. For further calculation, define

$$\Lambda_1 \equiv \mathsf{E}\left[(\hat{a}_1 - a_1)\Big[m\,t + \sum_{i=1}^{t} \varepsilon(i) + \mu(L)\,\varepsilon(t)\Big]\right]^2.$$

Owing to independence of $\hat{a}_1$ and ε, covariance terms can be ignored. Λ_1 simplifies to

$$\begin{aligned} \Lambda_1 &= (m\,t)^2\mathsf{E}(\hat{a}_1 - a_1)^2 + \sum_{i=1}^{t} \mathsf{E}(\hat{a}_1 - a_1)^2\mathsf{E}[\varepsilon(i)]^2 + \mathsf{E}(\hat{a}_1 - a_1)^2\mathsf{E}[\mu(L)\varepsilon(t)]^2 \\ &= \Big[(m\,t)^2 + [\mu^2(1) + t]\sigma_\varepsilon^2\Big]\mathsf{Var}(\hat{a}_1). \end{aligned}$$

In parallel, define

$$\Lambda_2 \equiv \mathsf{E}\left[(\hat{a}_0 - a_0)(\hat{a}_1 - a_1)\Big[m\,t + \sum_{i=1}^{t} \varepsilon(i) + \mu(L)\,\varepsilon(t)\Big]\right] = (m\,t)\,\mathsf{Cov}(\hat{a}_0, \hat{a}_1)$$

because of the above-mentioned independence properties of the random variables.
In sum, the revision variance is $\mathsf{E}[\hat{x}(t,v) - x(t)]^2 = \sigma_v^2 + \Lambda$ with

$$\begin{aligned}\Lambda &\equiv \mathsf{Var}(\hat{a}_0) + \Lambda_1 + 2\Lambda_2 \\ &= \mathsf{Var}(\hat{a}_0) + \Big[(m\,t)^2 + [\mu^2(1) + t]\sigma_\varepsilon^2\Big]\mathsf{Var}(\hat{a}_1) + 2\,(m\,t)\,\mathsf{Cov}(\hat{a}_0, \hat{a}_1).\end{aligned}$$

■

Proof of Proposition 3

The revision mean follows immediately from using the expectation operator in (12) with $\mathsf{E}[\varepsilon(t)] = 0$, $\mathsf{E}[\xi_v(t)] = 0$ and $\mathsf{E}[\xi_{v+1}(t)] = 0$.

Owing to independence of ε and ξ_i, $i = v, v+1$, the revision variance is

$$\mathsf{Var}[\Delta\bar{x}(t,v) - \Delta x(t)] = (1 - a_1)^2\,\mathsf{E}\Big[[1 + (1 - L)\mu(L)]\varepsilon(t)\Big]^2 + \mathsf{E}[\xi_v(t) - \xi_{v+1}(t-1)]^2$$

The first term is $(1 - a_1)^2[1 + 2\mu^2(1)]\sigma_\varepsilon^2$. Due to (7) and cross-correlation between releases, the second term is

$$\begin{aligned}\mathsf{E}[\xi_v(t) - \xi_{v+1}(t-1)]^2 &= \mathsf{E}[\xi_v^2(t)] + \mathsf{E}[\xi_{v+1}^2(t-1)] - 2\mathsf{E}[\xi_v(t)\xi_{v+1}(t-1)] \\ &= \delta^{2v}[1 + \theta_v^2(1)]\sigma_\xi^2 + \delta^{2(v+1)}[1 + \theta_{v+1}^2(1)]\sigma_\xi^2 - \\ &\quad - 2\delta^{2v+1}\theta_v(1)[1 + \theta_{v+1}(1)]\mathsf{Cov}[\xi(t-1,v), \xi(t-1,v+1)].\end{aligned}$$

With $\mathsf{Cov}[\xi(t-1,v), \xi(t-1,v+1)] = \sigma_\xi^2\rho_{v,v+1}$, the equation can be transformed to

$$\mathsf{E}[\xi_v(t) - \xi_{v+1}(t-1)]^2 = \lambda\,\delta^{2v}[1 + \theta_v^2(1)]\sigma_\xi^2$$

where

$$\lambda = 1 + \delta^2\left[\frac{1 + \theta_{v+1}^2(1)}{1 + \theta_v^2(1)}\right] - 2\delta\left[\frac{\theta_v(1)[1 + \theta_{v+1}(1)]}{1 + \theta_v^2(1)}\right]\rho_{v,v+1}.$$

■

Proof of Proposition 4

By multiplying the expression of $[\hat{x}(t,v) - x(t)]$, which is given in (10), with its analog for $[\hat{y}(t,v) - y(t)]$ and taking expectation, the cross products between the three main components can be ignored owing to the independence of parameter estimates across transformation functions due to separate estimation. Thus, the covariance is

$$\begin{aligned}\mathsf{E}[\hat{x}(t,v)-x(t)][\hat{y}(t,v)-y(t)] &= \mathsf{E}[\xi_v(t)\eta_v(t)] + \mathsf{E}[(\hat{a}_0-a_0)(\hat{b}_0-b_0)] + \\ &+ \mathsf{E}\left[(\hat{a}_1-a_1)(\hat{b}_1-b_1)\Big[\sum_{i=1}^{t}\varepsilon^2(i)+\mu(L)\kappa(L)\varepsilon^2(t)\Big]\right].\end{aligned}$$

Since the parameter estimates of the transformation functions are further independent of the stochastic elements of the data generating processes, the third term can be rearranged, simplifying the covariance expression to

$$\begin{aligned}\mathsf{E}[\hat{x}(t,v)-x(t)][\hat{y}(t,v)-y(t)] &= \mathsf{E}[\xi_v(t)\eta_v(t)] + \mathsf{E}[(\hat{a}_0-a_0)(\hat{b}_0-b_0)] + \\ &+ [\mu(1)\kappa(1)+t]\sigma_\varepsilon^2\,\mathsf{E}[(\hat{a}_1-a_1)(\hat{b}_1-b_1)].\end{aligned}$$

The expressions $\mathsf{E}[(\hat{a}_0-a_0)(\hat{b}_0-b_0)]$ and $\mathsf{E}[(\hat{a}_1-a_1)(\hat{b}_1-b_1)]$ are zero because the estimates are supposed to be consistent. As a consequence, the last two terms disappear, suggesting that the observed revision covariance coincides with the covariance of the revision processes. The use of $\mathsf{Cov}[\xi_v(t),\eta_v(t)] = \mathsf{E}[\xi_v(t)\eta_v(t)]$ completes the proof of the statement for the regression approach.

In the case of rebasing, the covariance can be derived quite similarly as

$$\begin{aligned}\mathsf{E}[\bar{x}(t,v)-x(t)][\bar{y}(t,v)-y(t)] &= \mathsf{Cov}[\xi_v(t),\eta_v(t)] + (\bar{a}_0-a_0)(\bar{b}_0-b_0) + \\ &+ (1-a_1)(1-b_1)[\mu(1)\kappa(1)+t]\sigma_\varepsilon^2.\end{aligned}$$

In the case of differencing, the covariance is given by

$$\begin{aligned}\mathsf{E}[\Delta\bar{x}(t,v)-\Delta x(t)][\Delta\bar{y}(t,v)-\Delta y(t)] &= \mathsf{Cov}[\Delta\xi_v(t),\Delta\eta_v(t)] + (1-a_1)(1-b_1)\times \\ &\times \mathsf{E}[1+(1-L)\mu(L)\varepsilon(t)][1+(1-L)\kappa(L)\varepsilon(t)] \\ &= \mathsf{Cov}[\Delta\xi_v(t),\Delta\eta_v(t)] + (1-a_1)(1-b_1)\times \\ &\times [1+2\mu(1)][1+2\kappa(1)]\sigma_\varepsilon^2.\end{aligned}$$

■

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