Cartel Dating

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Published in:
Journal of Applied Econometrics

DOI:
10.2139/ssrn.2860613
10.1002/jae.2660

Link to publication

Citation for published version (APA):
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Summary
The begin and end dates of cartels are often ambiguous, despite competition authorities stating them with precision. The legally established infringement period(s) from documentary evidence need not coincide with the period(s) of actual cartel effects. In this paper, we show that misdating cartel effects leads to a (weak) overestimation of but-for prices and an underestimation of overcharges. Total overcharges based on comparing but-for prices to \textit{actual} prices are a (weak) underestimation of the true amount overcharged, irrespective of the type and size of the misdating. The bias in antitrust damage estimation based on \textit{predicted} cartel prices can have either sign. We extend the before-during-and-after method with an empirical cartel dating procedure, which infers structural breaks of unknown number and dates that mark the actual begin and end dates of the collusive effects. Empirical findings in the European Sodium Chlorate cartel corroborate our theoretical results.

1 | INTRODUCTION

Collusive practices such as price fixing and market sharing allow firms to exert market power they would otherwise not have, artificially restrict output, and increase prices. Cartels are per se illegal and when caught can expect fines and liability for antitrust damages. To determine the quantum of these requires knowing with some precision when and how long the cartel was effective. In their prosecutions of cartel offenses, antitrust agencies specify the begin and end dates of cartel violations with seeming precision, often to the day. These formal cartel dates are typically based on documentary evidence of communication, such as minutes of the initiating cartel meeting or emails in which members disassociate themselves from the agreements. The infringement period is subject also to (settlement) negotiations (American Bar Association, 2014, p. 318; Commission of the European Communities, 2013, recital 43 and footnote 38; both call for the development of econometric techniques to determine effective cartel dates).

The formal cartel dates need not coincide with the point(s) in time at which the anticompetitive effects produced by the cartel violation began or ended. The collusive agreements may take a while to take effect, become ineffective before its members ultimately disband, or rather have lasting effects—including unilateral incentives for former cartellists to keep post-cartel prices up in order to mask their conspiracy (see Harrington, 2004). Also, between periods of coordinated high prices, the cartel spell can include temporary price wars or reversionary episodes due to internal tensions, from which the cartel regathered (see Ellison, 1994; Green & Porter, 1984; Porter, 1983; Rotemberg & Saloner, 1986).

In this paper, we study the consequences for antitrust damage estimation of using cartel dates that are different from the actual begin and end dates of the period(s) in which the collusion had effects. Obviously, different dates imply different periods and volumes purchased over which damages can be claimed. We show that misdating the period(s) in which
a cartel was effective introduces a bias that leads to a (weak) overestimation of but-for prices and an underestimation of overcharges. The effect on total damages depends crucially on whether overcharges are based on predicted or actual cartel prices. When the effective cartel dates are a priori unknown, the common approach of comparing but-for prices to predicted values is shown to be unreliable. The use of observed prices, on the other hand, leads to a (weak) underestimation of cartel damages, irrespective of the type and size of the misdating error.

To overcome bias in damage estimation due to incorrect cartel dates, we propose an empirical procedure for determining the number and dates of the effective cartel periods in price time series that is based on econometric tests for multiple structural change as developed by Bai and Perron (1998, 2003). The method is applied to the European Sodium Chlorate cartel. While the cartel became effective shortly after its formal begin date, we find that it was a single period with overcharges that lasted long after the formal cartel end date. The empirical findings corroborate our theoretical results. The actual total cartel damage in Sodium Chlorate is underestimated by more than 25% when using the formal dates.

Even though most cartel damage studies disclosed rely on formal cartel dates, cartel periods are the subject of dispute. In the few public cases in which the effective cartel dates are argued, methods vary. White (2001) concluded a shorter effective period of the Lysine Cartel than alleged by inspection—as well as a higher but-for price. Bernheim (2008) and Marshall et al. (2008) observed that the Vitamins Cartel potentially affected prices beyond the formal cartel periods and analyzed a number of alternative effective dates. Davis and Garces (2010) mentioned that the start and end dates of cartel effects may be ambiguous, but offered no methods for determining them. Hüschelrath et al. (2013) modeled the transition from cartel to non-cartel regime in the German cement cartel with dummy fractions. Boshoff and van Jaarsveld (forthcoming) applied a Markov regime-switching model to analyze recurrent cartel overcharges in the South African cement market.

While structural break tests are widely used in economics, published applications in determining cartel effects are few. Carlton and Leonard (2004) reported on an application of Bai–Perron tests on a plaintiff's price series in an undisclosed case in which the authors acted for the defense. Having found breaks at all in only a small percentage of runs of Monte Carlo simulations, the authors concluded no statistically significant effect from the alleged conspiracy, and asserted that the plaintiffs had determined their damages period by “cherry picking” the time series for price declines. For the purpose of detecting suspicion of collusion, Harrington (2008) proposed the Quandt (1960)–Andrews (1993) test for a single unknown break date. Crede (forthcoming) explored the Bai–Perron tests also as part of an empirical cartel screen.

This paper is organized as follows. In Section 2 we review common time series regression analysis to estimate but-for prices and overcharges. We analyze in detail under which assumptions unbiased cartel damage estimation occurs. Section 3 sets out how using formal cartel dates that are different from the effective cartel dates affects the damage estimates. In Section 4 we describe the empirical cartel dating procedure. Section 5 illustrates the effects of misdating bias by the European Sodium Chlorate cartel. Section 6 concludes. The proofs and supportive simulation results are given online in the Supporting Information Appendix.

## 2 | ESTIMATING CARTEL DAMAGES USING TIME SERIES

Statistical inferences on antitrust damages in empirical cartel studies often rely on time series analysis, comparing prices over time on the same market, where outside the alleged effective cartel period(s) the industry is assumed to have been in its normal form of competition. Figure 1 is an illustration of the estimation of cartel effects in a hypothetical case in the Commission of the European Communities (2013) Practical Guide (recital 79) on quantifying harm in actions for antitrust damages.

The solid line (“Actual price”) is distinctly elevated during a single effective cartel spell between the marked start and end of the infringement effect. The dotted line (“Estimated price”) is what would have been prices under competition. Typically, these but-for prices are estimated using multivariate regression models that include the relevant factors affecting product prices. The difference between the cartel price and estimated but-for price is the price overcharge by the cartel.

A claimant’s total amount overcharged is the sum over all periods in which the cartel was effective, of each period’s price overcharge multiplied by the total quantity purchased during that period.

For determining but-for prices, two regression approaches are presented. In the forecasting approach, competitive prices during the periods of cartel effects are estimated only from data outside those periods. In the case of a long-lasting cartel and a short post-cartel period, data may be insufficient to forecast. The alternative before-during-and-after method also involves data from during the cartel periods by including a dummy variable in the regression that quantifies the cartel occurrence as a price shift. While this potentially increases the accuracy of the estimates, the underlying assumption that coefficients are constant over the damage and the benchmark period(s) may not hold. For example, cost pass-through can
be structurally lower under collusion than competition. Also, the use of explanatory variables that may have been affected by the cartel, such as capacity, should be avoided. (For further discussion on the relative merits of both approaches, see Godek, 2011; Marshall & Marx, 2012; McCrrey & Rubinfeld, 2014; Nieberding, 2006; White, Marshall, & Kennedy, 2006)

Many recent cartel studies make use of dynamic regression models to quantify cartel overcharges (see Bernheim, 2008; Davis & Garces, 2010; Nieberding, 2006; Roeller & Steen, 2006; White et al., 2006). It is natural and advantageous to include lagged prices as additional explanatory variables. Parametrically modeling short-run dynamics allows for the typical gradual price adjustments over multiple periods and transition of non-cartel to cartel states. Cartel talks often aim at maintaining or increasing prices relative to recent values, leading to serial correlation in prices. Also, the lagged price terms contain valuable information on relevant other, but unobserved price determinants.

In this paper we take the dummy variable approach to a dynamic regression model. Suppose that price over time develops according to the data-generating process (DGP):

$$p_t = \alpha_1 + \alpha_2 D_t + \beta' x_t + \gamma p_{t-1} + \epsilon_t, \quad t = 1, \ldots, T,$$

where \(p_t\) is the product unit price in period \(t\), \(D_t\) the dummy variable quantifying the presence of the cartel effect, \(x_t\) a set of explanatory variables, \(p_{t-1}\) the lagged value of price, and \(\epsilon_t\) an error term. Collusion is aimed at increasing prices; that is, \(\alpha_2 > 0\). The coefficient \(\alpha_2\) measures the immediate or short-run response of the price to the collusion as a price-level shift. Typical explanatory variables \(x_t\) that control for regular changes in prices include cost factors and demand and supply shifters. The regressor \(p_{t-1}\) captures the partial adjustment in prices due to recent changes in the cost structure and market conditions. We assume \(0 < \gamma < 1\); that is, the dynamic relation is stable. Note that DGP (Equation 1) is a general reduced-form equation of the price that encompasses various specifications with (and without) autoregressive distributed lags.\(^1\)

In DGP (Equation 1), the occurrence of the cartel is specified as an intercept shifter, but the model can be generalized to account for slope shifters too. For example, the autoregressive coefficient \(\gamma\) can be different between cartel and non-cartel regimes when the cartel is particularly aimed at increasing prices relative to recent values. Furthermore, a different cost pass-through by a cartel implies changing elements of \(\beta\). We find no empirical evidence in our analysis of the Sodium Chlorate cartel that suggests a need to include slope shifters, and therefore maintain constant slope coefficients as in Equation 1.

We allow for recurrent collusion that alternates with any number of intermittent episodes of low prices, where the cartel may go through price wars or temporary break-ups. These are assumed to be Nash reversions to competition by the use of a single cartel dummy. That is, the sample period \(T = \{1, \ldots, T\}\) consists of periods with and without cartel effects, labeled \(T_C\) and \(T_N\) respectively, with \(T = T_C \cup T_N\). We denote the number of periods in \(T_C\) and \(T_N\) with \(T_C\) and \(T_N\) respectively, with \(T_C + T_N = T\). \(D_t\) is 1 in effective cartel periods (\(t \in T_C\)) and 0 otherwise (\(t \in T_N\)). In the case of recurrent collusion, \(D_t\) defines \(R > 1\) periods of cartel effects; that is, \(T_C = T_{C1} \cup \ldots \cup T_{CR}\).

\(^1\)Including the specifications used in the earlier empirical cartel studies referred to in the text. Without loss of generality, we consider the case of one lagged value of the dependent variable regressor only. The vector of explanatory variables \(x_t\) can include both contemporaneous and lagged regressors.
The standard before-during-after scenario of a single and continuous period of collusive high prices ($R = 1$) as in Figure 1, which we will analyze in Section 3.3 for application to Sodium Chlorate, has an actual begin date $T_B$ and end date $T_E$, with $1 < T_B < T_E < T$, so that $\mathbb{T}_C = \{T_B + 1, \ldots, T_E\}$ and $\mathbb{T}_N = \{1, \ldots, T_B, T_E + 1, \ldots, T\}$ and

$$D_t = \begin{cases} 0, t \leq T_B \\ 1, T_B < t \leq T_E \\ 0, t > T_E. \end{cases} \quad (2)$$

The but-for price in period $t$ is the expected price level in $t$ when the cartel dummy in Equation (1) is switched off ($D_t = 0$), or

$$\text{bfp}_t = \alpha_1 + \beta t + \gamma \text{bfp}_{t-1} + \epsilon_t, \quad t \in \mathbb{T}_C. \quad (3)$$

Outside effective cartel periods, the but-for price and actual price coincide. Within effective cartel periods, the actual price minus the but-for price in each period is that period’s overcharge:

$$O_t = p_t - \text{bfp}_t, \quad t \in \mathbb{T}_C. \quad (4)$$

Typically, the basis for a cartel damages claim is the total amount overcharged, defined (before interest) as

$$\text{CD} = \sum_{t \in \mathbb{T}_C} O_t Q_t, \quad (5)$$

in which $Q_t$ is the actual quantity purchased in period $t$.

We analyze whether population magnitudes such as the but-for price (Equation 3), overcharge (Equation 4) and total cartel damages (Equation 5) are accurately estimated based on a sample of time series observations $(p_t, x_t', D_t)$, combined with per period quantities purchased $Q_t$, $t = 1, \ldots, T$. The possible bias in these estimators can be characterized in terms of their probability limit, which is the value around which their distribution is increasingly concentrated as the sample size increases. In specific situations, the probability limit will coincide with the mean of the estimators. However, in general this expected value cannot be derived analytically. For its approximation by the probability limit to be well defined, we will consider the estimation of averages per period—rather than the total amount, which does not converge as $T$ increases. In the following we assume that, as the sample size increases, the number of observations in both the cartel and the non-cartel period ($\mathbb{T}_C$ and $\mathbb{T}_N$) increases proportionally, which is customary in the econometric literature on structural change (see Perron, 1989). Similarly, we assume that the length of each of the given $R$ effective cartel periods increases proportionally with the sample size.

The probability limit of the average overcharge over the cartel period(s)

$$\bar{O} = \frac{1}{\mathbb{T}_C} \sum_{t \in \mathbb{T}_C} O_t, \quad (6)$$

can be shown to be equal to

$$\text{plim} \bar{O} = \frac{\alpha_2}{1 - \gamma}. \quad (7)$$

That is, DGP (Equation 1) implies an immediate shift of magnitude $\alpha_2$ in the level of the product price as a result of the collusive agreement, controlling for usual developments in price caused by the explanatory variables in $x_t$. Due to the autoregressive short-run dynamics, this initial effect accumulates by gradual price adjustment over time to the full cartel effect of size $\alpha_2/(1 - \gamma) > 0$.

The effects of the various explanatory variables on the price in model 1 are estimated by the standard ordinary least squares (OLS) method. Consistent OLS estimation of the regression coefficients is feasible under the following assumption:

**Assumption 1.** DGP (1) satisfies

$$E[\epsilon_t | F_t] = 0,$$

$$\text{var} (\epsilon_t | F_t) = \sigma^2_N + D_t (\sigma^2_C - \sigma^2_N),$$

where $F_t = \{D_{t-j}, x_{t-j}, p_{t-j-1}, j = 0, 1, \ldots\}$; and

$$\text{cov}(D_t, x_s) = 0, \quad \forall s, t.$$

Furthermore, the processes $p_t$ and $x_t$ are jointly stationary within the cartel and non-cartel regimes.
For consistency, it is only required that the contemporaneous error term $\varepsilon_t$ is uncorrelated with current values of $D_t$ and $x_t$. Lagged feedback of price to its determinants is allowed for.\(^3\) The begin of a cartel period, for example, need not be exogenous but may be induced by a recent price fall. The assumption also implies that $E[\varepsilon_t | \varepsilon_{t-j}] = 0$ and hence $\text{cov}(\varepsilon_t, \varepsilon_{t-j}) = 0$ for $j > 0$. We furthermore rule out collinearity between the cartel dummy $D_t$ and $x_t$, which is a common assumption in the cartel literature. The main complication from having collinearity between elements in $x_t$ and $D_t$ is in the construction of the but-for price for $p_t$, which will then also depend on but-for price paths of those explanatory variables affected by the cartel. Note that by imposing orthogonality between cartel dummy and error term Assumption 1 rules out the additional simultaneity bias that occurs when the formation of a cartel is jointly determined with prices and quantities. Alternatively, one can view model 1 as a reduced-form price equation. Note that the presence of the cartel may also have changed the variability of prices. We therefore allow the conditional error variances $\sigma^2_C$ and $\sigma^2_N$ to be different between cartel and non-cartel regimes.

Once the coefficients of model 1 have been consistently estimated, but-for prices per period are predicted by their own lagged values, plus current empirical values of the additional covariates

$$\hat{\text{bfp}}_t = \hat{a}_1 + \hat{\beta}' x_t + \hat{\gamma} \hat{\text{bfp}}_{t-1}, \quad t \in T_C. \quad (8)$$

In the recursive dynamic simulation, the natural initial but-for price is the product unit price in the last period of competition before the start of the cartel.

Obviously, the estimated and actual but-for prices generally vary over time. We obtain that the but-for price estimator is consistent, however.

**Proposition 1.** Under Assumption 1:

$$\text{plim} \frac{1}{T_C} \sum_{t \in T_C} bfp_t = \text{plim} \frac{1}{T_C} \sum_{t \in T_C} \text{bfp}_t = E[\text{bfp}_t]. \quad (12)$$

Given but-for prices, there are two different practical approaches to the estimation of overcharges in the literature. One is to compare but-for prices as in Equation 8 to *predicted* cartel prices:

$$\hat{p}_t = \hat{a}_1 + \hat{\alpha}' x_t + \hat{\gamma} \hat{\text{bfp}}_{t-1}, \quad t \in T_C. \quad (9)$$

constructed again using recursive dynamic simulation. They are predictions of the price in the case of collusion ($D_t = 1$) and compared with those of the counterfactual competitive regime ($D_t = 0$).\(^3\) The overcharge during collusion is then defined as

$$O_{1,t} = \hat{p}_t - \hat{\text{bfp}}_t, \quad t \in T_C. \quad (10)$$

The other approach is to compare estimated but-for prices as in Equation 8 to *observed* prices $p_t$.\(^4\) This defines the overcharge as

$$O_{2,t} = p_t - \hat{\text{bfp}}_t, \quad t \in T_C. \quad (11)$$

A crucial difference between the two overcharge types for further damage assessment is that $O_1$ is based on the constant cartel price effect $\alpha_2$—as is the actual overcharge—in DGP (Equation 1), whereas $O_2$ implicitly includes the OLS residual and is period specific.\(^5\) Note that Equations 10 and 11 together imply that $O_{2,t} = O_{1,t} + \hat{\varepsilon}_t$.

For correctly specified models, for which Assumption 1 holds and OLS estimators are consistent, there is no difference in expectation for the estimated overcharges. Under consistent estimation of the model parameters we have that

$$\text{plim} \frac{1}{T_C} \sum_{t \in T_C} \hat{\varepsilon}_t = 0. \quad (12)$$

\(^3\)Distribution theory is markedly different in the case of cointegration between price and its determinants, which we here exclude. Unreported findings, both analytical and by Monte Carlo simulation, suggest that similar conclusions apply also for nonstationary versions of DGP (Equation 1).

\(^4\)This approach to calculating overcharges is taken, for example, in Nieberding (2006). For static models it reduces to the approach of Davis and Garces (2010), McCrary and Rubinfeld (2014) and Laitenberger and Smuda (2015).

\(^5\)This approach to calculating overcharges is taken, for example, in Finkelstein and Levenbach (1983), Harrington (2004) and Schinkel, Tuinstra, and Rüggeberg (2008).

\(^7\)It is not clear which overcharge type is advocated in the Commission of the European Communities (2013) *Practical Guide*. While in various places overcharges are defined, as in $\hat{O}_1$, in comparison to the “price actually paid” (recital 79) and “the observed prices” (recital 101), the *Practical Guide* appears also to interpret the before-during-and-after method as generating a constant overcharge, which is more in line with overcharge definition $\hat{O}_1$—see, for example, footnote 77.
Hence average estimated overcharges defined as
\[ \hat{O}_j = \frac{1}{T_c} \sum_{t \in T_C} \hat{O}_{jt}, \quad j = 1, 2 \] (13)
converge to the average actual overcharge as defined in Equation 7, as stated in the following proposition.

**Proposition 2.** Under Assumption 1:
\[ \text{plim} \hat{O}_1 = \text{plim} \hat{O}_2 = \text{plim} \hat{O}. \]

For either of the two different approaches to estimating overcharges, the total amount overcharged (Equation 5) can be estimated by
\[ \hat{CD}_j = \sum_{t \in T_C} \hat{O}_{jt} Q_t, \quad j = 1, 2. \] (14)

In order to analyze the accuracy of these estimators, the relation between quantities and prices needs to be made explicit. Let the per period quantity purchased, \( Q_t, t = 1, \ldots, T \), be price dependent and stochastic, with
\[ E[Q_t] = Q_{C_t}, \quad t \in T_C, \]
\[ E[Q_t] = Q_{N_t}, \quad t \in T_N. \] (15)

We then make the following assumption.

**Assumption 2.** Quantities \( Q_t \) satisfy
(i) \( Q_N \geq Q_C \); and
(ii) \( E[Q_t \epsilon_{t-j}] \leq 0, \quad t = 1, \ldots, T, j \geq 0. \)

Assumption 2(i) is the natural condition that, in periods in which the cartel raised prices, the average quantity purchased will be less than or equal to the volume purchased under the lower competitive prices—that is, that the demand curve is (weakly) downward sloping. Assumption 2(ii) is a high-level assumption. It asks that \( Q_t \) is either negatively correlated or uncorrelated with current and past price shocks \( \epsilon_{t-j}, j \geq 0 \). This is consistent with a (weak) negative correlation between price and quantity. Assumption 2(ii) is satisfied under specific (sufficient) conditions that depend on the structure of demand. It holds (with equality), for example, when the demand curve is linear and (weakly) downward sloping, demand shocks are uncorrelated with prices, and \( x_t \) is strictly exogenous. Assumption 2(ii) can also hold in more complicated oligopoly settings where demand shocks are correlated with prices. For example, it is satisfied in the symmetric Cournot oligopoly model if the variance of cost shocks is larger than the variance of demand shocks.\(^6\) For a general (weakly) downward-sloping demand curve, Assumption 2(ii) requires stronger independence conditions.

We are now ready for the first result on the damages estimators.

**Theorem 1.** Under Assumptions 1 and 2:
\[ \text{plim} \frac{1}{T} \hat{CD}_1 = \text{plim} \frac{1}{T} \hat{CD} \]
and
\[ \text{plim} \frac{1}{T} \hat{CD}_2 \leq \text{plim} \frac{1}{T} \hat{CD}, \]
with strict equality if and only if \( E[Q_t \epsilon_{t-j}] = 0. \)

The use of \( \hat{O}_{1t} \) as overcharge estimator returns a consistent damage estimator, irrespective of the structure of demand, as the overcharge definition (Equation 10) is independent of quantities in all its components. Overcharge estimator \( \hat{O}_{2t} \), on the other hand, does depend on \( Q_t \), and therefore on the period-to-period interaction between price and quantity, through the OLS estimates of the error term. Only when price and quantity are uncorrelated is \( \hat{CD}_2 \) a consistent estimator of \( CD \). For regular demand, their correlation will be negative, implying that \( \hat{CD}_2 \) underestimates true total overcharges. The overcharge method based on actual cartel prices therefore is conservative.

\(^6\)See Supporting Information Appendix A, Assumption 2(ii) under Linear Demand. We note that Assumption 2(ii) excludes intertemporal demand substitution.
3 | MISDATING BIAS

In this section we analyze the consequences of using a classification of cartel begin and end dates that is different from the actual dates that define the periods $T_c$ and $T_N$. When effective cartel and non-cartel periods are misclassified, the difference between overcharge calculation based on predicted and actual cartel prices turns out to matter fundamentally for cartel damage estimates as limit result (Equation 12) no longer holds.

In Section 3.1 we first consider the DGP (Equation 1) without autoregressive dynamics. We find that the bias of the overcharge estimator $\hat{O}_1$ can have either sign, so that over- or underestimation of cartel damages can occur. The estimator $\hat{O}_2$, however, robustly remains a conservative basis for calculating antitrust damages when the effective cartel periods are misclassified. Section 3.2 provides, in the simplest case of a comparison of average prices, the main intuition for these results. In Section 3.3 we extend the results of Section 3.1, for the case of a single cartel overcharge period, to the full model specification (Equation 1) including autoregressive dynamics.

3.1 | Static cartel overcharges

Consider the DGP

$$p_t = \alpha_1 + \alpha_2 D_t + \beta x_t + \epsilon_t, \quad t = 1, \ldots, T,$$

(16)

in which the dummy variable $D_t$ is defined as in Equation 2. Without loss of generality, we consider the case of a single explanatory variable $x_t$ only. The cartel overcharge is a fixed positive margin $\alpha_2$ over the competitive price level. We continue to assume that the regression model (Equation 16) is correctly specified—that is, as in Assumption 1.

Suppose now that the cartel periods used in the estimation are misclassified in the sense that they are different from the effective cartel dates—we will refer to these dates as the formal cartel dates. That is, the estimated model for $p_t$ is

$$p_t = \alpha_1 + \alpha_2 d_t + \beta x_t + u_t, \quad t = 1, \ldots, T.$$

(17)

Depending on the values for $D_t$ and $d_t$ any number of different misdating scenarios is possible: around each period with cartel overcharges and across, the formal begin and end dates may be either both earlier, both later, or respectively earlier and later, or vice versa, than the effective dates. Define

$$\lambda_{ij} = \frac{1}{T} \sum_{t=1}^{T} I(d_t = i, D_t = j), \quad i = 0, 1; j = 0, 1,$$

(18)

with $I(\cdot)$ the indicator function. That is, $\lambda_{ij}$ is the fraction of observations for which scenario $ij$ occurs. We furthermore define $T_{ij}$ as the collection of observations for scenario $ij$, which has $T_{ij}$ observations. For the effective cartel and non-cartel periods we have $T_c = T_{01} \cup T_{11}$ and $T_N = T_{00} \cup T_{10}$. Also we define $T_c = T_{10} \cup T_{11}$ and $T_n = T_{00} \cup T_{01}$ to indicate the formal cartel and non-cartel periods with $T_c = T_{10} + T_{11}$ and $T_n = T_{00} + T_{01}$ observations respectively.

Obviously, if $\lambda_{01} = 0$ and $\lambda_{10} = 0$, the OLS estimators are unbiased and consistent. To analyze the effects of using formal cartel dates that are different from effective cartel dates—that is, $\lambda_{01} > 0$ and/or $\lambda_{10} > 0$—we adapt the approach from Perron (1989) on neglected structural breaks to the more general framework of mismeasured structural change. We obtain the following result on the OLS coefficient estimators of model 17 when the DGP is Equation 16.

**Lemma 1.** Let $\lambda_{01} > 0$ and/or $\lambda_{10} > 0$. Under Assumption 1:

$$\text{plim} \ \hat{a}_1 = a_1, \quad \lambda_{01} = 0,$$

$$\text{plim} \ \hat{a}_1 > a_1, \quad \text{otherwise},$$

$$\text{plim} \ \hat{a}_2 < a_2, \quad \forall \lambda_{01}, \lambda_{10},$$

$$\text{plim} \ \hat{\beta} = \beta, \quad \forall \lambda_{01}, \lambda_{10}.$$

Misdating the cartel dummy regressor $d_t$ in Equation 17 leads to an attenuation bias: The estimator of the dummy coefficient $a_2$ is biased downward, irrespective of the direction of the misdating of the effective cartel periods. In addition, misdating implies that the estimator of the competitive price level $a_1$ is biased upwards, except when the formal cartel periods encompass the effective periods ($\lambda_{01} = 0$), in which case the estimator is consistent. The misdating bias in $a_2$ does not carry over to the estimation of $\beta$, however.
Lemma 1 implies that estimators of but-for prices and overcharges are biased too, as stated in the following proposition.

**Proposition 3.** Under the conditions of Lemma 1:

(i) 
\[
\limsup \frac{1}{T} \sum_{t \in T} \hat{bfp}_t = E[bfp_t], \quad \lambda_{01} = 0,
\]
\[
\liminf \frac{1}{T} \sum_{t \in T} \hat{bfp}_t > E[bfp_t], \quad \text{otherwise}.
\]

(ii) 
\[
\lim \hat{O}_1 < \bar{O},
\]
\[
\lim \hat{O}_2 < \bar{O}.
\]

Using cartel dates in the before-during-and-after method that are different from the effective cartel dates results in estimated but-for prices that are, on average, higher than the actual but-for price, except when the formal cartel periods encompass the effective cartel periods, in which case they are correct, on average. Note that while, on average, but-for prices are higher, in individual time periods but-for prices can be lower using formal dates. In addition, Proposition 3 shows that the average overcharge is strictly underestimated for all types of misdating. The inequalities are strict, including the case \( \lambda_{01} = 0 \), as the average is taken over the formal cartel periods, and so always includes misclassified data.

The effect that misclassifying the effective cartel period(s) has on total damages is twofold, since the total overcharge is determined by the period overcharge(s), as well as by the number of periods designated as collusive, with a quantity purchased each. The overcharge definition used, \( \hat{O}_{1t} \) or \( \hat{O}_{2t} \), turns out to matter materially for the consequences of misdating, as shown in the following result.

**Theorem 2.** Under the conditions of Lemma 1 and Assumption 2:

(i) 
\[
\lim \frac{1}{T} \hat{CD}_1 \geq \lim \frac{1}{T} \hat{CD}, \quad \lambda_{01} = 0,
\]
\[
\lim \frac{1}{T} \hat{CD}_1 < \lim \frac{1}{T} \hat{CD}, \quad \lambda_{10} = 0,
\]
\[
\lim \frac{1}{T} \hat{CD}_1 \leq \lim \frac{1}{T} \hat{CD}, \quad \text{otherwise}.
\]

(ii) 
\[
\lim \frac{1}{T} \hat{CD}_2 \leq \lim \frac{1}{T} \hat{CD}, \quad \lambda_{01} = 0,
\]
\[
\lim \frac{1}{T} \hat{CD}_2 < \lim \frac{1}{T} \hat{CD}, \quad \text{otherwise}.
\]

The estimator \( \hat{CD}_1 \) can under- as well as overestimate the actual damage, because the overcharge \( \hat{O}_{1t} \) is applied to a sum total of quantities labeled as purchased under the cartel regime that may be higher or lower than the actual total quantity. The estimator \( \hat{CD}_2 \), on the other hand, for all misdating scenarios provides a conservative estimate, in the sense that the actual total damage during the effective cartel period(s) is at least as large as the damage estimated using misclassified dates in the but-for price estimation. The reason for this difference between \( \hat{CD}_1 \) and \( \hat{CD}_2 \) is that \( \hat{O}_{2t} \) is the sum of \( \hat{O}_{1t} \) and the OLS residual, which now picks up the measurement error. In addition to (weakly) underestimating the actual overcharge, because the overcharge \( \hat{O}_{2t} \) is period specific, in misdated estimations that lead to higher than actual but-for prices (i.e., in all cases but \( \lambda_{01} = 0 \)) this overcharge becomes negative in competitive periods falsely labeled as collusive (i.e., for \( t \in T_N \), where \( \alpha_2 = 0 \)), thus decreasing the total damage estimate. As in Theorem 1, the estimator \( \hat{CD}_2 \) is consistent for CD only when \( E[Q_t \epsilon_{1t}] = 0 \).

When the dates are correct, by Theorem 1 \( \hat{CD}_1 \) is a consistent estimator. Hence all bias in \( \hat{CD}_1 \) established in Theorem 2 is due to misdating. For \( \hat{CD}_2 \), Theorem 1 showed a downward bias already when price and quantity are negatively correlated. The misdating bias in \( \hat{CD}_2 \) is therefore the sum of this bias under correct dating and additional bias due to misdating. Only when \( \lambda_{01} = 0 \) is the latter effect zero. In general, the relative size of the two sources of bias depends on the own-price elasticity of demand and the extent of the misdating.
### 3.2 Illustration by comparison of mean prices

To gain intuition for the source of the misdating biases, consider the special case without covariates:

\[ p_t = a_1 + a_2 d_t + u_t. \]  

Let \( \bar{p}_c \) be the mean price over the formal cartel periods and \( \bar{p}_n \) be the mean price over the formal non-cartel periods. The OLS estimators of model 19 can be expressed as

\[ \hat{a}_1 = \bar{p}_n, \quad \hat{a}_2 = \bar{p}_c - \bar{p}_n. \]  

Misspecifying the effective cartel dates amounts to falsely labeling part of the competitive prices as collusive and/or part of the collusive prices as competitive. Hence including higher than competitive prices in \( \bar{p}_n \) results in an upward bias in estimating \( a_1 \)—the competitive price level—because \( \bar{p}_n \) overestimates the true mean price in the non-cartel periods. Only when \( \lambda_{01} = 0 \) is \( \hat{a}_1 \) based on non-cartel data only, so that it is unbiased.

For the same reason, mistakenly including competitive prices in the cartel sample decreases the average \( \bar{p}_c \). This is the case in all misdating scenarios but when the effective cartel periods coincide with the formal cartel periods. The estimated cartel effect \( \hat{a}_2 \), which is the difference in average price between cartel and non-cartel periods, has a downward bias in all misdating cases, as either \( \bar{p}_c \) or \( \bar{p}_n \) is affected, or both. When \( \bar{p}_c \) is correct (\( \lambda_{10} = 0 \)), \( \bar{p}_n \) is higher than the true mean non-cartel price, and when \( \bar{p}_n \) is correct \( \bar{p}_c \) is lower than the true mean cartel price (\( \lambda_{01} = 0 \)).

Figure 2 illustrates for a single cartel period how the use of estimator \( \hat{a}_1 \) as a basis for damage calculation is conservative. The actual cartel price increase by \( \alpha_2 \) lasts from \( T_B \) to \( T_E \). The vertical dashed lines at \( T_b \) and \( T_e \) are the formal cartel dates used in the estimation. We consider the misdating case in which both the begin and end of the cartel are formally dated too early (\( \lambda_{01} > 0 \) and \( \lambda_{10} > 0 \), but the effective and formal cartel periods still overlap.

The average actual but-for price is \( \alpha_1 \), yet by using the misclassified formal cartel dates the estimator \( \hat{a}_1 \) uses price data from prior to \( T_b \) and past \( T_e \). The estimate therefore includes observations between \( T_e \) and \( T_E \) that are actually cartel prices, but falsely labeled competitive prices. Inclusion of these higher prices increases the estimated but-for price \( \hat{bfp}_t \) above the level of the actual but-for price.

For three reasons, therefore, the estimated damage total is smaller than the actual damage. First, the overcharges are smaller by area \( C \), because the but-for price is overestimated. Second, the formal cartel end date \( T_e \) lies before the effective cartel end date \( T_E \), so that part \( D \) is missed. Third, during the period \( T_b < T_B \), the overestimation of the but-for price results in negative overcharges the size of area \( B \), which is subtracted from the damage estimate area \( A \).

The damage total \( \hat{CD}_2 \) underestimates \( CD \) even when the formal cartel period is longer than the effective. This can be illustrated in Figure 2 by moving \( T_b \) further to the left of \( T_B \), other things equal. It enlarges the area \( B \) subtracted. The effect of this larger misdating error on \( \hat{CD}_1 \), however, is ambiguous. While \( \hat{CD}_1 \) is lower, the earlier formal cartel begin date brings more of the larger competitive volume under the damage period. These effects trade off, depending among other things on the own-price elasticity of demand.

**FIGURE 2** Theorem 2 illustrated. The figure illustrates how the \( \hat{CD}_2 \) estimator underestimates the true cartel damages, in a hypothetical situation where the formal cartel begin and end dates \( T_b \) and \( T_e \) precede the actual cartel begin and end dates \( T_B \) and \( T_E \). The actual cartel damages are represented by the area \( \alpha_2(T_E - T_B) \) of the rectangle consisting of parts \( A, D, \) and \( C \). The estimated cartel damages are represented by the area of \( A \) minus the area of \( B \).
When $\lambda_{10} = 0$, in which the formal cartel period falls entirely within the effective period, there are no negative overcharges, yet the but-for price is grossly overestimated and two periods of actual damages left and right from the effective cartel dates are not counted. Only when the formal period includes the effective period—that is, $\lambda_{01} = 0$—will the $\tilde{O}_{2t}$ overcharges calculated outside the effective cartel period be zero on average, thereby not contributing to the damage estimate, so that $\tilde{C}D_2$ is an unbiased estimator of actual damages. $\tilde{C}D_1$, however, may still overestimate the actual damage in this case, as $\bar{\hat{p}}_c$ (and so $\hat{O}_{1t}$) decreases in the length of the formal cartel period, while at the same time more purchases are alleged to be affected.

### 3.3 Full price dynamics with one overcharge

Now consider the full model specification (Equation 1), including the lagged value of price. First note that there is no reason to think that estimator $\tilde{C}D_1$ will improve in the extended model, and so we need not examine it further. To see if $\tilde{C}D_2$ remains a robust conservative estimator, consider the estimated model

$$p_t = \alpha_1 + \alpha_2 d_t + \beta x_t + \gamma p_{t-1} + u_t, \quad t = 1, \ldots, T. \tag{21}$$

Autoregressive dynamics in the but-for price series implies that allowing for multiple cartel periods ($R > 1$) would lead to a substantial increase in the types of scenarios to be considered, as misdating one cartel period potentially has ambiguous lasting effects on later episodes. To avoid this complexity, and in keeping with our case study in Section 5, we limit the analysis to a single period of cartel effects as defined in Equation 2.7 Using misclassified cartel dates in the estimation (Equation 21) with $R = 1$ means that the cartel dummy variable $d_t$ takes on the following values:

$$d_t = \begin{cases} 
0, & t \leq T_b \\
1, & T_b < t \leq T_e \\
0, & t > T_e, 
\end{cases} \tag{22}$$

where $T_b$ is the formal cartel begin date and $T_e$ the formal cartel end date.

Obviously, if $\lambda_{01} = \lambda_{10} = 0$ ($T_b = T_B$ and $T_e = T_E$) the OLS estimators are consistent. There are four possible misdating scenarios for the break dates in total. We obtain the following results on the OLS coefficient estimators of model 21 when the DGP is Equation 1.

**Lemma 2.** Let $\lambda_{01} > 0$ and/or $\lambda_{10} > 0$ and $R = 1$. Under Assumption 1:

- $\text{plim} \, \hat{\alpha}_1 < \alpha_1$,
- $\text{plim} \, \hat{\alpha}_2 < \alpha_2$,
- $\text{plim} \, \hat{\beta} \geq \beta, \quad \text{if } m_{xp-1} < 0$,
- $\text{plim} \, \hat{\gamma} > \gamma$,

where $m_{xp-1} = \text{plim} \frac{1}{T} \sum_{t=1}^{T} x_t p_{t-1}$.

Misdating bias now occurs in all coefficients. The cartel coefficient $\alpha_2$ continues to be underestimated in all misdating scenarios. The intercept $\alpha_1$ now is always underestimated as well. The autoregressive coefficient $\gamma$ is overestimated always, whereas the direction of the bias in the coefficient $\beta$ of the explanatory variable depends on the sign of the particular data moment $m_{xp-1}$.

With bias in the OLS estimators of all regression coefficients, it is not obvious what the impact is of misspecified cartel begin and end dates on the estimated but-for prices, cartel overcharges and total damage. We obtain the following for the average but-for price and average overcharge.

**Proposition 4.** Under the conditions of Lemma 2:

(i) $\text{plim} \frac{1}{T_c} \sum_{t \in T_c} \hat{\text{bf}} p_t = \text{plim} \frac{1}{T_c} \sum_{t \in T_c} \text{bf} p_t, \quad \lambda_{01} = 0,$

$\text{plim} \frac{1}{T_c} \sum_{t \in T_c} \hat{\text{bf}} p_t > \text{plim} \frac{1}{T_c} \sum_{t \in T_c} \text{bf} p_t, \quad \text{otherwise.}$

---

7Monte Carlo experiments for dynamic models with $R > 1$ suggest that the conclusions of Lemma 2, Proposition 4, and Theorem 3 continue to hold.
plim\(\hat{O}_2 < \text{plim}\hat{O}\).}

Hence but-for prices continue to be overestimated in all misdating scenarios but with inclusive formal dates, and the average overcharge is underestimated in all cases. Moreover, for the preferred overcharge estimator \(\hat{O}_{2t}\) we find the following analogue to Theorem 2.

**Theorem 3.** Under the conditions of Lemma 2 and Assumption 2:

\[
\text{plim} \frac{1}{T} \hat{CD}_2 \leq \text{plim} \frac{1}{T} CD, \quad \lambda_0 = 0,
\]

\[
\text{plim} \frac{1}{T} \hat{CD}_2 < \text{plim} \frac{1}{T} CD, \quad \text{otherwise}.
\]

We conclude that when the effective cartel dates are unknown the preferred approach to cartel damages estimation is to compare estimated but-for prices to observed prices, not predicted cartel prices. The estimator \(\hat{CD}_2\) applied to the misclassified cartel dates provides a lower bound for the actual cartel damages, irrespective of the type and size of the misdating. \(\hat{CD}_2\), furthermore, is consistent as long as the formal cartel dates used in the estimation encompass the effective cartel dates. In conclusion, when the effective cartel dates are unknown, the preferred approach to cartel damages estimation is to use conservative overcharge estimator \(\hat{O}_{2t}\).

## 4 INference with Structural Breaks of Unknown Number and Dates

The bias from misspecifying effective cartel begin and end dates can be mitigated by determining the effective dates with more precision. One way to do this is to supplement the before-during-and-after method with tests for multiple structural change. Conditional on the estimated number of breaks and estimated break dates, an alternative cartel dummy regressor is constructed, which is then used in the empirical analysis to estimate cartel effects. In this section we discuss the accuracy of this empirical strategy in more detail.

We use Bai and Perron (1998, 2003) test procedures for detecting a priori unknown multiple structural breaks. Bai and Perron suggest modeling any serial correlation parametrically by including lagged regressors.\(^8\) While Bai–Perron tests can in principle also be used to determine possible changes in (some or all) coefficients of the explanatory variables, in line with the cartel literature we treat the difference between cartel and non-cartel regimes as price-level shifts.

Consider a general version of DGP (Equation 1) with up to \(m + 1\) different intercepts:

\[
p_t = \delta_j + \beta^t x_t + \gamma p_{t-1} + \epsilon_t, \quad t = T_{j-1} + 1, \ldots, T_j,
\]

for \(j = 1, \ldots, m + 1\) with \(T_0 = 0\) and \(T_{m+1} = T\). As the intercepts equal \(\delta_1, \ldots, \delta_{m+1}\), the model allows for \(m + 1\) regimes with unknown break dates \(T_1, \ldots, T_m\).\(^9\)

Given a set of candidate break dates \(T_1, \ldots, T_m\), the least squares estimates minimize the residual sum of squares from (Equation 23):

\[
S_T(T_1, \ldots, T_m) = \sum_{j=1}^{m+1} \sum_{t=T_{j-1}+1}^{T_j} (p_t - \delta_j - \beta^t x_t - \gamma p_{t-1})^2.
\]

The effective cartel dates \((\hat{T}_1, \ldots, \hat{T}_m)\) are found as follows:

\[
(\hat{T}_1, \ldots, \hat{T}_m) = \text{argmin}_{T_1, \ldots, T_m} S_T(T_1, \ldots, T_m),
\]

where the minimization is over all partitions for which \(T_j - T_{j-1} \geq h\), the trimming parameter \(h\) being the minimal number of observations between two breaks.

The null hypothesis of no break is tested against the alternative hypothesis of \(m\) breaks, using the standard \(F\)-test statistic for goodness of fit of the model with and without the estimated break dates \((\hat{T}_1, \ldots, \hat{T}_m)\), developed in Chow (1960) for known break dates. This is referred to as the sup \(F\)-test: the maximum over all possible Chow \(F\)-tests. It has a nonstandard asymptotic distribution, for which asymptotic critical values are provided in Bai and Perron (1998). Perron (2006)

\(^8\)As an alternative approach to treating potential serial correlation in the time series, Bai and Perron (1998, 2003) suggest estimating a static model and treat serial correlation nonparametrically by exploiting HAC covariance matrix estimation.

\(^9\)Note that in model 1 \(m = 2\), implying \(\delta_1 = a_1, \delta_2 = a_1 + a_2\) and \(\delta_3 = a_1\).
conjectures that most tests for structural change will have nonmonotonic power when the true number of breaks is larger than the number of breaks imposed in the construction of the test statistic. It is advisable, therefore, to report outcomes of the sup $F$-test for a number of different choices of $m$. As the number of breaks in the data is not a priori known, use is made of the unweighted and weighted double maximum tests, labeled UD max and WD max, which are the maxima of a series of sup $F$-statistics, to test the null hypothesis of no break versus an unknown number of breaks—up to some prespecified maximum.

The break dates so found indicate structural changes in the specification, such as would be caused by a regime switch from competition to collusion, and vice versa. The procedure thus allows for establishing empirically whether there were effects of the cartel violation and whether they were continuous, or rather consisted of several collusive episodes with lower price intermittents. It is important to note that in the dynamic specification the breaks in the time series should be interpreted as the dates where the cartel began to become effective, or started to lose its effectiveness. The transitions from competitive to collusive prices and back typically are gradual. For various reasons discussed, price levels may continue to be affected by a cartel long dissolved. The end date of such lasting effects of a cartel would be when the but-for prices and the actual prices have fully converged.

A concern about the test procedures is that they rely on asymptotic distribution theory. Monte Carlo simulation studies on univariate time series models have shown that finite-sample distributions of the various test statistics may deviate substantially from their asymptotic approximations (see Bai & Perron, 2006; Diebold & Chen, 1996; O’Reilly & Whelan, 2005). In particular, size distortions are found to grow (and power to decrease) when time series become more persistent. In principle, the bootstrap can be used to get a better approximation of the finite-sample distribution of the structural break tests. Diebold and Chen (1996) show that for the univariate first-order autoregressive model, using a standard nonparametric bootstrap leads to tests with the correct size.

To the best of our knowledge, no such simulation results exist for asymptotic and bootstrap Bai–Perron tests for multiple regression models with autoregressive short-run dynamics and additional explanatory variables. We have therefore analyzed in a small-scale Monte Carlo study for our DGP (Equation 1) whether the implementation of structural break tests for detecting and dating cartel effects has enough size control. The results are reported in Supporting Information Appendix B.

The actual size of the statistical tests for structural change is found to be close to nominal significance levels when the data are moderately persistent. Size distortions occur, however, when there is high persistence, which could lead to spurious breaks. Yet the bootstrap can effectively deal with this problem and leads to test procedures with the correct size. The simulations also show that the bootstrap tests have nontrivial power, at least for the cases examined. Therefore, in practice, the bootstrap version of the break tests should be favored.

We have also analyzed by simulation the accuracy of OLS inference based on the estimated break dates. We focus on the bias of the OLS coefficient estimator of the cartel effect $\alpha_2$, and the size distortion of the corresponding $t$-ratio. Finite-sample bias and size distortions are small in case of moderately persistent data. Even in case of high persistence only moderate coefficient bias and size distortions occur; hence the accuracy of OLS inference based on estimated break dates is satisfactory.

5 | DATING THE SODIUM CHLORATE CARTEL

In June 2008, the European Commission adopted a cartel infringement decision in *Sodium Chlorate* against four groups of chemical producers, imposing fines of 79 million euro in total. Sodium chlorate is a chemical compound synthesized from the electrolysis of salt and water. It is applied in the bleaching of pulp to make it suitable for the manufacture of printing and tissue-grade papers that meet the elemental chlorine-free (ECF) paper standard. The cost of sodium chlorate as part of the final chemical pulp prices is about 1%. Supply agreements typically took the form of a medium-term framework contract (usually 3–5 years), including estimated purchase volumes and a clause for price adjustments based on specified cost indicators. Price revisions were made throughout the year and common for the new calendar year.

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10In a landmark decision in a paper case in 2010, the German Federal Court of Justice allowed taking “lingering effects” of the cartel into account as a factor augmenting the damage (see German Federal Court of Justice, June 28, 2011, ORWI, recital 84; see also Hüschelrath et al., 2013).

11Additionally, Bai and Perron (2006) report Monte Carlo simulation results showing that the use of nonparametric covariance matrix estimators leads to substantially larger size distortions than modeling serial correlation parametrically by using dynamic regression models.

12European Commission decision of June 11, 2008 in Case COMP/38.695—*Sodium Chlorate*. The decision was upheld by the European General Court.
When demand began to stagnate in anticipation of the stricter totally chlorine-free (TCF) paper standard, and (expected) overcapacity put downward pressure on prices in the first half of the 1990s, the main sodium chlorate producers in Europe formed a cartel to implement a strategy of stabilizing the market. The first cartel meeting of which the Commission obtained evidence was on September 21–22, 1994, in Helsinki, in which Akzo/EKA and Kemira agreed on upward price adjustments for 1995. The cartel expanded in 1995 and 1996, when smaller suppliers joined. Jointly, the cartel members served over 90% of the total sodium chlorate market in Europe.

In 1998 some internal tensions are reported to have risen between the cartel members, after one sodium chlorate producer was suspected to have secretly supplied another’s customer. Repeated attempts were made to maintain cartel stability, leading to coordinated price increases in 1997, 1998, and 1999. In March 2003, Akzo/EKA brought the existence of the cartel to the attention of the European Commission with a successful leniency application. The Commission determined in its decision that the infringement had formally ended on February 9, 2000, when Akzo/EKA had denounced the cartel at a trade association meeting.

The monthly price per ton of sodium chlorate is analyzed in the sample period January 1993 to December 2005 on the basis of \( T = 156 \) volume-weighted average delivered price observations \( (p_t) \). The price data were obtained from several large direct purchasers of the cartel, which together exercised almost half of total European demand for sodium chlorate in the relevant period. Monthly variation in the price series reflects (contractual) price revisions made throughout the year. The price data do not exhibit a seasonal pattern.

We estimate the dynamic regression model:

\[
p_t = \alpha_1 + \alpha_2 d_t + \beta_1 x_{1t} + \beta_2 x_{2t} + \beta_3 x_{3t} + \beta_4 x_{4t} + \gamma_1 p_{t-1} + \gamma_2 p_{t-2} + u_t, \tag{26}
\]

in which \( d \) is the cartel dummy, \( x_1 \) is the electricity price, \( x_2 \) labor costs, \( x_3 \) Western European chemical pulp production, and \( x_4 \) European production capacity for sodium chlorate.13 Electricity and labor are the major input costs in the production of sodium chlorate and the main common contract indexation factors, while pulp production and sodium chlorate production capacity serve as demand and supply shifters, respectively. The short-run dynamics of product prices are modeled by two lagged price regressors \( p_{t-1} \) and \( p_{t-2} \). Except for the cartel dummy, all variables are measured in logarithms, so that the regression coefficients are (semi-)elasticities.

The cartel effect is best modeled with just an intercept shifter. Allowing, conditional on the found dates, for breaks in the parameters of the control variables or lagged price regressors returns no significant differences except for the coefficient of labor costs, for which results for average but-for price and overcharge change only marginally, implying that its economic significance is negligible (see Godek, 2011). We therefore maintain the assumption of constant slope coefficients in the construction of but-for prices and overcharges.

Table 1 reports the results of various structural break tests on the intercept. To avoid possible low power due to an underspecified number of breaks, we report sup \( F \)-test outcomes for \( m \in \{1, 2, 3, 4, 5\} \). We use two different trimming parameters \( \mu = h/T \in \{0.10, 0.15\} \). Furthermore, the UD max statistic is used to determine the number of breaks. For both asymptotic and bootstrap critical values, all sup \( F \)-tests as well as the UD max test indicate two— and only two—break points, estimated at January 1995 and February 2002, marking structural changes in the effectiveness of the cartel.

Figure 3 displays the actual sodium chlorate prices as the solid line \( p \). The effective cartel dates estimated are indicated with solid vertical lines, while the dashed vertical lines are the formal cartel begin and end dates: September 1994 and February 2000. But-for prices using the effective (formal) cartel dates are \( \text{bfp (bfpo)} \).

Sodium Chlorate is a single and continuous period of cartel effects of which both the begin and the end are dated too early, which is the case illustrated in Section 3.2. The effective cartel begin date matches the Commission’s evidence that the initial cartel meetings in September 1994 aimed to raise prices as of the following year: prices increase steeply from the beginning of year 1995, to peak in 1998. Even though prices subsequently declined in the second half of 1998—in fact with electricity prices, the main cost component in the production of sodium chlorate—they stabilized with the reported resolve of internal stability issues. The effects of the cartel on prices subsequently persisted until the beginning of 2002, when sodium chlorate price fell rapidly, almost 2 years after the formal end date of February 9, 2000. By the time the existence of the cartel was notified to the European Commission, in March 2003, prices had returned to a more stable competitive trend.

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13Sources are respectively: Eurostat, “Electricity—industrial consumers—half-yearly prices”; OECD System of Unit Labour Cost Indicators; CEPI, European Chemical Pulp Production, 1995:Q1 to 2008:Q3; Harriman Chemsult, Chemicals Economics Handbook—SRI Consulting and CMAI.
TABLE 1  Empirical outcomes from structural break tests in Sodium Chlorate

<table>
<thead>
<tr>
<th>μ</th>
<th>Homogeneity 0.10</th>
<th>Heterogeneity 0.10</th>
<th>Asymptotic cv 0.10</th>
<th>bootstrap cv 0.10</th>
</tr>
</thead>
<tbody>
<tr>
<td>sup F(1)</td>
<td>1.36</td>
<td>1.36</td>
<td>1.29</td>
<td>9.10</td>
</tr>
<tr>
<td>sup F(2)</td>
<td>27.34</td>
<td>8.44</td>
<td>29.71</td>
<td>10.07</td>
</tr>
<tr>
<td>sup F(3)</td>
<td>22.81</td>
<td>3.54</td>
<td>24.24</td>
<td>3.87</td>
</tr>
<tr>
<td>sup F(4)</td>
<td>21.01</td>
<td>4.90</td>
<td>21.82</td>
<td>5.49</td>
</tr>
<tr>
<td>sup F(5)</td>
<td>20.16</td>
<td>4.04</td>
<td>19.93</td>
<td>4.10</td>
</tr>
<tr>
<td>UD max</td>
<td>27.34</td>
<td>8.44</td>
<td>29.71</td>
<td>10.07</td>
</tr>
</tbody>
</table>

# breaks 2022

The table reports the results of the structural break tests (sup $F(m)$ and UD max) discussed in Section 4. The first four columns contain the values of the test statistics, for two versions of the tests (“heterogeneity” allows the error distributions to change at the break dates; “homogeneity” does not) and two values of the trimming parameter $\mu = h/T$. The final four columns contain the corresponding 5% critical values (cv). Bootstrap critical values are calculated using 1,000 replications, assuming heterogeneous error distributions (imposing homogeneity leads to very similar critical values.) “# breaks” and “break dates” refer to the number of breaks and their dates implied by the tests at the 5% significance level.

FIGURE 3  Cartel price effects in Sodium Chlorate. The figure displays the actual sodium chlorate prices (solid line, $p$), together with the but-for prices using the effective cartel dates (short-dashed line, bfp) and using the formal cartel dates (long-dashed line, bfpO)

We estimate the dynamic model (Equation 26) with OLS for two date specifications of the cartel dummy. Using the formal dates—that is, September 1994 to February 2000—we obtain

$$\hat{p}_t = 0.783 + 0.013d_t + 0.007x_{1t} + 0.023x_{2t} - 0.005x_{3t} - 0.054x_{4t} + 0.596p_{t-1} + 0.299p_{t-2}. \quad (27)$$

While using the effective cartel dates—that is, January 1995 to February 2002—we find

$$\hat{p}_t = 2.033 + 0.029D_t + 0.056x_{1t} + 0.031x_{2t} + 0.002x_{3t} - 0.145x_{4t} + 0.455p_{t-1} + 0.300p_{t-2}. \quad (28)$$

Using the formal cartel dates returns coefficients of the explanatory variables that are all insignificant, whereas the specification does contain the primary cost factors known to determine price in the sodium chlorate industry. Applying the effective cartel dates instead, coefficients all have the expected sign. Capacity is the most important determinant of price, followed by electricity costs.

The empirical model has strong combined explanatory power, with a large $F$ statistic (827.70) on the overall significance of the regression. For both specifications, the estimated short-run dynamics are adequate; that is, additional lags of the regressors are not significant. Furthermore, Breusch–Godfrey Lagrange multiplier misspecification tests do not indicate
significant residual autocorrelation, which indicates that all observed autocorrelation in the price has been modeled parametrically by the estimated dynamic specification. The predictive power of the estimated dynamic model is superior to the static specification. Various sensitivity checks indicate that the estimation results are robust to changes in functional form and sample period.

Comparing the estimates in Equations 27 and 28, their relative magnitude corroborates the results of Lemma 2. The short-run cartel effect $\hat{\alpha}_2$ is more than twice as large using the effective cartel dates rather than the formal cartel dates (2.9% versus 1.3%). The sum of the autoregressive coefficients is also higher under the formal cartel date specification, whereas the estimated intercept $\hat{\alpha}_1$ is lower.

Using the effective cartel dates specification, but-for prices are constructed by recursive dynamic simulation as follows:

$$bfp_t = 2.033 + 0.455bfp_{t-1} + 0.300bfp_{t-2} + 0.056x_{1t} + 0.031x_{2t} + 0.002x_{3t} - 0.145x_{4t},$$

from January 1995 onwards. The observed prices in November and December 1994 are used as initial values of the period-by-period but-for simulation; that is, $bfp_{1994:11} = p_{1994:11}$ and $bfp_{1994:12} = p_{1994:12}$. In Figure 3, these are plotted as the dotted line (labeled bfp). While indeed the cartel started to lose its effectiveness in raising price in February 2002, the but-for and the actual price series are fully converged only from February 2003, which identifies the end date of the cartel effect. This lingering effect is consistent with contracts and negotiations common in the industry, as well as the late leniency application.

Similarly, but-for prices using the formal cartel dates are simulated as the dash-dotted line (labeled bfp0). On average, the but-for prices estimated on the basis of the formal dates are 0.95% higher than the but-for prices corresponding to the effective dates, which corroborates Proposition 3(i)—notwithstanding the fact that the formal dates but-for prices are lower than the effective dates but-for prices during the period 1996–1997.

The overcharge estimates are also in conformity with the theoretical predictions. Using the effective dates, we find that the average $\bar{O}_1$ and $\bar{O}_2$ overcharges are almost equal, corroborating Proposition 2. Moreover, using the formal cartel dates, the average $\bar{O}_1$ and $\bar{O}_2$ overcharges are respectively 9.33% and 8.02% lower than their counterparts using the effective dates, which is in line with Proposition 3(ii). The downward bias in overcharge estimation is more or less equal for $\bar{O}_1$ and $\bar{O}_2$.

Total damage estimates $\hat{CD}_1$ and $\hat{CD}_2$ are almost identical under the effective dates. From Theorem 1, this indicates that sodium chlorate demand has a very low own-price elasticity. Indeed, estimation of a dynamic regression of sodium chlorate quantities on prices shows an insignificant price effect. This is consistent with demand for sodium chlorate use in bleaching being driven mainly by pulp and paper production and prices, the availability of only imperfect and more expensive substitutes, and the cost for sodium chlorate being a negligible cost factor in chemical pulp production.

The damage estimates $\hat{CD}_1$ and $\hat{CD}_2$ are respectively 28.32% and 28.57% lower using the formal rather than the effective cartel dates, which is consistent with Theorem 3. The largest part of this difference is due to the fact that the formal end date lies before the effective end date—that is, area $D$ in Figure 2—which accounts for 25.68% and 23.95% lower damage estimates $\hat{CD}_1$ and $\hat{CD}_2$. These significant differences clearly illustrate the anomalous effects of misspecifying cartel begin and end dates: It results in downward bias that can be large in comparison to proper dating. The bias is due to both lower overcharges and a shorter damage period.

6 | CONCLUDING REMARKS

We have shown that proper cartel dating is crucial for obtaining accurate estimates of cartel damages. Using misclassified cartel begin and end dates leads to a (weak) overestimation of but-for prices and an underestimation of overcharges. When overcharges are defined as the difference between actual prices and but-for prices, the resulting damage estimator is conservative in the sense that it always leads to a (weak) underestimation of the true damage. While longer formal cartel periods subsume more volume under the damage claim, but-for prices are (weakly) overestimated, so that volumes purchased in falsely alleged cartel period parts are premultiplied by nonpositive overcharges. Instead, using the difference between predicted cartel prices and but-for prices, both over- and underestimation of cartel damages can occur.

The misdating bias can be avoided with econometric tests for structural change. Even with proper empirical cartel dating though, when the effective cartel dates are unknown, the preferred approach to cartel damages estimation is to compare estimated but-for prices to observed prices. The approach is conservative when the effective cartel dates are exactly estimated, and remains so for an error margin. It is more robust against misdating than comparisons with predicted
cartel prices, which recent reports appear to favor. The empirical findings in Sodium Chlorate corroborate our theoretical results. Using the formal cartel dates estimates damages more than 25% lower.

Under tort law, the injured party is entitled to compensation from the wrongdoer for harm that is caused by—and typically follows—the tortious act. Damages resulting from the collusion outside the formal cartel period(s) should therefore in principle be permitted as part of an antitrust claim, including post-cartel tacit collusion and lingering effects. Together with qualitative evidence of the cartel’s modus operandi, cartel dating can help corroborate or falsify the collusion as the legally recognizable cause of the harm. Plaintiffs in an antitrust damages action should apply dating also to avoid the risk of leaving part of their actual cartel damages unclaimed. Defendants can use the techniques to demonstrate an alleged cartel to be ineffective. Agencies may want to consider estimating effective cartel dates for the purpose of setting deterring fines.

Detailed knowledge of the cartel’s price dynamics remains essential also to determine the likely direction of bias in estimating the cartel effects. During price wars that are part of the cartel spell, prices may be lower than in competition, as in Abreu et al. (1986) optimal temporary punishment strategies. Cyclical cartel price dynamics as in Rotemberg and Saloner (1986) can instead include low price episodes in which there is still a substantial overcharge—although gradual price movements in continuous collusion may not give rise to structural breaks, as in Sodium Chlorate. Including such intermittent episodes in the but-for price estimations can bias them, respectively down or up.

If there is indication that intermittent pricing is different from Nash reversion, these episodes can be identified as separate regimes by appropriately including more than one dummy, as used in the structural break tests.¹⁴ Likewise, if cost elasticities are structurally higher in competition than collusion, or capacity management is tighter, an extended empirical cartel dating procedure can determine such additional cartel effects as parameter breaks between collusive and competitive regimes.

Alternatively, once the effective cartel dates have been determined, but-for prices can be predicted using the forecasting approach from data outside the effective cartel period(s), as well as any intermittent period(s) identified as other than competitive pricing. Properly dating the cartel assures that only data from non-cartel period(s) is used. Preliminary findings indicate that our results on misdating bias also apply to the forecasting approach, so that dated forecasting would minimize underestimation of total damages as well. We leave for future research too analysis of autoregressive dynamics when multiple effective cartel periods alternate with competitive episodes.

ACKNOWLEDGMENTS

We thank Dennis Carlton, John Connor, Carsten Crede, Wim Driehuis, Tomaso Duso, Hans Friederiszick, Olav Haazen, Joe Harrington, John Kwoka, Bob Marshall, Dan Rubinfeld, Craige Romaine, Lily Samkharadze, Till Schreiber, the co-editor, two anonymous referees, and participants in various conferences for discussions and comments to earlier versions of this paper. Opinions and errors remain ours. We are grateful to Cartel Damage Claims (CDC), Brussels, for providing us with the data. Bun and Schinkel acted as expert witness on behalf of CDC in litigation against members of the European Sodium Chlorate cartel.

REFERENCES


¹⁴Ellison (1994) revisits the data on price wars during the Joint Executive Committee railroad cartel analyzed in Porter (1983), to conclude that they were most likely of the Nash reversion type explained in Green and Porter (1984).


**SUPPORTING INFORMATION**

Additional Supporting Information may be found online in the supporting information tab for this article

**How to cite this article:** Boswijk HP, Bun MJG, Schinkel MP. Cartel dating. *J Appl Econ.* 2019;34:26–42. https://doi.org/10.1002/jae.2660