THE ECONOMETRICS OF DATING ANTI-COMPETITIVE EFFECTS WITH APPLICATION TO EXCESSIVE PRICING

by

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Declaration

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Abstract

Anti-competitive behaviour in a market is associated with related effects that may not always be captured in traditional approaches to locating damages. Conventionally, to determine when anti-competitive behaviour have started, prevailed, and ended, analysts rely on the formal documentary evidence presented in the case. However, the associated effects may have lasted before and/or after the formally established liability date. Such effects are central to proper damage estimation and may even have an impact on establishing the existence of conduct in the first place. With the focus on excessive pricing, this study firstly analyses the various benchmarks used locally and abroad to determine excessive pricing and finds that such benchmarking is done non-econometrically despite the need to properly control for demand and supply factors. This study then applies what is known from collusion literature to the dating of excessive pricing. Collusion literature shows that there is a problem with pre- and post-effects, and it is often necessary to differentiate between formal and effective dates. Furthermore, this study examines current policy approaches to determining anti-competitive dates and finds that precise dating is less important in pre-Covid-19 cases. However, there is a problem with dating Covid-19 excessive pricing cases because of the relevant regulations specifying the fixed 3-month comparative period. Econometric methods are particularly important in this regard and will be for excessive pricing cases to come. This study supplemented the traditional counterfactual methodology with a structural break test and a Markov switching-regime test to properly determine the effective dates of the case at hand. The application is to the *Foskor* excessive pricing case. This quantitative approach shows that, although the actual effect may precede or follow after the formal identified dates, wrongly specifying the relevant periods leads to a significant underestimation of the damages in the case at hand.

Opsomming

Teenmededingende gedrag in 'n mark hou verband met effekte wat nie altyd vasgevang kan word in die tradisionele benadering tot die opsporing van skadevergoeding nie. Om vas te stel wanneer teenmededingende gedrag ’n aanvang geneem het, voortgeduur het en beëindig is, word gewoonlik van die formele dokumentêre getuienis, wat in die saak na vore tree, gebruik gemaak. Die gepaardgaande effekte kan egter voor en/of na die formele vasgestelde aanspreeklikheidsdatum voorkom. Sulke effekte is egter noodsaaklik in die korrekte beraming van skade en kan selfs 'n invloed hê op die bepaling van die teenmededingende gedrag in die eerste plek. Met die fokus op buitensporige prysbepaling, ontleed hierdie studie eerstens die verskillende maatstawwe wat plaaslik en in die buiteland gebruik word om buitensporige pryse vas te stel, en bevind dat sulke maatstawwe hoofsaaklik nie-ekonometries van aard is, ondanks die behoefte om vraag- en aanbodfaktore in ag te neem. Hierdie studie maak dus op die samespanningsliteratuur staat om die ekonometriese datering van buitensporige pryse te bestudeer. Die samespanningsliteratuur toon dat daar 'n probleem is met voor- en na-effekte, en dat dit dikwels nodig is om te onderskei tussen formele en effektiewe datums. Verder ondersoek hierdie studie huidige beleidsbenaderings tot die bepaling van mededingingsdatums en vind dat presiese datering minder belangrik is in sake voor Covid-19. Daar is egter 'n probleem met die datering van sake rondom buitensporige pryse wat met die Covid-19-pandemie verband hou, aangesien die tersaaklike regulasies ’n vaste vergelykingsperiode van drie maande spesifiseer. Ekonometriese metodes is veral belangrik in hierdie verband en vir toekomstige buitensporige prysgevalle. Hierdie studie het die tradisionele kontra-feitelike metodologie met 'n strukturele-breek-toets en 'n Markov-omskakelingstoets aangevul om die effektiewe datums van die betrokke saak te bepaal. Dit is toegepas op die *Foskor*-saak vir buitensporige pryse. Hierdie kwantitatiewe benadering toon dat, aangesien die werklike effek die formeel geïdentifiseerde datums vooraf kan gaan of kan volg, die verkeerde effektiewe tydperk tot 'n aansienlike onderskatting van die relevante skadevergoeding kan lei.

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1. Introduction

Anti-competitive behaviour in a market is usually associated with related effects that may not always be captured in traditional approaches to locating damages. Following the European position, South Africa deems anti-competitive conduct to have started, prevailed, and ended based on the formal documentary evidence at hand (Boswijk *et al.*, 2019:26). However, the associated effects may have lasted before or even long after the formally established liability date. Such effects are central to proper damage estimation and may even have an impact on establishing the existence of conduct such as excessive pricing in the first place.

Thus, establishing the appropriate dates or periods when such anti-competitive effects were truly being felt in the market is an important and often neglected issue in competition cases.[1] The use of different econometric techniques may assist in identifying case-specific effects in the market. The quantitative approach to this task shows that although the actual effect may precede or follow after the formal identified dates, wrongly specifying the relevant periods leads to a significant underestimation of the damages in the case at hand.

This paper focusses on excessive pricing. Section 2 analyses the various benchmarks used locally and abroad to determine excessive pricing and the conditions in which the most recent investigative developments (intertemporal benchmarking) are preferable: which are where there is a comparable competitive period and a structural break that delineates a competitive from a non-competitive period (Boshoff, 2020:6). It finds that such benchmarking is done non-econometrically despite the need to properly control for demand and supply factors. There has recently been an emphasis in collusion literature on dating. The literature shows that there is a problem with pre- and post-effects, and it is often necessary to differentiate between formal and effective dates. Applying what is known from collusion literature, this paper focusses on the dating of excessive pricing. Section 3 of this paper examines current policy approaches to determining anti-competitive dates, cases of abuse other than

[1] In this study, different areas of anti-competitive conduct such as the abuse of dominance and collusion are used interchangeably to a certain extent. This is because the main objective is to use econometrics to capture any anti-competitive structural change irrespective of the specification of the anti-competitive behaviour. Also, many overlapping principles will be addressed, but the commonality here lies in the fact that there is a change which needs to be captured in the econometric method, irrespective of what anti-competitive behaviour drove the change. For example, guides on quantifying damages make it clear that "the harm caused by excessive pricing is similar to that caused by cartels: a firm with significant market power restricts output and raises prices. This leads to the same two types of harm as in a cartel case".

excessive pricing, and cases of excessive pricing, and finds that precise dating is less important in previous pre-Covid-19 cases, but there is a problem with dating Covid-19 excessive pricing cases because of the new regulation specifying the fixed 3-month comparative period. Econometric methods are particularly important in this regard and will be for excessive pricing cases to come. Section 4 describes the methodologies used and section 5 shows their application to the *Foskor* excessive pricing case.

2. Literature review

To understand the dating of anti-competitive effects with application to excessive pricing, this section first explains how to determine excessive pricing by analysing the possible benchmarks in South African and international competition law to which the investigated price may be compared. It also identifies relevant conditions in which retrospective or so-called intertemporal benchmarks are preferable, as suggested by recent developments. Secondly, this section addresses the reasons why the intertemporal benchmarking method is traditionally followed in South African cases without considering econometric methods, and then explains why there is a need to consider such econometric methods when dealing with the dating of such cases. Thirdly, this section discusses the recent emphasis in intertemporal benchmarking in collusion on dating with regard to the problem of pre- and post- effects, thereby creating different formal and effective dates, which is also applicable to cases of excessive pricing. The section concludes by showing how dating is important in cases of excessive pricing both with regard to the calculation of damages and for the purposes of detecting such conduct.

2.1 Determining excessive pricing

In comparison with international practice, South Africa has been very active in mitigating the "risks of, and prosecuting, excessive pricing" during Covid-19 (Boshoff, 2020:2). The international approach to addressing pricing issues in this time of crisis seems to rely mainly on competition policy (Boshoff, 2020:19). It should be noted at this stage that not all other jurisdictions prohibit exploitative abuses. The United States anti-trust policy, for instance, reasons that the US does not prosecute dominant firms charging higher prices because they do not want to deter innovation (Boshoff, 2020:2). South Africa follows the position of the European Union, where charging excessive prices is in fact prohibited (Boshoff, 2020:2). According to Gilo (2018) international practice in this regard features comparative benchmarking and cost-based benchmarking to obtain a more competitive benchmark for use in establishing excessive pricing. Generally, international practice seems to tend towards the use of intertemporal benchmarks (embedded in comparative benchmarking) as being preferable to other benchmarking systems as the data for drawing such comparisons seems to be more easily accessible and it limits the need for complicated cost and profit calculations (Boshoff, 2020:19).

South African competition law, prior to the Covid-19 pandemic, in a sense gave limited guidance on determining excessive pricing. Section 8(a) of the Competition Act 89 of 1998 (Republic of South Africa, 1998) "prohibits a dominant firm" from charging a so-called excessive price to consumers where an excessive price is defined as a higher price "that bears no reasonable relation to the economic value of that good or service." The essential problem with this concept is that the authorities will usually have to consider what constitute the economic value in question, without having a definition or measurement of it, and then determine what price is reasonable in relation to this economic value (Theron, 2019). To determine whether a price is excessive or not, it must be compared with some price benchmark (Gilo & Spiegel, 2018). The complexity of determining an appropriate benchmark to identify malpractice is clear. In fact, this definition of economic value has been abandoned and has been replaced in section 1(b) of Act 18 of 2018 by a set of factors to be taken into consideration in such matters. [2] From an economics perspective these factors are quite "close to the factors" that the authorities are already considering when determining economic value, and it is thus uncertain what the explicit listing of them really contributes to the issue. In other words, listing them does not resolve the complexity of obtaining a "competitive benchmark price" (Boshoff, 2020:4). One factor that may be emphasised in the context of this study (in Section 8(3)(d)) is the "length of time the prices have been charged" at that excessive pricing level. In the *CC v Dis-Chem Pharmacies* (CR008Apr20) excessive pricing case, it is stated that the deliberate removal of the concept of economic value in the Act poses an "intention to exclude" it completely, and instead one is required to look at a "competitive price" to detect whether a price is excessive. It is important to emphasise a well-known economic fact early on in this study: a competitive price is a price determined by supply and demand. Up until the end of 2019, excessive pricing had been proved in

[2] According to section 3 of the Act, these factors may include: "the respondent's price-cost margin, internal rate of return, return on capital invested or profit history; the respondent's prices for the goods or services; relevant comparator firm's prices and level of profits for the goods or services in a competitive market for those goods or services; the length of time the prices have been charged at that level; the structural characteristics of the relevant market; and any regulations made by the Minister".

no South African cases (all such cases having been settled) due to the different possible interpretations of what constitutes excessive pricing (Theron, 2019). Thus, the challenge with excessive pricing is clear: one needs to determine an appropriate competitive benchmark price to compare with the price in question (Boshoff, 2020:5). This is not a simple task, and its complexity is probably the reason why excessive pricing cases before the national disaster, both internationally and locally were limited to very specific settings: usually state-supported, high-end, and large corporations (Boshoff, 2020:1).

However, after the declaration of a national state of disaster by the President of South Africa in March 2020, new regulations regarding excessive pricing on certain products and services were published.[3] Although these regulations are valid only for the period of the proclaimed disaster, they suggest a benchmark for use in judging the setting of excessive prices (Boshoff, 2020:1). Specifically, section 4 of the said regulations had a significant influence on the judging of two instances when a price was *prima facie* excessive: first, a price increase that did not correspond to the "increase in the cost" of providing that product, and secondly, a price increase which resulted in bigger profit margins than in the three month period before the disaster was proclaimed (up to 1 March 2020).[4] This comparison constitutes the so-called intertemporal benchmark (comparisons drawn across different time periods), is in accordance with the international practice, and may lessen the complexity of finding an appropriate competitive benchmark (Boshoff, 2020:19). Analysing specific factors in a firm under investigation and comparing them with the same factors in the same firm in the pre-disaster period eliminates the need to scrutinise other factors that are difficult to measure, as discussed later. Therefore, the most recent suggested benchmark is the same market (as that which is under investigation) in an earlier time period, when there are enough available data for the earlier period (Boshoff, 2020:6). The period before the disaster is assumed to be the period used for the purposes of comparison, because the firm had not then had the opportunity, in the absence of the unique circumstances of Covid-19, to abuse its dominance by charging an excessive price. In the *CC v Dis-Chem Pharmacies* case, well known competition economist Massimo Motta supported the

[3] Despite the publication of block exemptions for various sectors, and the more procedural tribunal rules for complaint referrals, the regulations which are of importance here are the Consumer and Customer Protection and National Disaster Management Regulations and Directions (GNR.350 of 19 March 2020) hereafter referred to as the regulations. The Minister of Trade and Industry has also published regulations in terms of the South African Consumer Protection Act 68 of 2000, in terms of "unconscionable, unfair, unreasonable and unjust" pricing, but this as well, is not the focal point of this study.
[4] These regulations apply to goods and services broadly listed in Annexure A: "Basic food and consumer items; emergency products and services; medical and hygiene supplies; emergency clean-up products and services".

use of the pre-disaster period as the competitive period, because according to him, the demand and supply conditions during that time were "presumably normal".

It is also important to note that with these new regulations a shift of focus is made in terms of conventional views of market power, as more smaller players are now also involved in excessive pricing cases. Boshoff (2020:3) explains that smaller players may gain temporal market power due to the abnormal circumstances of a crisis like Covid-19, for example, if the disaster generates a change in the behaviour of customers or fewer firms can operate fully, limiting the choice available to consumers. Such market power will influence the possibility of intervention (Boshoff, 2020:1). Conventional policy, both local and abroad, was more likely to consider market power in the long run and the ability to sustain it (Boshoff, 2020:3). However, recent South African cases have focussed on shorter time periods, which shows that policy is now more concerned with price increases in the short run (Boshoff, 2020:3). Even though there are contrasting views about market power and specifically this new temporal market power, this study will assume that a firm in question does have some form of market power, as the focus is on determining the excessive pricing that follows the exercise of such power.

The fact of the matter is that even with the change in the conventional approaches arising from the new regulations, determining excessive pricing is still a complex matter and care should be exercised in finding an appropriate competitive benchmark. The type of benchmark chosen depends on the circumstances at hand. The intertemporal benchmark is the option favoured in Covid-19-related investigations, but there are also other possible benchmarks that could be of use in detecting excessive pricing. In earlier South Africa cases, such as the *Mittal* and *Sasol* cases, the authorities preferred to reference a bottom-up, cost-based benchmark, and rightfully so, as it was more appropriate to the circumstances of those cases. They lasted for many years (excessive pricing has been an issue for a long time) with no clear break between a competition- and anti-competition period and an intertemporal benchmark would therefore not have been an appropriate fit (Boshoff, 2020:7).[5] A cost-based approach involves determining the "average cost" for the product or service under investigation and then identifying a reasonable profit margin (Boshoff, 2020:5). Such an approach clearly has its own challenges, such as determining what profit margin would apply in a more

[5] What happened in many earlier collusion cases is that a certain period was chosen to do a before-during-and-after analysis, but it reflected no changes in the periods, as the firms kept their high built-in premiums throughout all the periods, which portrayed them as responding to cost and demand conditions in the same way over time, while hiding the overcharge or premiums.

competitive market, or translating "accounting costs" into "economic costs", and would normally be preferred only when a comparable market or firm is not available (Boshoff, 2020:5). International authorities are hesitant to use a cost-based approach and tend to use comparator benchmarking. This involves the use of a competitive benchmark where the investigated price is compared with the price charged contemporaneously in another similar market by the same firm or by similar firms (Gilo & Spiegel, 2018:1). For example, one might compare a dominant firm's local prices with its export prices to establish whether the price charged on the local market is too high. This is called the spatial approach in cartel literature, and it will be used in the *Foskor* case study presented later in this paper. The challenge with this approach, however, is to account for all the idiosyncratic errors across different markets, for example. Akman and Garrod (2011) recognise that some features in terms of demand, supply and the structure in different markets meant for comparison, may be unobservable. That is why it is easier to look at the prices set by the same firm in different time periods as there are fewer incommensurable variables to take into consideration (Gilo, 2018). Nevertheless, the intertemporal benchmark is nested within the comparator benchmark set, and this set is the dominant set used at present.

The intertemporal benchmark approach is likely to be successful under only two conditions. Firstly, it is vital that there must be an exogenous structural break or shift that delineates a competitive from a non-competitive period (Boshoff, 2020:6).[6] A structural shift may come in different forms: it could be a factor causing more or fewer competitors to enter the market, which would change the competitive conditions but not the demand and costs, so that the increase in price could be directly linked to the change in competitive conditions; it could be a change in the regulative environment, as when a regulated price period is seen as the competitive period and deregulation is seen as the shift; or it could simply be a change in consumer behaviour (the consumers might become more sensitive to prices, for example) (Gilo, 2018). It is important, however, that such shifts should be significantly linked to the relevant changes in competitive conditions (Boshoff, 2020:7). For instance, the national disaster has resulted in a situation where there was a more competitive period before the disaster than after, which has allowed for price changes related to the changes in the conditions of competition (Boshoff, 2020:7). One might have to support and verify the impression that there have indeed been changes in the competition conditions in any particular case, by assessing market power for example, but Covid-19-related cases are nevertheless very useful for fixing an

[6] The necessity of an exogenous structural shift is well illustrated in cartel damage calculation, where it is used in the overcharge estimation between collusive and non-collusive periods.

intertemporal benchmark. This is so because the starting date of the proclamation of the disaster presents a clear structural break in the conditions of competition. For example, consumers were limited to stores that could stay open at critical times, or might have been prepared to pay more for a product to limit their exposure to an open environment. With this clear shift or break – when one can assume demand and cost conditions were similar across the period (which is the crux of the matter, and the second condition in the next paragraph) – one may then attribute any rise in prices to the change in the conditions of competition, showing that excessive pricing is taking place.

Secondly, Boshoff (2020:7) explains that there also needs to be a comparable competitive period. This means that the cost and demand conditions in the different periods must be similar, so that the competitive effects may be isolated (Akman & Garrod, 2011). One must be aware of the different possible demand and cost shocks (one must account for such different conditions) that may exist throughout the different periods. Put differently, obtaining a comparable benchmark that reflects a more competitive stance requires conditioning for both cost and demand side factors in the different periods (Gilo, 2018). Accounting for cost and demand is in line with international practice. As noted previously, a competitive price is one determined by both supply and demand. The regulations allow for conditional comparisons on cost increases but do not clarify how changing demand conditions will be treated. Basically, changes – increases - in demand will have their effect in the profit margin (making it significantly larger) which is prohibited through excessive pricing. This indicates that authorities do not want firms to respond to demand in such disaster periods. In considering profit margins, one would still consider unchanged demand, but essentially firms are prohibited to respond to the surge in demand created by Covid-19. This is a strong position to take, and not allowing for responses to changing demand may be devastating to the market, especially if it results in shortages in supply, as it may disincentivise increasing supply to respond to demand. Boshoff (2020:10) makes it clear that if such regulations treat demand-based price increases as if they are necessarily anti-competitive, this will undermine the consistency of intertemporal benchmarking, and it is unclear why price-demand relationships are not accounted for. The regulations seem to be creating a stricter test or benchmark than general excessive pricing approaches to compare prices against. The regulations are acting in a price-gouging way by not controlling for demand, and although it is much easier for the authorities to apply such a benchmark, it may not be appropriate. Even in very competitive markets, prices can rise significantly in response to cost or demand surges, and this is not necessarily incompatible with competitive behaviour (Boshoff, 2020:20).

Boshoff (2020:11) ran simulations to determine when the regulations' benchmark might work. Note that when a sustained price increase in a specific case is very high, for example a 300% mark-up, it will exceed both the general excessive pricing benchmark and the regulations' stricter benchmark. Thus, in such cases it would not be difficult to identify excessive pricing. However, Boshoff (2020:11) notes that cases between these benchmarks will pose much more difficulties. He finds that the regulations' stricter benchmark will not be consistent with normal excessive pricing intertemporal benchmarks if the higher demand is sustained rather and not a mere spike (Boshoff, 2020:16). Section 2.2 of this paper will show that when dealing with smaller firms, one is mostly concerned with a spike in demand, probably due to the firms facing uncertainties. This is not serious in terms of excessive pricing, and the stricter benchmark may as well be used. However, with larger firms with a more established position in the market one may be faced with elevated demand which calls for a proper consideration of the price-cost *and* price–demand relationships. The next section of this paper will study the related excessive pricing cases to get more clarity on how the regulations are applied. There is no doubt that the intertemporal benchmark approach has the potential to overcome the challenges of accounting for demand changes between the periods. It also prohibits the "full exploitation of willingness-to-pay" in line with the act's objectives by not allowing for sources of market power like price increases due to structural shifts in demand (Boshoff, 2020:10).However, these advantages will only occur if the benchmark is applied properly.

Determining excessive pricing is without a doubt a challenge, and an appropriate case-to-case, benchmark with which to compare the price under investigation against is unavoidable. Various benchmarks have been developed locally and abroad, including the cost-based and comparative benchmarks as already discussed. Most recent developments, especially with regard to Covid-19-related investigations, prefer an intertemporal benchmark in assessing excessive pricing, which requires the presence of a comparable competitive period and a structural break that delineates a competitive from a non-competitive period (Boshoff, 2020:19). Whether this preference will give rise to a permanent change in competition law will depend on case-by-case developments and it is yet to be discovered how the situation will play out after the Covid-19 disaster period. The important thing is that even though the intertemporal benchmark is the most appropriate benchmark to use, caution must be exercised when applying this system in its strictest form to cases with slight price increases where there is sustained higher demand.

2.2 Intertemporal benchmarking in South African cases

Intertemporal benchmarking is in fact not traditionally the preferred method in South African cases dealing with excessive pricing, but Covid-19 has made it so. There have been various new excessive pricing cases in South Africa since the new regulations were published. This section of the paper will analyse how intertemporal benchmarking is performed in South Africa, based on these cases.

The more recent excessive pricing cases are against firms (smaller pharmacies in narrow geographic markets and some bigger retailers like Dis-Chem) selling facial masks, sanitisers, and other Covid-19-related products. In most of these cases – those where the product was not being sold for the first time - the intertemporal benchmark was indeed used. In studying the cases against the smaller firms, Boshoff (2020:7) found the following:

> price responses to demand and cost were relatively quick in these cases, implying a shorter interval for dynamic price adjustment. In addition, it would have been relatively clear in these cases that the demand spike had subsided after a few weeks. Consequently, these cases involve a reasonably straightforward judgment of whether price continued to exceed the higher level justified by the initial demand spike and, hence, whether the price is excessive.

Thus, the benchmark applied by the authorities by looking at costs and profit margins was appropriate against these firms, which were facing a demand spike. Table 1 shows some of these recent cases at the time of writing, but the list could be extended in similar fashion, with new cases of this type coming up until early March. As suggested by the regulations, the authorities mostly have considered a comparator consisting of the price or profit margin in an earlier period, and in some cases the cost implications. The current methods applied may be appropriate when there is merely a once-off spike in demand, but it is important to note that cases facing consistently higher demand will not yield standard averages if one accounts only for cost drivers (Boshoff, 2020:19). Then it would be more appropriate to apply the conventional excessive demand approach used in South Africa before these recent developments took place (Boshoff, 2020:11). Boshoff (2020:11) also emphasises the importance of understanding the elasticity of price, especially regarding elevated demand, and what competitive reaction may be expected. Only two such cases have been contested so far: *CC v Babelegi Workwear (CR003Apr20)* and *CC v Dis-Chem Pharmacies (CR008Apr20)*, which shed some light on excessive pricing issues.[7] *CC v Babelegi Workwear (CR003Apr20)* will be analysed in depth in section 3.3 of this paper. In *CC v Dis-Chem Pharmacies*, the same stricter benchmark was used, which may be contestable, as Dis-Chem, a larger, well-established firm, is very

[7] It is important to emphasise that these cases were dealt with under the existing competition legislation (as opposed to under the new regulations) and is therefore very applicable to traditional excessive pricing cases under the Act.

likely to be faced with elevated demand, especially when considering reasonableness. In such cases the analysis is much more complicated than simply considering costs and constant profit margins. Persistent higher demand must in fact be reflected in the competitive prices. No econometric models were used in any of these cases for example to analyse the demand involved. From the discussion in section 2.1 it is clear that there is a need for such models, especially in in-between cases with sustained demand, as the current "silver-bullet" way of estimating whether or not a price is excessive does not account for demand-side factors and may result in over- and under-enforcement errors.

The reasons why the Tribunal is not using an econometric method for detecting excessive pricing cases may include practical considerations, especially regarding the short time intervals over which these new cases persist (Boshoff, 2020:11). Considering only applicable cost changes and profit margins is understandably much easier to prosecute, and as seen above, may be appropriate in *some* cases.

Table 1: Covid-19 excessive pricing (source: https://www.comptrib.co.za/cases-current)

Order date	Excessive pricing case	How did the tribunal identify excessive pricing
2020-07-08	CC v Mica Barberton	Non-econometrically. The price of facial masks in the disaster period was compared to what it had been before 1 March 2020. The CC found a 711% mark-up and that it constituted a contravention of the Act.
2020-07-08	CC v Green Hygiene	Non-econometrically. Green hygiene sold sanitizer dispensers at a 45,8% mark-up during the disaster period, which constituted a contravention of the Act.
2020-07-08	CC v Eldoram Dienste CC t/a Eldopark Pharmacy	Non-econometrically. Prior to the disaster, there was a 39% mark-up on facial masks; after March, a 54% mark-up. They also started selling new products in this regard. The commission investigated the costs of such products and calculated the mark-ups, which they found to be "unreasonably high".
2020-07-07	CC v Dis-Chem Pharmacies	Non-econometrically. The CC compared the prices of masks before and within the disaster period and considered the cost increase in relation to the price increase and noted that the latter occurred first, which meant that the demand drivers had not been considered (Boshoff, 2020:18). Awaiting appeal at the time of writing.

2.3 Intertemporal benchmarking in collusion on dating

Section 2.1 of this paper discussed the intertemporal condition of a structural break. There has been recent emphasis on this in addressing collusion on dating. Collusion models also compare prices during the existence of a cartel with prices prior to or after the cartel (a more competitive period) to estimate the price overcharges which form the basis of calculating damages (Boshoff, 2020:6). The same challenge is faced in cases of excessive pricing: to determine the overcharge, which is simply the excessive amount charged over and above a competitive price (Boshoff, 2020:11). Therefore, the literature on collusion offers an econometric methodology for arriving at benchmark price by using a reduced-form regression (Boshoff, 2020:11).

As explained in the previous section, it is necessary to apply an econometric analysis when dealing with some excessive pricing cases in order to assess "the dynamic relationships between cost, demand and price" (Boshoff, 2020:11). Dynamic econometric models can capture the possible *effects* of the relations between price, cost drivers and demand drivers. The challenge identified in the cartel damage literature is that the relevant cartel period (identified via formal qualitative methods) is not necessarily the same as the relative effective period[8]. Competition authorities identify the period of collusion with seeming precision based on documentary evidence of communication, the so-called formal dates, but these dates are in effect rarely as straightforward as they seem to be when they are conventionally dealt with (Boswijk *et al.*, 2019:26). The starting point of the period of liability identified by authorities is the date that may be listed on such records as "notes from diaries, records of meetings, emails referring to meetings or exchange of information, and memos describing pricing schemes" initiating the anti-competitive incentive (Davis and Garcés, 2010:376). The formal end of the liability period is assumed to be when the suspected firm is raided, together with the evidence of "notifications, guilty pleas and consent agreements" (Boswijk *et al.*, 2019:26). Thus, the conventional position is as follows:

> …whenever there is consensus and the parties agree, in any form, on anti-competitive behaviour, or if it entails abuse of dominance (only one firm) – whenever that firm agrees on anti-competitive incentives, then that date up until the date when the party is raided, is taken as the official period of the anti-competitive effect (Bredenkamp, 2019:3).

In contrast, the effective date is not normally found on formal documentation, but is instead when the effect of the conduct occurs in the market. It is understandable that one would tend to accept

[8] Boswijk, Bun, and Schinkel (2019) show the difference between the liability and formal periods empirically in the Sodium Chlorate cartel in the EU, and Boshoff and Van Jaarsveld (2019) also address misdating in the South African context.

formal evidence as the period indicators at first sight, due to its simplicity and uncontroversial appeal (Davis and Garcés, 2010:376). Thus, in some instances, especially with smaller and less complex firms, the formal evidence may prove to be the appropriate period at hand, but it is more often the case that anti-competitive conduct has an effect in the market that lingers or lags over periods different from those noted in formal evidence. The concept of lingering effects has drawn some attention in the literature. It simply refers to when anti-competitive effects last beyond the initial end point, or drag on as opposed to having an immediate end. Boswijk *et al*. (2019:26) go so far as to say that there is no economic reason why the formal dates should reflect the collusive effective dates. When the legal liability period does not coincide with the period of effect, this may relate to: “the nature of the conspiracy or industry” (Boswijk *et al.,* 2019:26); the size of the firm under investigation (large firms with more branches and various different agreements with agents might take longer to adjust prices than smaller firms) (Bredenkamp, 2019:4) ; the nature of the anti-competitive agreement (Boswijk *et al.,* 2019:26); the lags involved in raising prices and menu costs; possible future developments such as “planned price rises, the steady dismantling of capacity, or the postponement of innovation” (Boswijk *et al*., 2019:26); anti-competitive behaviour like cartels which normally form gradually (members join sequentially) (Boswijk *et al*., 2019:26); cartel firms withholding the anti-competitive actions after the initial cartel agreement meeting to circumvent getting caught, or firms stopping communication when raided and having no incentive to change their anti-competitive behaviour due to the already weaker competition conditions (Bredenkamp, 2019:4); anti-competitive behaviour normally having lasting lingering effects and possibly being subject to a settlement negotiation period (Boswijk *et al*., 2019:26); sunk investments delaying the anti-competitive operations (Fagart & Boshoff, 2019); customers getting accustomed to the anti-competitive conditions (this can be due to benefit programmes or lengthy corporate agreements or longer recovering market shares) (*Comair Limited v South African Airways (Pty) Ltd* [2017] 2 All SA 78 (GJ)); “structural effects of the infringement that may be difficult and lengthy to undo (existing contractual obligations, network effects, or other barriers to the re-entry of a foreclosed competitor)” and “compensation not only for the profits lost during the infringement period but also for the profits foregone after its termination” (The Practical Guide Quantifying Harm in Actions for Damages Based on Breaches of Article 101 or 102 of the Treaty on the Functioning of the European Union, 2013:55); input price rises reflecting only months later in downstream products (for example, if Foskor raise the price of fertilisers, although their customers pay the higher price at that moment this may reflect in food prices only later); and the list goes on. To make the problem even more clear, consider that when a CEO sends an email to another CEO incentivising a cartel, the date on that email is not necessarily the exact date on which all the members involved raised their prices (Bredenkamp, 2019:4).

When one analysing these factors as they come into play in different cases, it becomes evident that in regard to the starting point, the effects of the conduct may lag behind the date formally identified by the authorities, and in regard to the identifying the end of the period when the conduct took place, its actual effects may end much later than the identified formal date of closure. Anti-competitive behaviour might have lasting incommensurate effects or could become ineffective long before the members formally disband (Boswijk *et al.*, 2019:26).

It is clear that pre- and post- effects in any anti-competitive behaviour, including the abuse of dominance by asking an excessive price, is a real and robust possibility and one therefore needs to be aware of the possible difference between the traditional formal dates and the relevant effective dates. The next section of this paper will explain why the dating of these effects matters in excessive pricing cases.

2.4 The importance of the dating of excessive pricing

The dating of excessive pricing is vital not only to the proper calculation of the relevant damages, but also in detecting the conduct in the first place. From the previous section, it is clear that with regards to Covid-19-related cases, excessive pricing is detected by comparing prices in the pre-disaster period with prices in the disaster period, which is the period under investigation. One is not only comparing the levels but also in periods. The determination of duration of the different competitive and anti-competitive periods and their placement may influence the detection of excessive pricing. The conduct of excessive pricing is more concerned with sustained behaviour as oppose to shorter fluctuations that normally occur in the market.[9] In section 2.2 of this paper it was indicated that the use of econometric methods is necessary in certain cases. It is important to note, however, that the econometric method will not adequately control for demand and supply if one is not working with the appropriate period.

[9] When the period is sufficiently long, this will allow for clarity with regard to the distinction between acceptable cases “involving a temporary upshot in price, which subsequently returns to the benchmark level” and those cases where there was a sustained price increase over a lengthy time period (Boshoff, 2020:20).

Normally, an appropriate competitive period is calculated on a case-by-case basis (Boshoff, 2020:10), but now the new regulations specify the particular time frame to use for the competitive benchmark: December 2019 to February 2020. This is odd, as the literature on this topic normally refrains from linking a benchmark to a specific time. To properly understand how prices respond to costs and demand, for example, would require a longer period. Features like seasonality can easily influence these relationships, especially over only a three-month period.[10] It is also necessary to consider longer periods retrospectively to see how preceding quarters or months changed the price (past responses) (Boshoff, 2020:11). For example, costs almost never have an immediate effect on prices. Dynamic models may account better for this. Giving a fixed competitive period for all cases would not accurately reflect the competitive period in all those cases. The authorities have been open to the suggestion that they consider a longer period in some of the most recent excessive pricing cases.[11]

In addition to it being necessary to consider a long enough period for a complete assessment, one also needs to choose a period with limited structural changes (Boshoff, 2020:10). Boshoff (2020:10) describes the period necessary for a benchmark as follows:

> A benchmark price is the average price over a competitive period of suitable duration, where such duration should reflect a balance between obtaining a thorough assessment of price setting and minimising the risk of structural or other factors contaminating the assessment.

Clearly the period chosen in each of the cases has a significant role in detecting excessive pricing. For example, if a firm had increased its prices in a sustainable fashion (over the three-month time frame) before the declaration of a state of disaster, comparing the averages over the two periods would not reflect an increased margin and would not show excessive pricing. Although it is not likely that the above scenario would have been anticipated by the firm, the use of a longer period as the competitive period might have reflected a more accurate estimation of its behaviour. Taking an average over a longer time period may account for uncertainties and existing demand and cost factors in the short period before March. Boshoff (2020:11) supports this argument in suggesting that it may be necessary to consider data even further back than the regulation-defined period, especially when faced with high elasticities in the specific case at hand.

[10] Normally higher demand around December and seasonal price discounts in January and February are followed by increases in demand that are not necessarily related to excessive pricing.
[11] Boshoff suggested rather using a comparative period that ends once there are observed "changes in competitive conditions; changes in demand and supply, associated with the disaster period" (Boshoff, 2020:11).

It is important to note that the appropriate length of the chosen period depends on the data available and the frequency of price data. For example, data reflecting a lower frequency will need a longer period, but even higher-frequency data may need a longer pre-disaster period, especially when "historically subdued demand and cost increases, seasonal fluctuations or other idiosyncratic price behaviour (including special discounts) characterise the market" (Boshoff, 2020:11).

The duration of the period is also important for calculating damages.[12] In cartel cases, for example, the estimated overcharge is multiplied by each time period when it was charged (Connor, 2014:252). Damages accrue over the period and the accuracy of its calculation therefore also depends on properly understanding the duration involved (Davis & Garcés, 2010:376). The placement of the period would also determine the different volumes purchased and many other factors that may influence the amount of the damages (Boswijk *et al.*, 2019:26). The dating of excessive pricing determines the exact period involved, which needs to represent the actual damages suffered. It is clear from the above that getting the appropriate dates of excessive pricing is important for detecting the conduct in the first place and then for calculating the damages caused.

3. Current policy approaches to determining the anti-competitive dates

Competition policy recognises that there may be a difference between formal liability dates and the effective dates of anti-competitive conduct. The first part of this section of the paper will analyse Boswijk *et al.* (2019), which draws extensively from The Practical Guide Quantifying Harm in Actions for Damages Based on Breaches of Article 101 or 102 of the Treaty on the Functioning of the European Union (2013) to guide the determination of cartel dates. The second part of this section will study abuse cases other than those based on excessive pricing, where the dating of anti-competitive effects was disputed. Lastly, this section will focus on excessive pricing cases where it will show how econometric methods may be helpful in determining appropriate dates. It will show that although dating is important for the more recent Covid-19 excessive pricing cases, it is also imperative for imminent damage claims.

[12] This paragraph draws heavily from previously conducted research (Bredenkamp, 2019).

3.1 European guidance of determining cartel dates

The collusion literature provides a novel framework to learn from with respect to dating anti-competitive effects. Traditionally, formal dates founded on the basis of documentary evidence were used as the start and end dates of cartels (Boswijk *et al.* 2019). Dussart-Lefret (2019), head of unit in the DG Comp established this position by stating that the start and end dates of a cartel are identified by the earliest date or latest date respectively for which there is a proof of cartel conduct, where such proof is found in documentary evidence such as minutes of meetings or phone calls, the exchange of emails or letters. Some policy instruments recognise some features relating to this issue of dating. The Practical Guide Quantifying Harm in Actions for Damages Based on Breaches of Article 101 or 102 of the Treaty on the Functioning of the European Union (2013:18) states the following:

> Some infringements start, or cease, gradually: and often doubt exists regarding the exact beginning of an infringement and, in particular, the effects it produces. Indeed, decisions of competition authorities regularly mention evidence suggesting that the infringement may have started earlier than the period established as the infringement period for the purposes of the decision. It is possible that a competition authority limits the finding of an infringement to a certain period, while in fact the infringement may have had a longer duration. Econometric analysis of observed data can be a way to identify when the infringement's effects started or ceased.

Even with the recognition given above, this failed to provide clear guidance as to the use of econometric methods in this regard. Therefore, in notable European cartel cases (for example the Vitamin and Lysine cartel case) the traditional method of relying on formal dates was applied.[13] The previous section of this paper has shown that these formal dates may differ from the actual effective dates.

Boswijk *et al.* (2019) draw on the European policy guide to find more appropriate start and end dates. They show mathematically that misdating cartel effects in this regard will lead to "a (weak) overestimation of but-for prices and an underestimation of overcharges irrespective of the type and size of the misdating" (Boswijk *et al.* 2019)[14]. The but-for price here simply means the price that

[13] See Bredenkamp (2019) for a discussion of these cases (Appendix A). Interestingly, in the Vitamin-cartel case, the parties admitted to their own staring point. This is worrying in the context of this study, as parties then have the power to drag their own overcharge average down by admitting, for example, to an earlier starting date.

[14] To explain these consequences in more depth as done in previously conducted research (Bredenkamp, 2019), the following details are listed: "overcharges can either be defined as the difference between actual prices and but-for prices or instead of actual prices rather the difference between predicted prices and these but-for prices (Boswijk *et al.* 2019). The total damage effect depends on how overcharge is calculated: it can be based on either predicted or actual anti-competitive prices, where the former proves to be unreliable when effect dates are unknown at first, which can lead to an over or under estimation (Boswijk *et al.*, 2019). Actual prices, however, always show a (weak) underestimation (the resulting damage estimator is conservative) (Boswijk *et al.*, 2019). While a longer alleged formal anti-competitive period in which more volume is sold to buyers, the but-for prices are weakly overestimated and therefore the volumes bought in

would have prevailed had the conduct not occurred (Boshoff, 2015). The "but for" methodology will be discussed in section 4 of this paper. The severe consequences of misdating found by Boswijk *et al.* (2019) support the claim that it is important to find appropriate anti-competitive dates.

Focussing on the *how* to determine appropriate anti-competitive dates, Boswijk *et al.* (2019) use econometric methods to mark the actual effective dates in a particular case. They use the European Sodium Chlorate cartel as an example to corroborate his findings. There are different econometric methods that may assist authorities in this matter of dating. The specific econometric method used by Boswijk *et al.* (2019) will be explained extensively in the methodology section, together with other possibilities. In short, Boswijk *et al.* (2019) extended a known comparison technique namely the before-during-and-after technique "with an empirical cartel dating procedure, which infers structural breaks of unknown number." The structural breaks identified in the data mark the corresponding dates where the effect was reflected in the market.

3.2 Dating of abuse cases other than excessive pricing

The dating of the anti-competitive effects caused by the abuse of dominance other than excessive pricing was primarily addressed in *Comair Limited v South African Airways (Pty) Ltd* [2017] 2 All SA 78 (GJ).[15] South African Airways (SAA) violated Section 8d(i) of the Competition Act 89 of 1998 (Republic of South Africa, 1998) as amended, which prohibits a firm from "requiring or inducing" an supplier/agent or customer not to deal with a competitor. The formal dates identified in this case were based on agreements made by SAA with travel agents,[16] to pay them considerable sums to deal only with SAA at the expense of its competitors (*Comair Limited v South African Airways (Pty) Ltd* [2017]).

If precedent had been followed, the formal dates identified would have prevailed. However, the economic experts involved considered different periods where the effects of the conduct may have

the inaccurate time periods are pre-multiplied by negative or zero overcharges (Boswijk *et al*., 2019). Boswijk *et al*. (2019) suggest the solution of structural break tests which they apply to the Sodium Chlorate cartel, and the case study corroborates the above findings".

[15] Note that all the information contained in the paragraphs that follow is obtained from *Comair Limited v South African Airways (Pty) Ltd* [2017] 2 All SA 78 (GJ) unless stipulated otherwise. For the sake of readability, this attribution will not be repeated after each and every sentence.

[16] Note that during the period of this conduct, one had to book a flight ticket through a travel agent. There were no easily accessible internet sales at the time of writing (Bredenkamp, 2019).

been present in the domestic airline market. Specifically, the end of the damage effective period was in dispute and they considered the possibility of lingering effects lasting in the market even after the end point of the damages.

Comair argued that although the existing agreements ended on 31 March 2005 (the formal end date stipulated by the Tribunal), the next "generation" of agreements was under draft[17] and was expected to take effect thereafter. Judge Matojane ruled in favour of this argument as it was supported by evidence showing that despite the existing agreements ending, most agents rationally expected there to be another set of agreements rewarding them for only selling SAA tickets, and therefore continued this induced behaviour for an additional four-month period up until 31 July 2005.

Following the establishment of an effective end point, the experts also disputed whether there were lingering effects lasting beyond this end point. Comair's argument was that customers had become accustomed to flying with SAA during the long period of abuse and would therefore not simply switch to "new competitors" after the abuse period. Comair supported this argument with three reasons: firstly, SAA had active benefit programmes incentivising customers to stay loyal to them; secondly, lengthy corporate agreements were in place (lasting beyond the formal end date) that would not have existed had it not been for the conduct; and thirdly, Comair's market share had not recovered overnight. Therefore, the court awarded another 12 months to account for these anti-competitive, lingering effects.

Thus, in consideration of the effects the formal end period of 31 March 2005 was amended by adding an additional 16 months (a 4-month extended end date plus 12 months to account for lingering effects). This additional period amounted to a finding of R450 million of damages additional to the existing damages caused, proving the underestimation thereof. SAA appealed against this decision, but the matter was settled. It is of the opinion that this case will form a benchmark (precedent) for future cases, which are likely to take a more effects-based approach when estimating period related damages.

When the authorities are faced with such challenging tasks, there is clearly a need for expert advice like the above, supported by economic analysis, in addition to traditional factual evidence, to assist

[17] The content of these agreements depended on the tribunal's decision.

them in their assessment. What follows naturally in this study is to use the data from the above case to test the experts' findings and support the use of effective dates. However, Harman (2020), the expert on the case, confirmed that this is not possible and listed the reasons therefore. Despite the confidentiality issues, Harman listed additional reasons which give insight into the limitations of econometric applications and will be discussed. They were: "SAA's market share and price data were not robust";[18] there was a shortage of data prior to the infringement; there were concerns as to whether "SAA continued to operate illegal commission structures" post infringement; "during the relevant periods, there were a number of airlines exiting the market, and the introduction of low cost carriers" which may have been partly responsible for reducing Comair's market share; the transitioning "away from travel agents to online bookings", the recession over the period[19] and the "high inflation, oil price changes", etc. had a significant impact on prices; various loyalty schemes were in operation but there was no evidence; no data to support SAA's claim that its "demand increased due to government policy"; prices were influenced by other factors as well, such as the fact that "its planes flew more routes at higher frequency, and were less full, its pricing model was flawed"; pricing in airlines is a complex matter in general, being based on "complex pricing algorithms which monitor demand over time"; quality also changed overtime; SAA focussed more on the back of the plane, "whilst Comair focussed on the front of the plane"; the prices available "reflected a simple average of different ticket types, so it was difficult to understand if a change in price reflected a mix change"; the airlines changed their "frequent flyer and staff flying policies over time"; they changed their "fleet over time (more fuel efficient planes)"; et cetera.

Thus, the application of econometrics to this case would not be possible due to there being insufficient data to model the demand and prices in the counterfactual scenario. Therefore, another case is chosen to do an empirical application of, and although it presents its own challenges, these are not as daunting as those in *SAA*. This study recognises the possible limitations and argues that where applicable, econometric application may assist authorities in identifying proper effective dates.

[18] According to Harman (2020) new data was being provided by SAA even during the trial, that fundamentally changed the estimate of damages.
[19] Recessions impact "business class and the back of the plane differently" (Harman, 2020).

3.3 Dating of excessive pricing cases

Two key South African cases caused significant debates around the standards of determining excessive pricing. These were *Mittal Steel v Harmony Gold/Durban Roodepoort Deep* 70/CAC/Apr07 (the *Mittal Steel* case) and *Sasol Chemical Industries v The Competition Commission* 48/CR/Aug10 (the *Sasol* case).

Mittal, a state supported company, was a near monopolist in the steel market (*Mittal Steel v Harmony Gold/Durban Roodepoort Deep* 70/CAC/Apr07). A complaint of excessive pricing was filed against Mittal relating to its flat steel products, and its import pricing parity was proof that its prices were indeed excessive – the Tribunal found Mittal guilty (Theron, 2019). Mittal also prevented its customers from reselling locally and the Tribunal prohibited this contractual term (*Mittal Steel v Harmony Gold/Durban Roodepoort Deep* 70/CAC/Apr07). However, the Competition Appeal Court found the tribunal's approach to be fundamentally flawed as it was based only on a structural point of view (involving only looking at Mittal's market share) and sent the case back to the Tribunal (Theron, 2019). The matter was settled before the trial (Theron, 2019).

In the Sasol case there was a lot of emphasis on Sasol's history as a state supported firm (*Sasol Chemical Industries v The Competition Commission* 48/CR/Aug10). The prices of "purified propylene and polypropylene" were in question and the Tribunal found them to be excessive on the basis that the prices bore no "reasonable relation" to the products' economic value, and applied behavioural remedies (*Sasol Chemical Industries v The Competition Commission* 48/CR/Aug10). Again, the Competition Appeal Court reversed this decision and a settlement followed. In both cases it is important to note that entry into these markets is not easily possible and having state support gives rise to lower costs – making it harder to determine excessive pricing (Theron, 2019). However, the issue of dating is less important in these cases (only cases before Covid-19). As explained in section 2.1, these cases used a cost-based benchmark because they lasted for many years with no clear break of a non-competition and competition period, and therefore a benchmark like an intertemporal benchmark requiring a break would be appropriate (Boshoff, 2020:7).

Although the above cases provided the first examples of South Africa's position on excessive pricing, more recent developments in the context of Covid-19 have come to light where the issue of dating is especially relevant. As already said, the most prominent problem in these cases is using the fixed 3-

month period prior to March as a competitive benchmark. In the next paragraph cases dealing with conduct that happened just before March 2020 will be discussed.

The first Covid-19-related case in South Africa was the *Competition Commission v Babelegi Workwear Overall Manufacturers and Industrial Supplies CC* CR003Apr20. Babelegi sold dust face-masks to customers from 31 Jan 2020 to 5 March 2020 (the complaint period) and effected "several price increases (before the actual increase in its supplier costs on 18 March 2020)" (Competition Tribunal, 2020). The first issue before the court was whether Babelegi was indeed dominant. The court relied on the abnormal circumstances of Covid-19 and made the highly controversial decision that a small firm like Babelegi was indeed "temporally" dominant. As one can see, dominance may also depend on the specific periods in consideration, but this is not the focus of this study. Secondly, the commission had to show that Babelegi had abused its dominance through charging an excessive price. Following the Act, a competition price had to be determined against which to measure Babelegi's current prices. The parties agreed that the pre-Covid price might be used as such a competitive price. Thus, when these prices were compared, the Competition Tribunal (2020) found a significant price increase that did not relate to cost increases which accordingly constituted excessive pricing. In other words, the prices charged "bear no reasonable relation to the prices charged and mark-ups prior the Complaint Period as the appropriate and sensible benchmark of what competitive prices and mark-ups would be under conditions of normal and effective competition" (Competition Tribunal, 2020). The Competition Commission phrased it as follows: Babelegi is taking advantage of increased demand by charging an excessive price (*Competition Commission v Babelegi Workwear Overall Manufacturers and Industrial Supplies CC* CR003Apr20). [20]

Regardless of the critique of the establishment of dominance in this case, the most significant critique for the purpose of this study is that discussed in section 2.4: a fixed short benchmark period may be subjected to many other factors disrupting its competitiveness. In fact, in the Babelegi case, a comparator period of less than 2 months was used (9 December 2019 to 31 January 2020). Nevertheless, it is also of the opinion that the pre-Covid period was not the appropriate competition period to compare with, as it did not present to similar demand and cost conditions, which is the usual requirement of an appropriate intertemporal benchmark (Sutherland, 2020). In the normal flow

[20] As noted before, an interesting dispute here was that the complaint period preceded the date of publication of the Consumer Protection Regulations (19 March 2020) and these regulations could therefore not be applied (*Competition Commission v Babelegi Workwear Overall Manufacturers and Industrial Supplies* CC CR003Apr20).

of a market, when supply is consistent and demand increases, a firm charging a price which tracks the demand is not in the wrong. A price response to increased demand cannot be an excessive price. Sutherland (2020) is of the opinion that a more appropriate competition price would fall within the Covid-19 period (a different period for the competition benchmark) to allow for similar conditions in the two poles of comparison and thus take the increased demand into account. Econometric methods are necessary to properly account for the changes in cost and demand, although the short time frames of the Covid-19 period would limit the amount of data available.

Other opinions state that the court should have considered more than simply “accounting costs”: the firm suffered the fear of future cost increases and a possible disruption in supply (Sutherland, 2020). As a smaller firm, it was not necessarily unreasonable for it to respond with price increases. Many other similar firms behaved similarly, but the tribunal dismissed this consideration too easily, by stating that they all practised the same exploitation. Econometric methods may show differently: that the price increases reflect higher demand that was not necessarily balanced by an increase in supply.

It is acknowledged that the tribunal was under pressure from the public and political forces in the Babelegi case, but this case created a chain effect and many other cases followed, based on a relatively system of determining excessive pricing (Sutherland, 2020). The authorities should be very careful in this regard. It is uncertain what type of benchmark would be used post Covid-19, but if the intertemporal benchmark prevails as the main excessive pricing determination method, the time frames of the different periods used to as poles of comparison are of concern. It is clear that the use of more sophisticated (econometric) methods is necessary to determine appropriate comparable periods, not only for these and similar Covid-19 cases, but also for future excessive pricing damage claims.

The importance of the use of econometric methods in such competition cases is supported by the work of Rubinfeld (1985). Even in earlier years he found an increasing acceptance of econometric methods in the advisory system, which according to him “opened the door to law-related econometric studies, particularly in connection with the use of multiple regression models” (Rubinfeld, 1985:1094). He acknowledged the necessity for this trend to continue and to evolve – exactly what this study is trying to achieve in a specific context. Rubinfeld (1985:1094) shows how valuable econometric techniques can be, especially in cases involving “complex, essentially empirical issues”, but he also recognises the possible misuses and the problem of standard statistical procedures which may be “subject to manipulation when applied in a courtroom setting.” He identified two problems as follows:

> The potential problems are of two distinct types. First is a tendency to interpret regression results as being more accurate than they are. Regression results may appear deceptively accurate because the underlying assumptions are seldom made explicit. A second problem is the experts' tendency to analyze data and report in a manner which biases the results for the particular side that they represent. This tendency could reflect an intent to mislead or deceive, but it need not. More likely, it reflects economists' standard practice of searching among alternative models, trying numerous functional forms and variables to find the ‘best fit,’ and reporting only the final outcome of the search process (Rubinfeld, 1985:1095).

Instead of giving rigid guidelines, his article intended to motivate researchers from both the legal and economic disciplines to develop econometric methods that would be “practical and suitable for a statistical litigation context.” The suggested solution is to consider the appointment of a “neutral” expert who is not acting for one side or the other, and motivating experts to provide all results and reasons, not just the “best fit” (Rubinfeld, 1985:1096). Following this advice, the author of this study attempts to adopt an unbiased approach to the case study that follows, and portrays all possible results to test the robustness of the econometric methods used.

4. Methodology

To understand the econometrics necessary to assist authorities with the issue of dating, this section will analyse four subdivisions: the ‘but for’ methodology; structural break tests; regime-switching modelling; and finally the advantages and disadvantages of the multiple methods.

4.1 The ‘but for’ methodology

The ‘but for’ methodology will be analysed by using price regression models with a focus on the dummy-variable techniques (although some attention will also be given to the forecasting method) followed by an explanation of the traditional reliance on fixed dummies.

As explained before, one of the more challenging difficulties with anti-competitive cases is not only the need to detect anti-competitive behaviour but also the need to estimate the appropriate damages that the conduct caused. In both collusive and excessive pricing cases, one essentially needs to calculate the “overcharge” or “excess price” asked by the anti-competitive firms/firm to determine the damage caused. To calculate such overcharge, one needs to take the difference between the prices charged in the period under scrutiny with the prices that would have prevailed had the conduct not occurred (this counterfactual price is called the "but-for" price) (Boshoff, 2015:222). Such a comparison should be cautious of other contributing factors. Boshoff, Bun, Schinkel and Van Jaarsveld (2019:2) put it as follows:

> A simple comparison, perhaps of the cartel price and pre-cartel price (as one example of a "but for" price) may run into serious challenges: demand and cost developments during the cartel period may also influence the price, apart from the decision to collude. The standard approach to estimating overcharge is therefore to develop a regression model in which price is determined by a range of relevant demand and cost drivers.

Boshoff (2015:222) refers to this problem as the identification problem: the modeller must be sure to isolate the effect of the anti-competitive action, for example a cartel, from these other effects caused by demand and cost conditions. The dominant method in practice of solving this problem and determining the but-for price is the comparative method, as mentioned in section 2.1 of this paper, where prices during the conduct period are compared with prices from a similar, comparable market which are not characterised by anti-competitive activity, or compared with the same market but for a different period when the market was competitive (Hüschelrath, 2013). The latter method, in which the same market is used but in different periods (the period under investigation is compared with a period "not characterised by the allegedly collusive conduct") is called the temporal approach. It is nested within the comparative approach previously discussed, and is the most commonly used (Boshoff, 2015:222). Temporal approaches in cartel literature relate strongly to intertemporal benchmarking, as discussed in excessive pricing cases. The temporal approach selects a period "either before or after the cartel" and uses this information to estimate the but-for prices involved during the cartel conduct period. In the same way, in excessive pricing cases the period under investigation is compared to the period before Covid-19.

There are several possible methods to apply the temporal approach (Boshoff, 2015:222). The simplest of these is the "before-and-after" method. The name is self-explanatory. Prices in the cartel period are simply compared with prices in non-cartel periods (Boshoff, 2015:222). In cartel literature it is known that when the before-and-after method is used it is very difficult to link the anti-competitive conduct solely to the change in price as one is not controlling for possible demand and supply shocks (White *et al.*, 2006). The literature tends to lean towards a multivariate price model to find coefficient estimates which can be applied to data in the anti-competitive period to predict but-for prices via forecasting or backcasting (Paha, 2010). Unfortunately, the temporal approach is vulnerable to structural changes that occur over time (Boshoff, 2015:222). Again, one becomes aware of the need for sufficient data. Boshoff (2015:222) confirms that comparing periods in this manner requires sufficient data in both comparable periods, and this may lead to lengthy data usage over long time periods which are susceptible to structural changes. (It is likely that cost, demand, and price relationships will have changed.)

As for the comparator benchmarking as discussed above, in cartel literature this idea is called the spatial approach. It is also embedded in the comparator-based approach (Boshoff, 2015:222). In the same way, similar markets in different regions can provide information for determining but-for prices (Rubinfeld, 2012). The spatial approach can take many forms[21] but one is still faced with the challenges of finding an appropriate comparator market and data constraints regarding controlling for cost and demand in every market. As already discussed, comparative benchmarking requires the same cost and demand conditions. The spatial approach also involves this assumption of similar demand and cost conditions, as discussed before. According to Boshoff (2015:224), this assumption is reasonable when there are comparator products available that are "closely related" to the product involved in the anti-competitive behaviour. If not, then econometric methods controlling for the demand and cost differences may be necessary. This study will use this approach in the next section, where the export prices of Foskor's phosphoric acid will be used as a comparator to predict the local but-for price, therefore assuming that the export and domestic prices share similar demand and supply drivers.

The econometric but-for methodology will now be explained through a standard price regression model similar to that used by Boshoff and Van Jaarsveld (2019:4).

The different approaches described above may be adopted to estimate the following long-run relationship between the price of the relevant product or service and its drivers:

$$p_{it} = c_0 + d_t\delta + \sum_{l=1}^{m} \alpha p_{t-l} + \sum_{l=0}^{n} \psi \mathbb{x}_{t-l} + \epsilon_t \quad (1)$$

where p_{it} is the price of the relevant product or service i at that time period t; d_t is a dummy variable which allows for different periods by changing the intercept: the anti-competitive period that takes the value of 1 when abuse or collusion is present, and 0 if such anti-competitiveness is not present (0 if in the competitive period); price is lagged, which adds a dynamic element to account for possible serial correlation (information criteria will indicate the optimal lag length); $\mathbb{x}_{t-l}$ represents a vector

[21] Boshoff (2015:223) describes some of these forms as follows: "One such a spatial approach is the yardstick method, which involves a direct comparison of prices in different regions. A more appropriate spatial method, which also incorporates temporal features, is a difference-in-difference (DID) analysis. DID analysis involves a comparison of the differences in prices between the market under investigation and the comparator market(s) before and during the cartel".

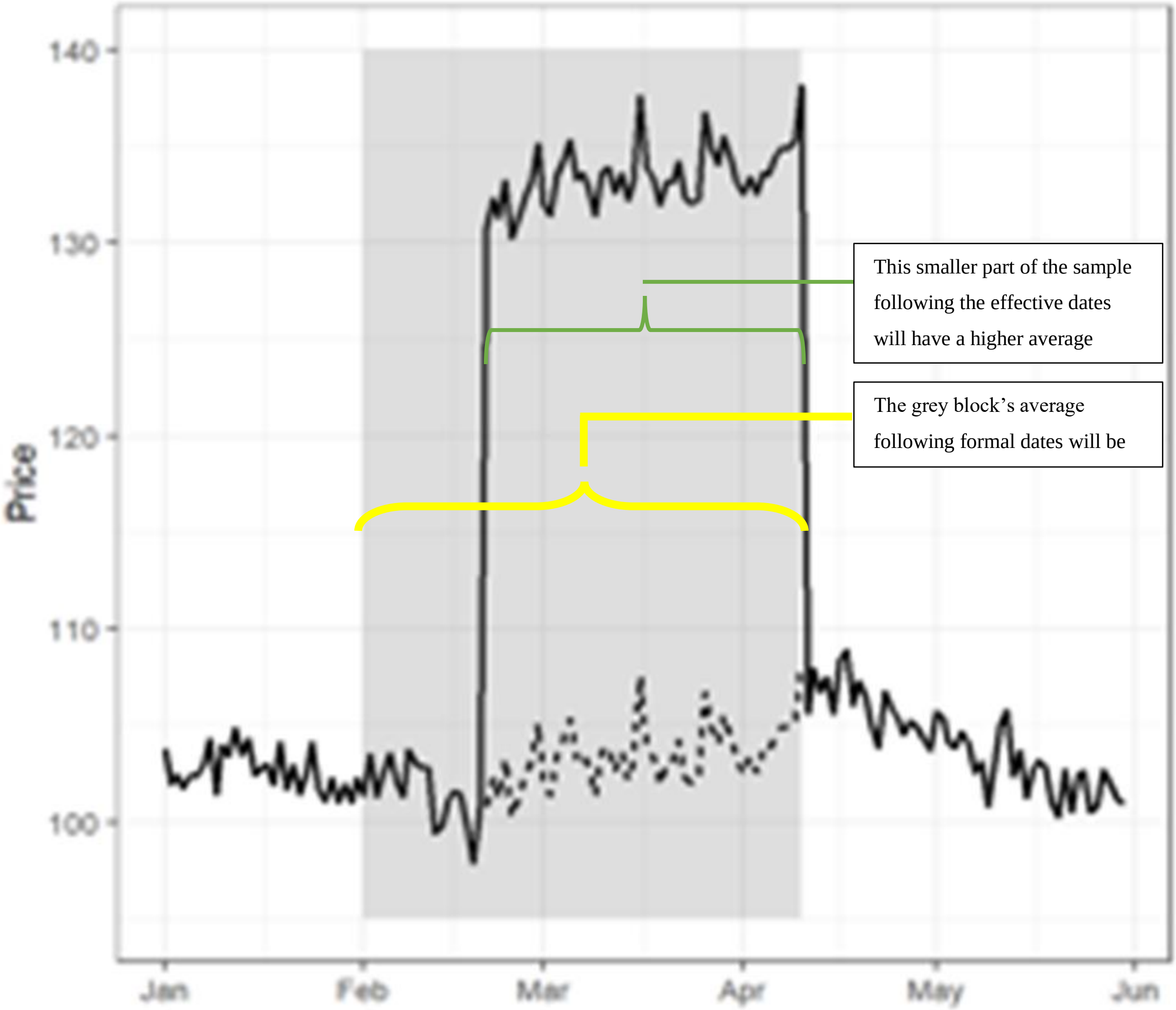
of price drivers – which may include cost and demand drivers, or import and export prices depending on the approach used; where the error is normal: $\epsilon_t \sim N(0, \sigma^2)$. Intuitively, this is a normal regression where price is explained by its expected drivers. However, the dummy-variable approach adds this d_t (dummy variable). Thus, if this dummy variable equals zero (in the instances where no anti-competitive behaviour is present) then this newly added term would disappear, and one would have a normal regression. But if this dummy variable equals 1 (during abuse/collusion) it would be multiplied by the parameter (δ) and the whole price regression would switch upwards or downwards with δ reflecting the higher or lower price respectively (Van Jaarsveld, 2019). Boswijk *et al.* (2019:28) describe it as follows: the dummy variable's parameter "quantifies the cartel occurrence as a price shift". To interpret a parameter economically, it would answer the question of how much the dependant variable changes when the independent variable changes. Thus, δ shows how much prices change when d_t changes from 0 to 1, or *vice versa*. Therefore, the overcharge or excessive price is reflected by this parameter (δ) of the dummy variable (Van Jaarsveld, 2019). The statistical significance of this parameter (δ) is very important when calculating overcharge/excessive pricing, as it is the answer of how much of overcharge exists (Van Jaarsveld, 2019). The δ parameter is then used further to calculate overall damages (Van Jaarsveld, 2019). Looking at the parameter once would simply mean the overcharge of one product or service at one point of time; it does not reflect all the damages. The following formula may be used to calculate all the relative damages:

$$Damages = \delta \sum_{t=1}^{T} D_t Q_t \qquad (2)$$

Basically, the overcharge parameter (when present) is multiplied by the "quantity sold" (Van Jaarsveld, 2019). Or in other words the estimated overcharge is multiplied by each time period it was charged for each quantity being charged for (Connor, 2014:252). The no-collusion or no-abuse period will not be summed over.

The reason why dating is such a crucial aspect here is because the above parameter (δ) estimated by the regression is a sort a of "average" estimation. The regression estimates the average overcharge or excessive price throughout the whole abuse/collusion period (Van Jaarsveld, 2019). The overcharge parameter is a fixed amount through all the abuse/collusion time period, and not dynamic. This per se is not problematic, but if the dates or periods of competition and anti-competition are wrong (the dummy is placed at the wrong point in time) it could underestimate the damages caused, as shown in figure 1:

Figure 1: Outcomes of misdating anti-competitive effects



Source: (Van Jaarsveld, 2019)

Figure 1 considers the first of the two likely cases as identified in the literature.[22] First, if a formal date earlier than the actual effective date is used (February), it includes lower competitive prices (from February up until 18 March) in this collusion/abuse sample over which the average is taken – dragging the average overcharge down. If only the prices over the effective dates were used (18

[22] Section 2.4.

March up until 10 April) it would show a much higher average overcharge. This is dependent on a case-by-case basis and the product can go either way.

The same logic follows for the second likely scenario, which occurs at the end. If a formal end date earlier than the actual effective end date is used, one essentially misses out on the remaining higher overcharges/excessive prices not included in the sample, also dragging the overcharge down. This intuition is exactly what Boswijk *et al.* (2019) proves mathematically, as described in the literature review. He shows that it normally leads to an underestimation of damages. Therefore, instead of the dates of the different periods being determined outside the model as in the traditional practice, it might be advantageous to supplement this fixed dummy with tests that identify the effective dates, as is done in the next section.

Lastly it is important to note what type of regression and model specifications would be deemed most appropriate. The Overcharge literature explains using the OLS (Ordinary Least Squares) estimation for the above model in (1). OLS assumes the exogeneity of the independent variables, but in this case OLS may lead to inconsistent results, as the variables might be correlated with the error term (Du Rand, 2020). The problem with OLS is that it assumes a standard relationship without any structural changes (like anti-competitive behaviour) in relationships between a price and its cost and demand drivers (the ψ parameter) (Boshoff & Van Jaarsveld, 2019:5). In most cases where anti-competitive behaviour was present, the variables will not be constant (non-stationarity will most likely be present) (Du Rand, 2020). Auto-regressive distributed lag models (ARDL) may then perform better in this case. ARDL can accommodate non-stationary variables and a mixture of stationary and non-stationary variables (Shrestha & Bhatta, 2018:79). Time series data (especially pricing data) are likely to have "some kind of relationship with [their] previous values" and controlling for such previous values (together with an adjustment factor) in the independent and dependant variables may be worthwhile (Shrestha & Bhatta, 2018:72). Section 5 of this paper will test different numbers of lagging variables based on information criteria tests and the significance of the lags.

As opposed to the dummy variable approach, one may also use forecasting to determine the but-for price with the estimates provided by (1), where data outside of the anti-competitive period are used to estimate the but-for price, provided that there are efficient data available pre and post the anti-competitive periods (Boswijk *et al.*, 2019:28). For example, one may fit the above models on data from similar markets/countries during the anti-competitive period and use such a fitted equation to forecast the relevant local but-for price needed to get overcharge (Boshoff, 2015:10). However, this method is also subject to hefty critiques, especially when trends are present. The focus of this study is on the dummy variable approach described above, where price data from the anti-competitive periods are also used.

4.2 Structural break tests

To address the issue of determining appropriate dates, Boswijk *et al.* (2019:36) propose to supplement the above before-during-after method with a multiple structural breakpoint test to capture the changes in price regression relationships due to the anti-competitive behaviour (Boswijk *et al.*, 2019:36). Boshoff and Van Jaarsveld (2019:359) describe the test as follows:

> …an unexpected change in a time-series variable that can change the mean or parameters of the underlying statistical process generating the data. Structural break dates may signal the start or end of a collusive agreement (Boshoff *et al*., 2018:359).

In an excessive pricing case, for example, a structural change occurs when prices are raised but the drivers remain constant, as explained in the second section of this paper. This links the price increase directly to the change in competition behaviour, implying a causal effect. Such a structural change may be identified by a structural break test, endogenously identifying the place in time where anti-competitive effects were present, and allows for more appropriate dummy placement.

The first structural break test was developed by Gregory Chow in 1960: the so-called "Chow breakpoint test", which is fitted for a single, *exogenously known* breakpoint in time[23] (Chow, 1960). But the whole point of this study is that one does not necessarily know for certain where the break occurred and one wants to determine it endogenously. Andrews (1993) extended this Chow breakpoint test by allowing for the detection of a single unknown point in time where the break occurred (in other words, where the data generating process changed). The process is described as follows: this model is first estimated without the break, after which the sum of squared residuals (SSR) is stored; then the model is estimated for each "possible date of the break" (but 15% of the

[23] Intuitively, if one has a sample of T time periods and one knows a break in the data generating process occurred at some point in time (t_m), the Chow test will first estimate (1) with all the data, and then store the sum of squared residuals (SSR_0), then it will estimate the model (1) with only the data up to the break point t_m: thus t = [1, t_m], and again store the sum of squared residuals (SSR_1), and finally it will estimate the model in (1) by using only the data after the break point in time t_m: thus t = [t_m+1, tT] and store the residuals as (SSR_2) (Du Rand, 2020). These saved residuals can then be used in the F-statistic to test the significance of the break (Du Rand, 2020).

first and last points in time are excluded to allow for the trimming parameter), whereafter each of these unique residuals is stored. The model with the lowest SSR is deemed most significant, and will indicate the related point in time if applicable (Du Rand, 2020). This test is limited to one break in time. It may be the case that anti-competitive behaviour started (one break) and stopped (another break) within the available sample, or even switched between regimes in the middle. The next paragraph will explain the test for multiple structural breaks and the section after that considers regime switching.

Based on very much the same logic as that described above, the generally accepted test for multiple structural breaks is that proposed by Bai and Perron (1998). Boswijk *et al.* (2019:36) confirm the above reasoning by explaining that the Bai and Perron test also seeks to identify the points in time that best fit the relevant regression by "trying many different divisions of the time-series, each with different candidate dates to switch the dummy variables." Borrowed from the author's previous research,[24] the following paragraphs explains this test methodology in depth.

The original Bai and Perron (1998) paper explains how the test works. The following is a summary of their methodology[25], which can be seen as consisting of two parts. The first part entails identifying the number of breaks in the series, irrespective of their statistical significance. Then the significance of each break can be tested, through a series of statistical operations making use of asymptotic values. When dealing with smaller series the methodology can yield significant size and power deviations. The second part emphasises a finite-sample complication when significance is tested for a set of breaks. Normally the F-ratio is used, which compares the SSR associated with the restricted model with the SSR associated with the unrestricted model. It is important to note that these breaks are found via a global minimisation process, and therefore there may be instances when a "set of t breaks" is not necessarily a "subset of t +1 breaks" (Bai & Perron, 1998). If this is the case, then the hypothesis of t +1 breaks is not nesting in the hypothesis of only t breaks, and the "1 / t t SSR SSR + ratio does not have the property of asymptotic convergence to the F-distribution" (Bai & Perron, 1998) The "asymptotic distribution depends on sample-specific parameters", for example on the size of the break (Bai & Perron, 1998). Bai and Perron (1998) propose always testing for the "presence of one break versus 0 breaks in the segments between breaks" to avoid this problem. One should be careful, however, especially when one deals with smaller time series, as they involve even smaller

[24] Bredenkamp. A.C. 2019. *Dating of anti-competitive effects in South African competition policy.*
[25] The following paragraphs are heavily drawn from Bai and Perron (1998). Their source will not be cited after each sentence for the sake of readability.

segments and only a few observations determine the result. The Bai-Perron test can accommodate features like autocorrelation[26] and heteroscedasticity, which is a vital capacity, as these statistics use asymptotic critical values (provided in tables at the main confidence levels) to conduct a Wiener process.[27] Bai and Perron propose that a bigger segment size relative to the sample should be used when one is faced with a smaller data series.

Bai-Perron's version of (1) would look as follows:

$$p_{it} = z_t'\delta_j + \sum_{l=1}^{m} \alpha p_{t-l} + \sum_{l=0}^{n} \psi \mathbb{x}_{t-l} + \epsilon_t \qquad (3)$$

where j =1,...,m+1, where m constitutes the number of structural breaks, p_{it} is the observed price (the dependent variable as described above), $\mathbb{x}_{t-l}$ and z_t are the vectors of covariates, ψ and δ_j are the corresponding coefficients (vectors), and ϵ_t is the disturbance term. The regression coefficients can be jointly estimated through the middle terms ($\sum_{l=1}^{m} \alpha p_{t-l} + \sum_{l=0}^{n} \psi \mathbb{x}_{t-l}$), while structural changes are captured through the first term. This equation also shows a "partial structural model" since the parameter ψ is not "subject to shifts and is estimated using the entire sample" (Bai & Perron, 1998). If the middle terms were dropped then it would be "a pure structural change model, where all coefficients are subject to change" (Bai & Perron, 1998). The ϵ_t can be non-independent and identically distributed under the null hypothesis.

Bai and Perron (1998) explain two possible ways to approach breaks. The first is called the global minimisation approach, where each break date (partition m) is identified as one which minimises the SSR (the sum of the squared residuals). This approach is advantageous as it identifies only the biggest breaks (the largest fall in SSR), or at least asymptotically. The cost, however, may be that not all the "biggest n breaks" may be included in the "biggest n + 1 breaks". Another approach is to find the breaks sequentially by starting with one that minimises the SSR, followed by getting the single break which minimises the SSR for each "resulting partition'. Of the two, the second break will be that with the minimum SSR regardless of the significance, as these significance tests can be done separately. The hypothesis will depend on whether the number of dates is known - for example, the null hypothesis of no breaks versus the alternative of a known number of breaks - but this number is

[26] The Bai-Perron test deals with "autocorrelation in a non-parametric fashion" and proposes to "correct the time series residuals either through a Newey-West procedure or by including the lag of the time series as one of the regressors in the projection model" (Antoshin, Berg & Souto, 2008).

[27] It is suggested, however, that such corrections must be made only when "there is a strong prior that the correction is necessary" (Antoshin, Berg & Souto, 2008).

not usually known. One can detect the "number of breaks" by estimating a sequence of supremum F statistics: first testing for a single break and then for *l* +1 via the F(*l* +| *l*) ratio and ending the process where the null hypothesis is not rejected. One should be aware that this test may "incorrectly estimate the number of significant breaks", especially in situations where there are up and down switching regimes and a lot of breaks, but according to Bai and Perron (1998) this problem may be reduced by using the "Dmax" statistic.

4.3 Regime-switching modelling

Another problematic aspect often ignored in standard models is that it does not account for the pace and duration of transition periods between competitive and non-competitive periods (Boshoff & Van Jaarsveld, 2019:5). A Markov-Switching model may pose a solution to many of the insufficiencies of standard models. Boshoff and Van Jaarsveld proposed the following price model (Boshoff & Van Jaarsveld, 2019:5):

$$P_{it} = \begin{cases} c_0 + d_t\delta + \sum_{l=1}^{m} \alpha p_{t-l} + \sum_{l=0}^{n} \psi \mathbb{x}_{t-l} + \epsilon_t\,, & S_t = 1\ (collusion) \\ c_0 + \sum_{l=1}^{m} \alpha p_{t-l} + \sum_{l=0}^{n} \psi \mathbb{x}_{t-l} + \epsilon_t\,, & S_t = 2\ (no\ collusion) \end{cases} \qquad (4)$$

This model is written consistently with the notation in this study as described above, where S_t represents the operational regime at time period *t*, but this time without any *a priori* assumptions: S_t will be treated as model-determined instead of determining it exogenously as in the standard model (Boshoff & Van Jaarsveld, 2019:5). Thus, two separate data-generating processes for the different regimes will run, based solely on "an assessment of the probability of being in a particular regime" (Boshoff & Van Jaarsveld, 2019:5). The changes in the regime will be reflected in the intercept, and we are therefore assuming that such changes are reflected in price level shifts, although the specification of the model can also allow for the other parameters in the model to be regime dependent (Boshoff & Van Jaarsveld, 2019:6). We furthermore assume that the probability law used to determine S_t follows a two-regime, first-order, Markov-chain following the constant transition matrix as described below (Boshoff & Van Jaarsveld, 2019:6):

$$\xi = \begin{bmatrix} \xi(S_t = 1|S_{t-1} = 1) & \xi(S_t = 2|S_{t-1} = 1) \\ \xi(S_t = 1|S_{t-1} = 2) & \xi(S_t = 2|S_{t-1} = 2) \end{bmatrix} = \begin{bmatrix} \xi_{11} & \xi_{12} \\ \xi_{21} & \xi_{22} \end{bmatrix} \qquad (5)$$

where $\xi(S_t = j|S_{t-1} = i) = \xi_{ij}$ describes the possibility of switching from any regime *i* at the previous time period to regime *j* in the next time period (Boshoff & Van Jaarsveld, 2019:5). The probability law assumption here is valid in a unit root process (having strong persistence) (Boshoff & Van Jaarsveld, 2019:6). This is indeed the case for Foskor's price data on phosphoric acid.

This methodology is based on the work of Hamilton (1989) and Kim (1994), where they present a recursive, likelihood-based approach to get estimations for the filtered probability of a model being in a particular regime, where this probability is described as follows (Boshoff & Van Jaarsveld, 2019:6):

$$\xi(S_t = i|\Omega_T;\theta) \tag{6}$$

Boshoff and Van Jaarsveld (2019:5) used this probability estimation to date the relevant regimes accordingly, and to measure what they called "cartel effectiveness", which reflects the speed of transitioning between the different regimes (Boshoff & Van Jaarsveld, 2019:6). What then follows is to estimate overcharge according to the identified anti-competitive periods by replacing the standard model's unique intercept (because of the presence of collusion: $d_t = 1$) by the estimated probability as follows (Boshoff & Van Jaarsveld, 2019:6):

$$p_{it} = \beta a_{i,t} + \sum_{l=1}^{m} \alpha p_{t-l} + \sum_{l=0}^{n} \psi \mathbb{x}_{t-l} + \epsilon_t \tag{7}$$

where β provides the overcharge percentage and $a_{i,t}$ equal to the probability in (5), shows the cartel effectiveness estimate providing an objective identification of the precise dates of the applicable regimes while accounting for the speed and lengths of transiting between them (Boshoff & Van Jaarsveld, 2019:6).

When it comes to regime-switching models - as another possible method to assist in dating anti-competitive effects - many types of specification choices must be made. This specifically includes the decision on which variables should be regime switching. According to Boshoff and Van Jaarsveld (2018) this is the first step in a Markov regime-switching model: "decide which parameters should be regime-dependent". When one follows the standard approach in the literature, only the intercept is varied by regimes, as shown in (4) (Boshoff and Van Jaarsveld, 2018). This is essentially then the same concept as that used in (1) (the dummy-variable method) as the dummy for overcharge is simply an additional intercept (Boshoff and Van Jaarsveld, 2018). However, as mentioned above, the Markov regime-switching model allows for the other parameters and the covariance matrix to switch as well. Many macroeconomists have used this Markov switching regime models in the same way. Haltiwanger and Harrington (1991) studied firms' incentives to collude during recessions and booms

by looking at price responses to demand shocks. Thus, in their context it was relevant for both the intercept and parameters to differ across the regimes (Boshoff and Van Jaarsveld, 2018).
An application to Foskor's excessive pricing case is implemented and all the different switching decisions are reported in section 5.4.

4.4 Multiple methods

There is some resemblance between the structural break and the Markov-switching models, and both may be helpful in assisting competition authorities to detect and calculate anti-competitive effects by providing supportive evidence for the appropriate dates involved in a specific case. The structural break methodology as a supplement to the dummy variable approach may be imperative when determining specifically the start and end dates of anti-competitive effects, without focussing on the transitions (Boshoff and Van Jaarsveld, 2019:7). Breakpoint tests merely shift from one regime to the other "as sudden deterministic events" (Boshoff and Van Jaarsveld, 2019:7). The regime-switching methodology, on the other hand, may also assist in understanding the effectiveness of anti-competitive behaviour during the transition periods between regimes, and is more fitting than breakpoint tests in markets facing recurrent anti-competitive behaviour with multiple episodes (Boshoff and Van Jaarsveld, 2019:7). It is important to consider the results of both the structural tests and the regime-switching models when determining the dating of anti-competitive effects, as both may provide supporting evidence in the relevant context.

Although advantageous, it is clear that such econometric methods are not a silver bullet applicable to every case in the same way, but may be of significance to authorities in appropriate cases, especially when dealing with highly complex matters. Even so, the above methodologies are also subject to limitations. The biggest limitation of the breakpoint tests is that it is likely to miss shorter intervals of regime changes (Boshoff and Van Jaarsveld, 2019:7). The Bai–Perron test, as said before, requires the specification of a "trimming parameter". This parameter determines the "minimum distance" between the structural breaks available for identification (Boshoff and Van Jaarsveld, 2019:7). Boshoff and Van Jaarsveld (2019:7) explain that the problem occurs when there are not enough data. If the sample size is not "sufficiently large, a trimming parameter as small as 5% of the total sample size can lead to imprecise test results." Thus, when multiple breakpoints are closer to each other than the specified trimming parameter, then this test will fail to detect both points as breaks (Boshoff and Van Jaarsveld, 2019:7). This is a concern for more recent excessive pricing cases as they are characterised by short intervals. Using a mere fixed three-month period before a possible structural break detection might result in the test's not detecting the breaks, and result in insufficient data to draw samples from.

Regime switching models also face limitations where firms consider future conditions when deciding whether to engage in anti-competitive behaviour (Boshoff and Van Jaarsveld, 2019:8). Boshoff and Van Jaarsveld (2019:8) describe this in the context of a cartel and recognise that empirical models must then be estimated from future data. Their transition probabilities are, however, based on the "entire history of prices, demand, and cost factors" (Boshoff and Van Jaarsveld, 2019:8). They defend their approach as follows:

> these probabilities are merely the initial (or prior) probabilities, and they are updated each time period, using a Bayesian approach, to obtain the filtered probabilities at that time-point. Therefore, the probabilities reported in this paper gradually assign greater weight to present than past data.

In the context of excessive pricing, one may also assume that firms would consider possible future demand and cost shocks before increasing prices excessively and facing the now high risk of prosecution. The data available in terms of these assessments will take time to be released and will be based on the past – however, as explained by Boshoff and Van Jaarsveld (2019:8), updated probabilities may gradually focus more and more on present conditions rather than on those in the past.[28]

Nevertheless, using both the structural break and switching-regime methodologies could provide supporting evidence of when anti-competitive effects were truly present in the market.

5. Case study

In 2007 the Competition Commission of South Africa received an excessive pricing complaint against Foskor on their price of phosphoric acid (*Competition Commission South Africa v Foskor (Pty) Ltd* (43/CR/Aug10) [2011] ZACT 10). Foskor is the only local supplier of "phosphoric acid" in South Africa (Foskor (Pty) Ltd., 2020:3). Phosphoric acid is a liquid that has "agricultural, industrial, medical and retail" applications. The products made from it include, amongst others:

[28] Boshoff and Van Jaarsveld (2019:8) explain this issue further: "These filtered probabilities are 'smoothed', using the Kim procedure, which involves weighting filtered probabilities at time $t-1$, t, and $t+1$ to obtain smoothed probabilities for time t. In this sense, the probabilities reflect 'local' data. We acknowledge that this may underplay relevant data further into the future, under the assumption that firms could predict some of these data. Even so, this is not much different from alternative techniques, such as the structural break methods discussed in the next section".

> catalysts, rust proofing materials, chemical reagents, latex, dental cements, tooth whiteners, toothpaste, disinfectants, food supplements, carbonated beverages, waxes, polishes and animal feeds (Foskor (Pty) Ltd., 2020).

Phosphoric acid may be used directly as a fertiliser but is normally processed further into end-product fertilisers (*sinne nomine*, 2019:3). The case of excessive pricing against Foskor will be used as an example to show how econometric methods may assist authorities in determining the effective period involved.

5.1 Foskor as a case study

Pursuant to the excessive pricing complaint received by the Competition Commission in 2007, an investigation into Foskor's pricing structures occurred. The Commission found that prior to 2008 Foskor charged an excessive price to their domestic customers as the *local* price was based on an export price formula that included the cost of freight (a notional transport cost, insurance costs and interest charges) which is only relatable to the export market (*sinne nomine*, 2019:3). The reason Foskor included these costs was because their pricing system was based on an "international pricing method of pricing phosphoric acid" as a world commodity.[29] Foskor admitted to this conduct, paid an administrative penalty, and agreed to charge from then on only the price based on the Free On Board Richard's Bay Port, which removed the freight costs for local customers. The Free On Board Richard's Bay (FOB) price is the international published price (the export price, also called the India price) less freight charges (*Omnia Group Proprietary Limited t/a Omnia Fertilizer v Foskor Proprietary Limited (74266/17) [2019]*). The formal settlement agreement between Foskor and the Commission occurred on the 26 July 2010, which agreement subsequently became a Tribunal consent order on 28 February 2011 (*sinne nomine*, 2019:4). It is well known that Foskor voluntarily complied after 1 August 2008 (*sinne nomine*, 2019:4). There exists therefore less ambiguity regarding the former abuse because such abuse is followed by a long price regulated period (6 years) and it is therefore expected that the effects of the abuse has worn out.

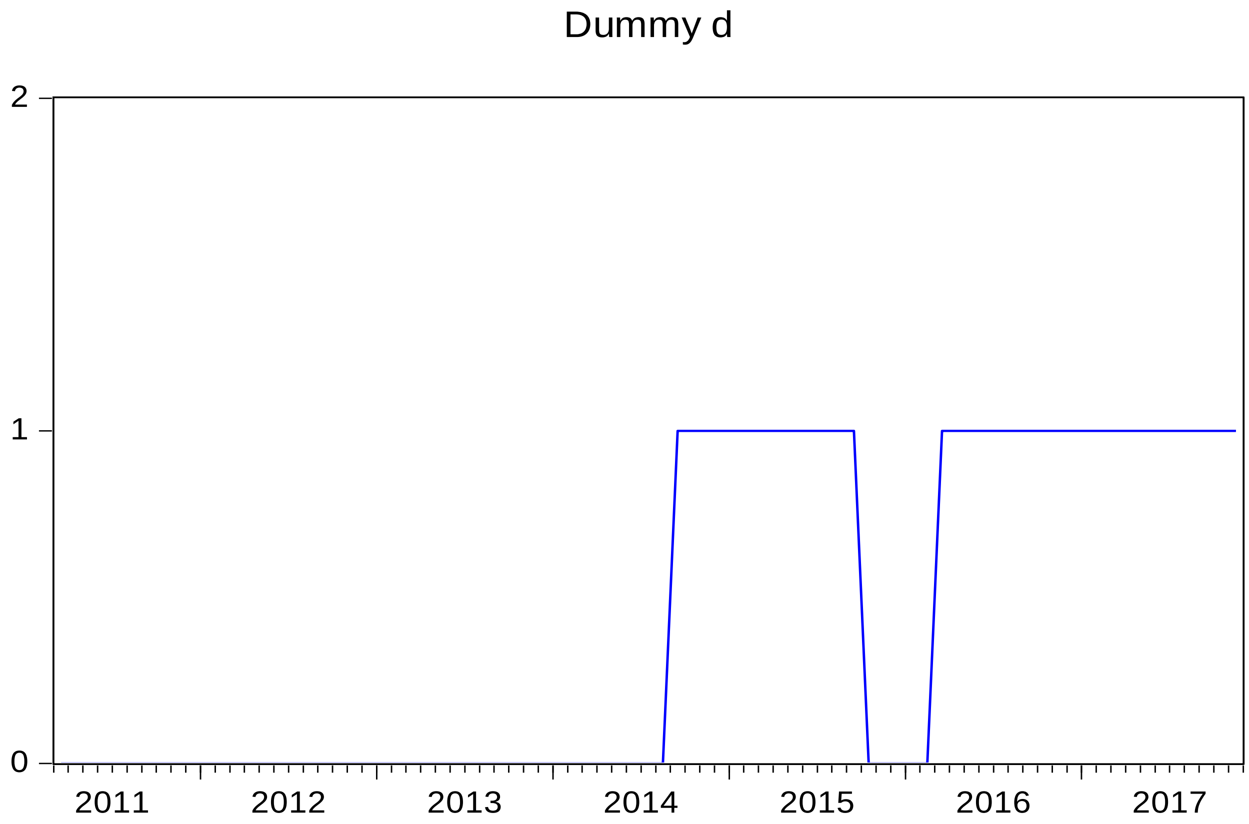
More intuitively for this study, is what happened after Foskor had complied with the consent order for 6 years. The consent order made no provision for changing market conditions, and the data

[29] This system was based on the "India CFA (free carrier) price, which comprised the India CFR (Cost and freight price) plus 30 day interest charges on US prime rates calculated on the India CFR price." Thus, "local customers were charged FOB price at Richards Bay Port calculated on India CFR phosphoric acid less 25% discount on the freight and interest charges. The nominal transport and interest charges therefore accounted for 75% of the freight portion of the India CFR price." (*sinne nomine*, 2019:3).

showed that the export prices benchmark that Foskor was obliged to charge fell below the production costs of phosphoric acid (*sinne nomine*, 2019:4). Foskor occurred significant losses in their local market by having to follow this strict pricing structure, and therefore breached the consent order after August 2014 by starting to charge a price based on a cost-plus pricing model that was higher than that based on the Free On Board Richard's Bay Port benchmark (*sinne nomine*, 2019:4). This formal date of September 2014 was identified in *Omnia Group Proprietary Limited t/a Omnia Fertilizer v Foskor Proprietary Limited (74266/17) [2019]* based on sales agreements that showed a higher price than the published international price agreed upon. This constitutes a highly anticipated break and is picked up by both econometric methods in the next sections. The focus in this case is that the start and end dates of excessive pricing were determined exogenously by considering formal liability evidence. Although the regulation of Foskor's price in this case is debatable, the objective is to show how formal dates that are determined qualitatively may be deceiving.

Therefore, accepting that the data period started in a "competitive regime", as Foskor asked the regulated FOB price after 2008, the first formal liability date follows, when it was shown on sales agreements that Foskor asked the FOB price up until August 2014. Therefore, September 2014 constitutes the first formal starting date. The dummy therefore switches from 0 to 1 and stays 1 until the judgement was granted on 16 October 2015, obliging Foskor to return to the regulated price (the dummy turns to 0). It was only in March 2016 when Foskor lodged an appeal that suspended the 2015 High Court order where Foskor was granted leave to appeal (the dummy changed to 1) up until November 2017 when the appeal was dismissed with costs – Foskor had to comply. The parties started a mediation process on the 23rd February 2018 to find a balance between them, whereafter they settled. Thus, the formal liability dummy in the data is represented as follows in figure 2.

Figure 2: Formal liability dates associated with a dummy variable



There are many reasons why the anti-competitive effects may differ from the periods shown in figure 2. What is expected to happen in 2014 is worrying: the dates on the initial sale agreements were only the start of the price increases. The Foskor acid division increased prices gradually and only in 2016 has the local price exceeded the production costs. It is possible to speculate with regard to the nature Foskor's business, its market share, the size of the firm, its operations, its existing agreements, its pricing lags, its menu costs, its dismantling capacity, postponing its innovation, et cetera, but the idea here is not to speculate, but to determine these dates quantitively via econometric methods and to see if they fit the Foskor context.

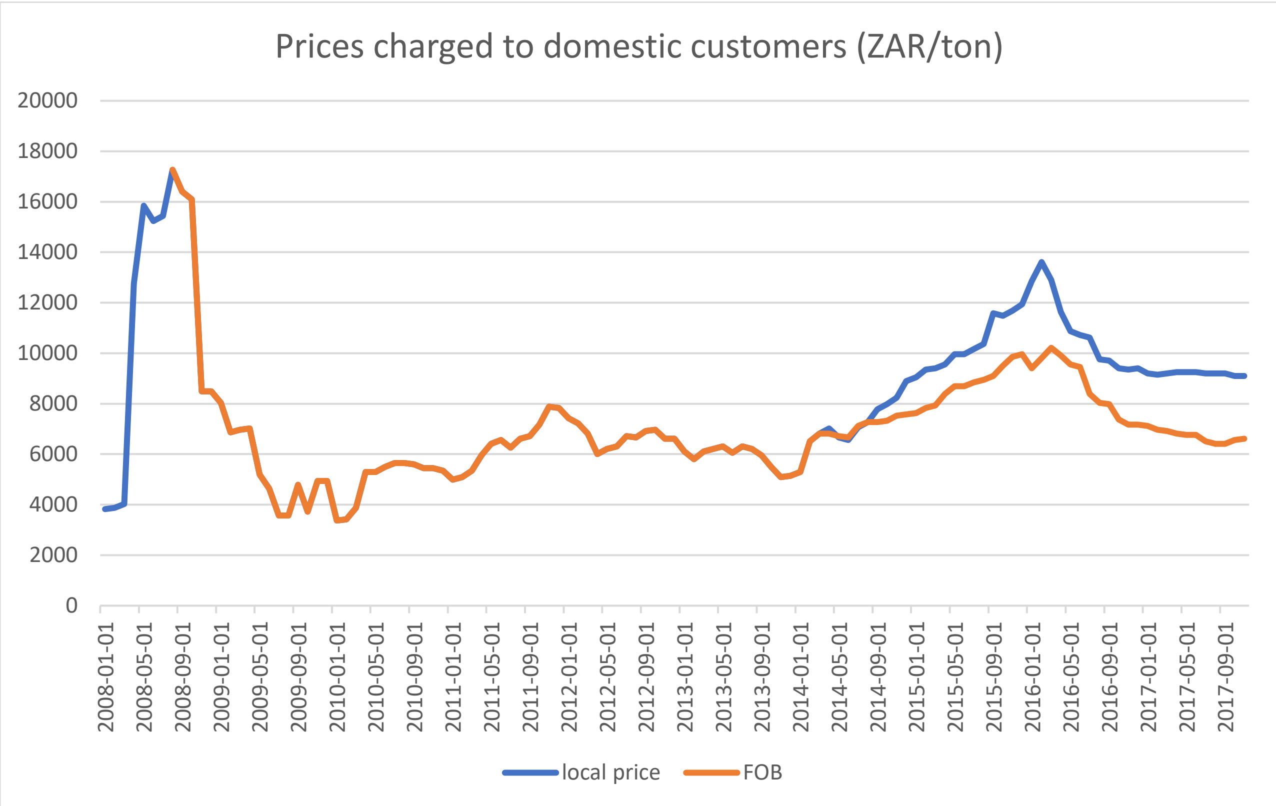
5.2 Data

Foskor is a more recent excessive pricing case, for which data is available, and is therefore chosen as the subject of a case study. Note that *Foskor* relates to behaviour prior to the Covid-19 pandemic, so that the subsequent regulations related to excessive pricing during the disaster period did not apply in this case.

Following international practice, the study relies on a comparator benchmark approach =where the investigated price (the local price) is compared to the price charged in a similar market (export prices) charged by Foskor. The price data in this study are compiled from a combination of sources, including Foskor's Integrated Reports (2011 - 2019), other phosphoric acid pricing data from Quantec Easydata and data from the Department of Mineral Resources. The data consists of the monthly pricing data (in Rand) of phosphoric acid. The sample used includes prices charged from January 2013 to November 2017, as the focus is on behaviour and the effects following the regulated period.

Referring back to equation (1), p_{it} here is the nominal price of phosphoric acid for the domestic market, measured in Rand per ton. As can be seen in Figure 3, the excessive local price was charged prior to 2008. FOB prices became relevant only after the initial excessive pricing case, as Foskor was obliged to charge FOB (thus the local price and the FOB price were the same) until the breach in 2014, when Foskor charged prices higher than the FOB price.

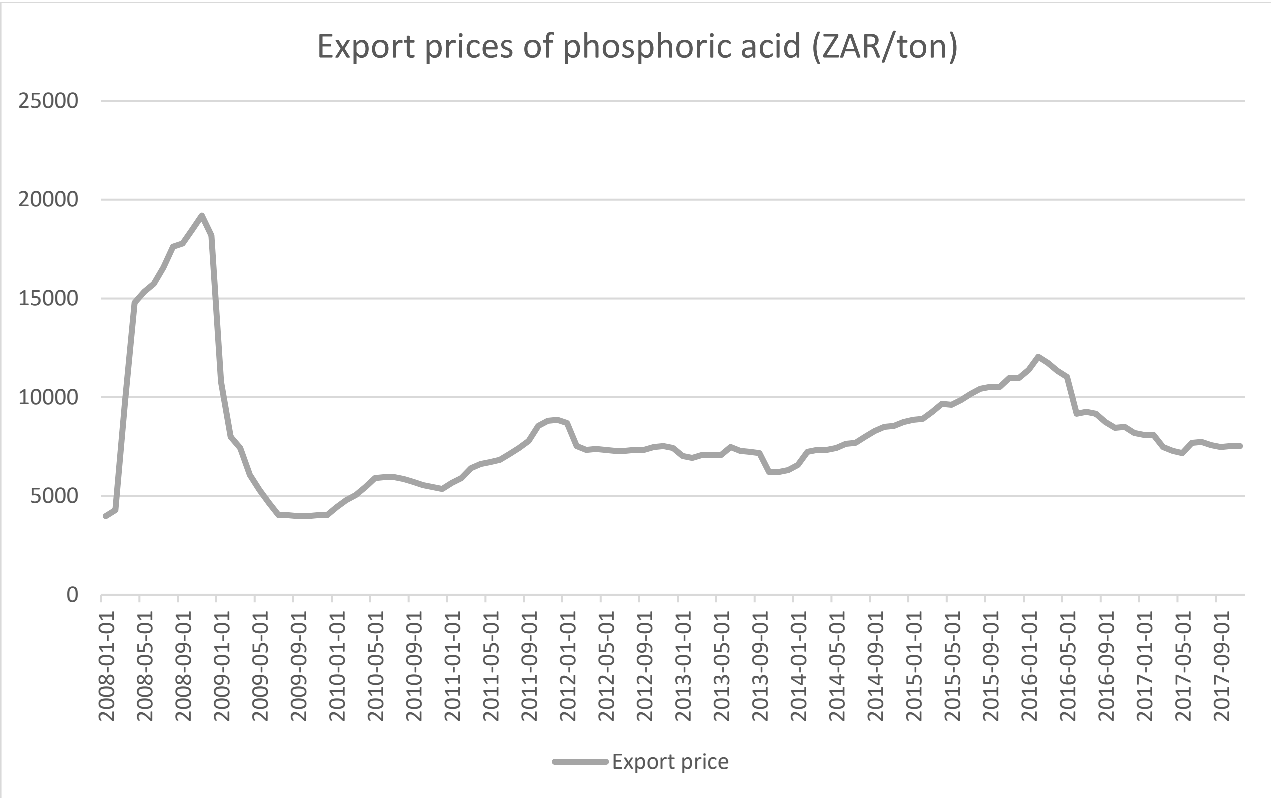
Figure 3: Local prices



Source: Foskor Integrated Report: 2011 – 2019 and Department of Mineral Resources.

The above dependant variable is regressed on Foskor's nominal export prices as the independent variable x_t in (1), measured in Rands per ton. Figure 4 shows the pricing data of this comparable market. Essentially, export prices are used to predict the domestic but-for price, had excessive pricing not occurred. In this case the but-for price is almost clearer, because the Tribunal explicitly obliged Foskor to charge the export price minus freight, constituting the benchmark to compare prices against formally. Whether one agrees with such rigid price regulation is highly contested but irrelevant for this paper. Using the export price in this way implies making the reasonable assumption that export and local prices share the same demand and supply drivers.

Figure 4: Export prices



Source: Foskor Integrated Report: 2011 – 2019, Department of Mineral Resources, and Quantec EasyData[30].

Foskor primarily exports phosphoric acid to India (the main export market) and the export price worked with is the CRF (Cost and Freight) India price. The CRF India price less freight costs as explained above constitute the FOB price that Foskor was obliged to charge. The price data are likely to adjust quicker than the data on cost and demand drivers, and are therefore used.

[30] Copyright 2020 Quantec EasyData. Title: Export — H28092: Phosphoric acid and polyphosphoric acids — C0: World — SA Rand (ZAR, current prices): Total, all measures. Download Date: 2020-03-21.

In terms of the data description, we do not reject the presence of a unit root within local prices, as expected and explained above. We do, however, reject the possibility for a unit root within the export prices at 1% significant level, which is also consistent with the expectation that there were no anti-competitive conditions that changed in the export market.[31] The rest of the descriptive statistics of the total dataset are reported in table 2.

Table 2: Descriptive statistics

	Local price	**Export price**
Mean	7840.225	8224.172
Median	6972.617	7479.716
Maximum	17266.73	19193.71
Minimum	3372.211	3980.730
Std. Dev.	2975.209	3138.216
Skewness	1.067915	1.649557
Kurtosis	4.006341	6.040816
Jarque-Bera	27.64019	99.81476
Probability	0.000001	0.000000
Sum	932986.8	978676.5
Sum Sq. Dev.	1.04E+09	1.16E+09
Observations	119	119

Source: Foskor Integrated Report: 2011 – 2019, Department of Mineral Resources, and Quantec EasyData.

5.3 Structural break results

Following Rubinfeld's (1985) advice on how to use econometrics in the courtroom, as discussed in section 3.3, this section refrains from reporting only the "best fit" results from the data, but reports the majority of the possible outcomes to support the consistency and robustness of the proposed tests in the light of recognising effective dates. However, some of the possible results must be sifted out, based on sound economic reasoning. It is expected that with the number of observations in the sample and the nature of the data (price data adjust quicker than cost and demand drivers, for example)

[31] Van Jaarsveld is currently doing work on what the impact of non-stationarity may be on estimating damages. He shows that this is of great concern when one is dealing with data which only a small part of the data entails anti-competitive behaviour. The data of *Foskor* consist out of roughly 55 -60% of anti-competitive behaviour, putting it out of the danger zones. Nevertheless, Van Jaarsveld propose doing cointegration tests to ensure that the results is not spurious. Such tests can be shown as it was conducted on the models in the results that follow. To summarise, it showed that with the traditional dummy variable model, no cointegration relationship exist as expected. Using formal dates leads to spurious damage results. The Markov – switching model on the other hand tested positive in the sense that a long-run relationship does exist (the null is rejected).

autoregressive lags up until 6 places are considered. Table 3 shows all the results for the identified structural breaks, with those in bold representing those with the smallest information criteria.

Table 3: Information criteria for model specification:

Model specifics	***# lags***	***AIC***	***SBC***	***HQC***	***Log Like-lihood***	***Significance of coefficients (Prob.)***	***Result - breaks:***
Bai-Perron. Only intercept classified as a breaking regressor	6	-3.697741	-3.169554	-3.491558	0.98	Many insignificant	2014M02
	5	-3.854433	-3.361458	-3.661996	0.98	Some lags insignificant	2014M02, 2016M03
	4	-3.718428	-3.331091	-3.567227	0.98	Many insignificant	2014M02
	3	**-3.904973**	**-3.517636**	**-3.753773**	**0.98**	**More significant**	**2014M12, 2016M06, 2017M03**
	2	-3.541003	-3.294515	-3.444784	0.97	Many insignificant	2014M02
	1	-3.558765	-3.382703	-3.490038	0.97	Constants insignificant	2014M02
	0*	-3.484854	-3.344004	-3.429871	0.97	All significant	2016M06, 2017M03
Bai-Perron. Intercept and dependant variable's lags specified as breaking regressors	6	-3.790611	-3.151149	-3.601955	0.98	Many insignificant	2015M09
	5	-3.411111	-2.988561	-3.246165	0.97	Many insignificant	No breakpoint selected
	4	-3.471067	-3.118942	-3.333611	0.97	Many insignificant	No breakpoint selected
	3	-3.627340	-3.204790	-3.462393	0.98	Some insignificant	2014M07
	2	-3.660601	-3.343689	-3.536892	0.97	Some lags insignificance	2016M02
	1	**-3.799837**	**-3.518137**	**-3.689873**	**0.98**	**Intercept and 1 lag insignificant**	**2014M02, 2016M04**
	0*	-3.484854	-3.344004	-3.429871	0.97	All significant	2016M06, 2017M03
Bai-Perron. All switching:	6, 5, 4, 3	Specification leads to singular matrix in at least one sub-sample					
	2	**-3.978744**	**-3.556194**	**-3.813797**	**0.98**	**Many insignificant**	**2014M04**
	1	**-3.955521**	**-3.673821**	**-3.845557**	**0.98**	**Slight insignificance**	**2016M03**
	0	**-3.926197**	**-3.714922**	**-3.843724**	**0.98**	**Mostly significant**	**2013M11 2016M04**

*Essentially the same model; Models in bold have the smallest information criteria.
Source: Foskor Integrated Report: 2011 – 2019, Department of Mineral Resources, and Quantec EasyData combined in excel and regressed via Eviews.

Even though many different specifications are used, there is some consistency between the identified breaks. The literature on this suggests that only the intercept should be allowed to switch, (this includes the first block in the table). Of those models (where only the intercept switches), the one in bold represents the best fit in terms of information criteria retrieved. Thus, the break dates of December 2014, June 2016 and March 2017 are identified. If this model is chosen, then these dates fit the context of Foskor quite intuitively: December 2014 is highly likely as Foskor started to deviate from the consent order only in August 2014, but it may have taken the months up until December to reach a point of abuse again, as prices were only gradually increased, for instance; June 2016 is recognised when Foskor lodged the appeal and the High Court order was suspended. We can assume that Foskor reverted to charging a higher price, as their objective was to get the price higher than production costs; and finally we can also justify the 2017 break as they might have abandoned their attempts, and the appeal was later dismissed. The difference between the formal dates and the effective structural break dates when using this model is summarised as follows:

Formal date	Effective structural break date	Difference
1 September 2014	December 2014	4 months
March 2016	June 2016	4 months
November 2017	March 2017	-9 months

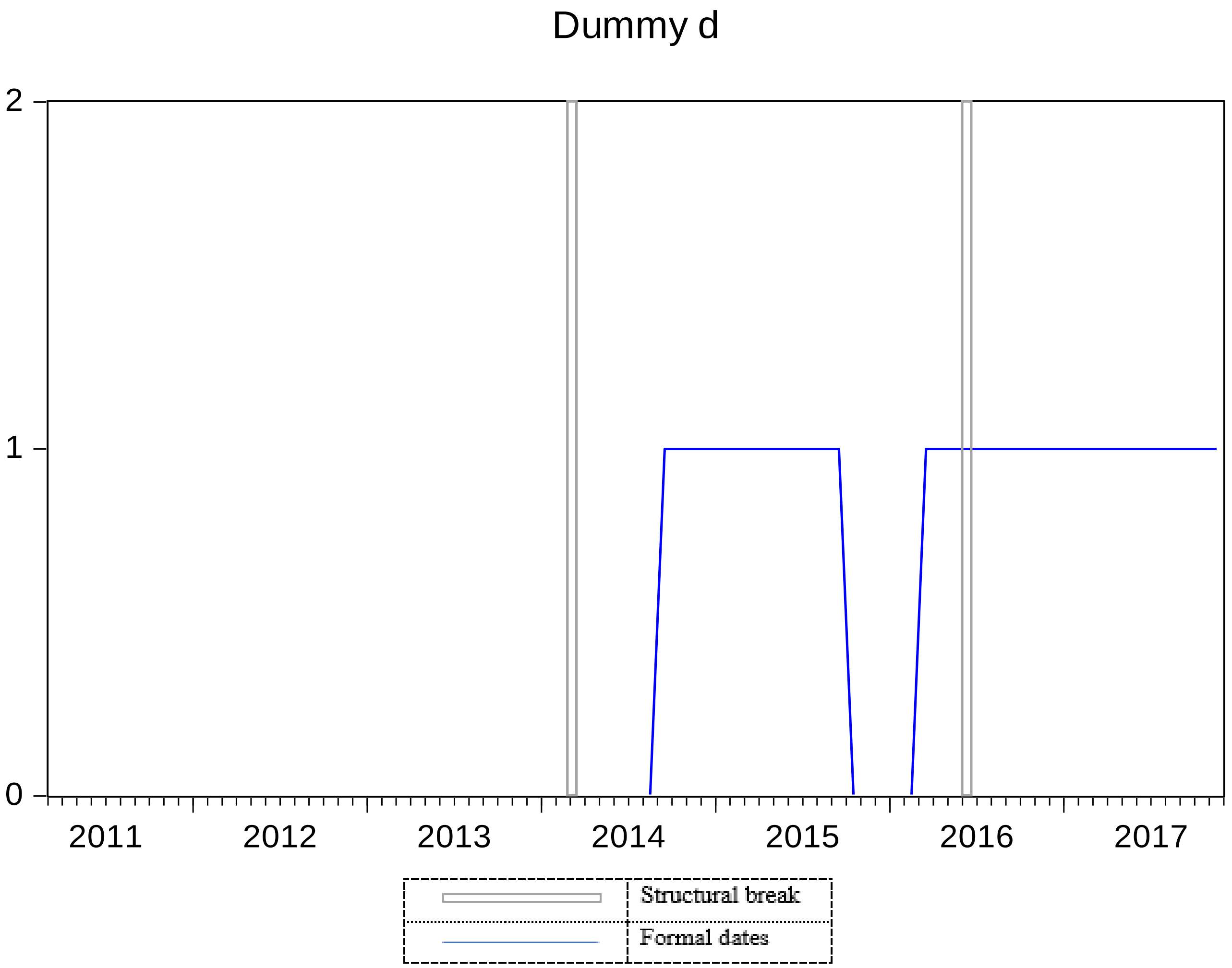
Clearly, there is a notable difference between the formal and effective dates in this model. However, if one decides not to use just one model, but to look at all the different results, then there is still some consistency. Most of the models recognise the 2014 and 2016 breaks. The above results are summarised from an overall viewpoint in table 4.

Table 4: The number of models identifying well-specified break dates

Nov 2013	**Feb 2014**	April 2014	Dec 2014	March 2016	April 2016	**June 2016**
1	**6**	1	1	2	2	**5**

The structural breaks identified most frequently by different models are those of February 2014 and June 2016. These are be represented visually in figure 5 to simplify the interpertation thereof:

Figure 5: Most frequently identified structural breaks compared to formal liability breaks



When adopting this point of view, a break (February 2014) earlier than the formal break (September 2014) is recognised, and it is of the opinion that this may be due to Foskor's not starting to charge every single customer the higher price at any one point in time. There may be still customers paying the lower price, and because one is working with a weighted average, such average will start to change earlier (Boshoff, 2020).[32] The 2016 break is the same as that identified in the model that was singled out above.

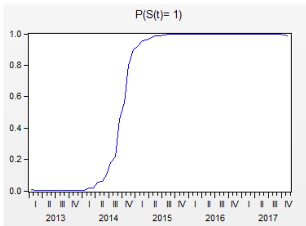
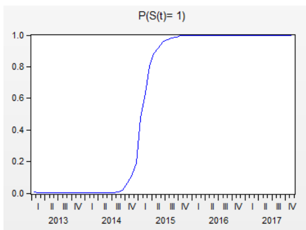
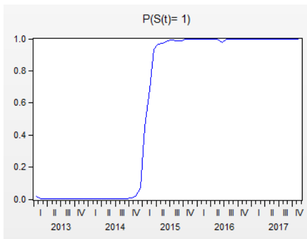
These structural break tests are fairly easy to replicate and do not entail many decisions/specifications. One would want that the following section showing the regime-switching models to produce fairly similar results, but it is expected that the effects might follow a bit later or last a bit longer due to transitioning.

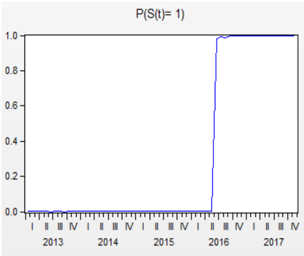
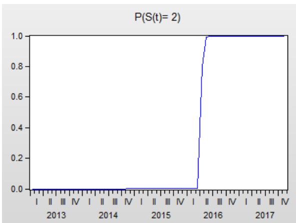
[32] These results were the topic of discussion in a CCLE (Centre of Competition Law and Economics) seminar held on 28 September.

5.4 Regime-switching results

Keeping to the same mind-set about exploring all possibilities and not simply the best fit, table 5 reports the possible regime-switching results. To compare them with the findings from structural breaks, the intercept is allowed to switch, as discussed above (which is essentially the dummy variable approach). These specific regime models also recognise the 2014 break, together with some transitioning. Some deviations recognise the changes in 2016 as well.

Table 5: Regime switching models

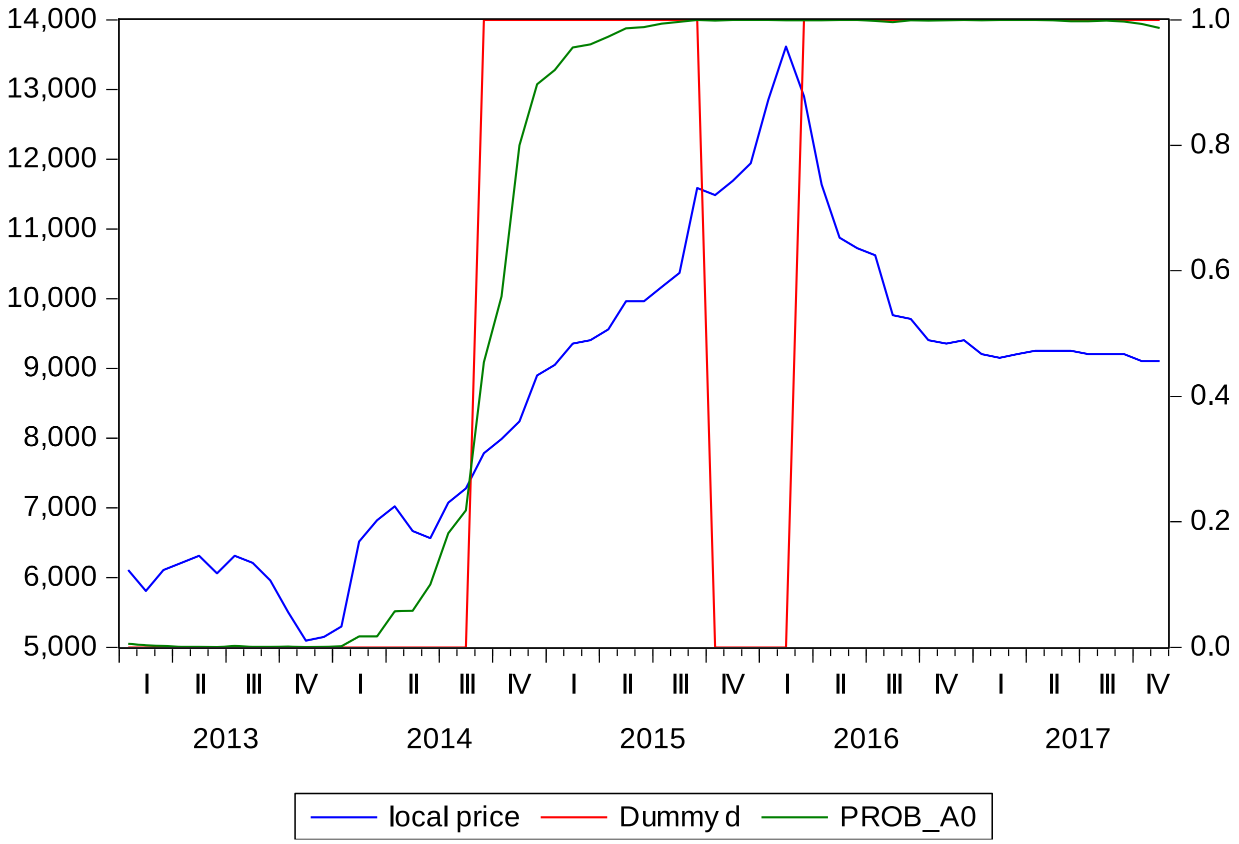
Model specifics	***#lags***	***AIC***	***SBC***	***HQC***	***Significance of coefficients***	***Result***	***Log-likelihood***	***Prob save***
Only intercept switch	6, 5, 4	NA	NA	NA	NA	NA	NA	NA
	3	**-1.548996**	**-1.196871**	**-1.411541**	**All significant (except intercept and export price lag)**	 **-Transition over 2014**	**93.60351**	**Prob_a0**
	2	-1.548996	-1.196871	-1.411541	All significant (except intercept and export price lag)	 -End **2014**	55.69538	Prob a2
	1	-2.300991	-2.019291	-2.191027	All significant (except intercept)	 -End **2014**	75.87924	Prob a3

	0	-2.516006	-2.304731	-2.433533	All significant	 -Start **2016**	80.22219	Prob a4 – still overcharge
Intercept, dependant variable's lags switch	6, 5, 4, 3, 2, 1	NA	NA	NA	NA	NA	NA	NA
Intercept, dependant variable's lags and variances switch	6, 5, 4, 3, 2, 1	NA	NA	NA	NA	NA	NA	NA
All switch (except variances)	6, 5, 4, 3, 2, 1	NA	NA	NA	NA	NA	NA	NA
All switch (except variances)	6, 5, 4, 3, 2, 1	Near singular matrix error. Regressors may be perfectly collinear						
	0	-3.436686	-3.190199	-3.340467	All perfectly significant 0.000	 -begin 2016	108.3822	Prob b1
All switch with variances switch	6, 5, 4, 3, 2, 1, 0	NA	NA	NA	NA	NA	NA	NA

Source: Foskor Integrated Report: 2011 – 2019, Department of Mineral Resources, and Quantec EasyData combined in excel and regressed via Eviews.

Once again, if one had to choose a model (based on the dictum in the literature that only the intercept should switch) the model in bold (three lags in the first set of models) would be preferred, based on maximum log likelihood. It shows a change occurring from the start of 2014 – but due to transitioning only reaching the "anti-competitive" state in 2015. It thus corresponds to the structural dates found in the previous section (February 2014/ December 2014 dependant on which view is taken), with the expected difference of taking a bit longer to account for transitioning and it therefore also fits the economic context of the market. A comparison of these results with the formal breaks is shown in figure 5 where "Dummyd" represent the formal dates and "Prob_A0" the relevant model's effective dates. The effective dates represent a period that started of gradually (not simply a straight line) and may likely represent the true effect in the market. In this model, the quick, formal transitioning from anti-competitive to a competitive state and back around 2016 did not influence the effect that was still present in the market. It is expected that such a short period of competitive prices is overshadowed by the underlying effect lingering over that period.

Figure 6: Identified regime-switching periods compared to formal liability periods



Source: Foskor Integrated Report: 2011 – 2019, Department of Mineral Resources, and Quantec EasyData combined in excel and regressed via Eviews.

If one is of the opinion that this particular model should not be preferred, then the other specifications also provide intuitive results. The next section will show that irrespective of which one of these models is used, it still amounts to an underestimation of the damages when the formal liability dates are used instead of these estimations of the effect. It might be worthwhile to mention that although the very last model (the blue one) yielded results, it is unlikely that a price model with no lags would yield significant results. These prices are non-stationary, and a dynamic model would be more appropriate, as will be seen in the next section.

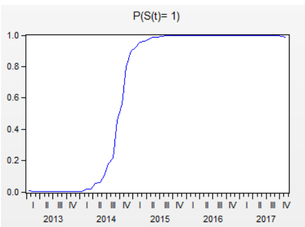
5.5 Damages results

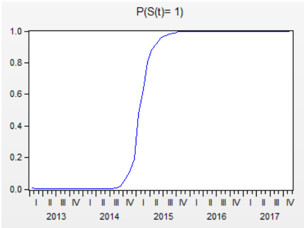
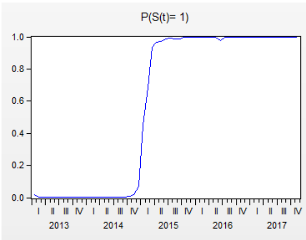
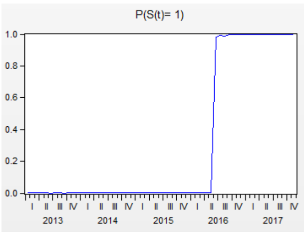
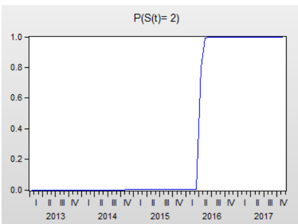
In the previous sections, although there are some possibilities for different results, the difference between formal and effective dates in all the possible models was clear. One would now want to know what the impact of the different dates is by comparing the damages under the formal identified period with the damages accrued under the effective period.

As determined in the data, we know we are working with a non-stationary dependant variable and a stationary independent variable. To determine the damages, we cannot therefore use a simple OLS regression. As discussed above, an ARDL regression would be more appropriate. We therefore first estimate the damages by using the formal liability dates, to compare the outcome with that of using more effective dates. These prices will be logged to simplify interpretation and all the regressions are run with 2 lags to be able to compare them to one another. (This might have been any other number – the intuition remains.)

Following the traditional method, running an ARDL regression with this dummy to determine overcharge yields an overcharge of 1.97%. The results are shown in Appendix A. All the regime-switching models that retrieved results were used to estimate the damages respectively, and the results are summarised in table 6. (The outputs are available in Appendix A.)

Table 6: Regime model damage estimations

Model specifics	***Result***	***Overcharge estimation***	***Significance (Prob.*)***	***Difference from dummy approach 1.97%***
Only intercept switch	 **-Transition over 2014**	**Prob_a0** **13.7%**	0.0001	-11.8%

	 -End **2014**	Proba2 9.7%	0.0105	-7.8%
	 -End **2014**	Proba3 6.9%	0.043	-5%
	 -Start **2016**	Proba4 8.6%	0.0016	-6.7%
All switch (except variances) -Not likely to represent true results without any lag terms in the probability estimation	 -begin 2016	Probb1 coefficient: 3.6%	0.1689	-1.7%

Source: Foskor Integrated Report: 2011 – 2019, Department of Mineral Resources, and Quantec EasyData combined in excel and regressed via Eviews.

It is apparent that although some models show a bigger difference than others, they all represent an underestimation of the damages when formal liability dates are used without second thought. It is important to note that these results may go both ways: in the case studied, it showed that there was an underestimation of damages, but if the case at hand is one that shows that there was actually less of an effect than that which is included in the formal liability dates, then the effective damages would reveal an overestimation of damages and the differences above would be positive, as less damage occurred. It aims to identify the true effects, irrespective of whether they are positive or negative, dependent on the case-relevant data. Although there are complications, the regime-switching models still yielded intuitive results.

6. Conclusion

This paper shows that anti-competitive effects are not necessarily captured by adopting the traditional approach of using formal documentary evidence to determine their presence in the market. Getting the period or dates of when these effects occur wrong has a significant implication for establishing excessive pricing and estimating the damages appropriately. After studying the different benchmarks against which excessive pricing is measured, current policy approaches and cases, this paper suggests that taking an econometric approach to the issue of dating may be beneficial in this regard. This involves supplementing the traditional but-for methodology with a structural break test and a Markov switching-regime test to properly determine the effective dates of the case at hand.

After analysing the results emanating from the proposed solution, it is clear that although intuitive, such additional econometric methods are not to be used as silver bullets in all like cases. What has been learnt from this study is that one must first see if the formal dates differ from those of effect, and if so, control whether sound economic reasoning can explain this difference in the particular case. Then it may be advantageous to use different econometric tests to identify more effective dates, conditional on whether they fit sound reasoning in the context of the market. Using different tests may confirm the robustness of the effective dates.

The author acknowledges that legal practitioners may find it impractical to adopt such an approach. Yet it may be worthwhile to create a type of threshold beyond which the approach should be adopted. For example, if the formal damages exceed X amount then it should be at least be thought desirable to study the effects, as this could have a significant impact in the market economy. New developments, however, like those produced by Adam Giles from the Financial Conduct Authority (FCA) are leading in the direction of using machine learning economics to determine effects. Nevertheless, this paper shows that taking a more fully-fledged effects-based approach by using econometric methods together with intuition from the context of formal evidence towards dating excessive pricing will produce better estimates, and that the outcomes would in practice therefore be more just.

7. Appendix A: Damage estimation

7.1. Damages under traditional fixed dummies:

Dependent Variable: LNLOCAL_PRICE
Method: ARDL
Date: 09/25/20 Time: 17:57
Sample: 2013M01 2017M11
Included observations: 59
Dependent lags: 2 (Fixed)
Dynamic regressors (2 lags, fixed): LNEXPORT_PRICE
Fixed regressors: DUMMY_D C

Variable	Coefficient	Std. Error	t-Statistic	Prob.*
LNLOCAL_PRICE(-1)	1.022189	0.136197	7.505197	0.0000
LNLOCAL_PRICE(-2)	-0.126415	0.135235	-0.934780	0.3542
LNEXPORT_PRICE	0.475538	0.138720	3.428028	0.0012
LNEXPORT_PRICE(-1)	-0.103358	0.203039	-0.509055	0.6129
LNEXPORT_PRICE(-2)	-0.265979	0.154792	-1.718303	0.0917
DUMMY_D	0.019753	0.015688	1.259093	0.2136
C	-0.023758	0.337896	-0.070311	0.9442

R-squared	0.975434	Mean dependent var	9.040476
Adjusted R-squared	0.972599	S.D. dependent var	0.255367
S.E. of regression	0.042271	Akaike info criterion	-3.378428
Sum squared resid	0.092916	Schwarz criterion	-3.131940
Log likelihood	106.6636	Hannan-Quinn criter.	-3.282209
F-statistic	344.1235	Durbin-Watson stat	2.001961
Prob(F-statistic)	0.000000		

*Note: p-values and any subsequent tests do not account for model selection.

7.2 Damages under the intercept switching model with the highest probability likelihood:

Dependent Variable: LNLOCAL_PRICE
Method: ARDL
Date: 09/25/20 Time: 17:47
Sample: 2013M01 2017M11
Included observations: 59
Dependent lags: 2 (Fixed)
Dynamic regressors (2 lags, fixed): LNEXPORT_PRICE
Fixed regressors: PROB_A0 C

Variable	Coefficient	Std. Error	t-Statistic	Prob.*
LNLOCAL_PRICE(-1)	0.829083	0.129797	6.387516	0.0000
LNLOCAL_PRICE(-2)	-0.220680	0.121029	-1.823373	0.0740
LNEXPORT_PRICE	0.414852	0.121927	3.402449	0.0013
LNEXPORT_PRICE(-1)	0.019754	0.181218	0.109009	0.9136
LNEXPORT_PRICE(-2)	-0.183126	0.137614	-1.330727	0.1891
PROB_A0	0.137484	0.033272	4.132132	0.0001
C	1.183294	0.422696	2.799394	0.0072

R-squared	0.980943	Mean dependent var	9.040476
Adjusted R-squared	0.978744	S.D. dependent var	0.255367
S.E. of regression	0.037231	Akaike info criterion	-3.632339
Sum squared resid	0.072081	Schwarz criterion	-3.385851
Log likelihood	114.1540	Hannan-Quinn criter.	-3.536120

F-statistic	446.0999	Durbin-Watson stat	1.994610
Prob(F-statistic)	0.000000		

7.3 Damages under the rest of the switching models:

Dependent Variable: LNLOCAL_PRICE
Method: ARDL
Date: 09/25/20 Time: 17:49
Sample: 2013M01 2017M11
Included observations: 59
Dependent lags: 2 (Fixed)
Dynamic regressors (2 lags, fixed): LNEXPORT_PRICE
Fixed regressors: PROBA2 C

Variable	Coefficient	Std. Error	t-Statistic	Prob.*
LNLOCAL_PRICE(-1)	0.928256	0.135783	6.836319	0.0000
LNLOCAL_PRICE(-2)	-0.247889	0.139075	-1.782416	0.0805
LNEXPORT_PRICE	0.469278	0.131494	3.568817	0.0008
LNEXPORT_PRICE(-1)	-0.008496	0.196570	-0.043223	0.9657
LNEXPORT_PRICE(-2)	-0.227944	0.148082	-1.539305	0.1298
PROBA2	0.097238	0.036637	2.654059	0.0105
C	0.734006	0.439001	1.671990	0.1005

R-squared	0.977705	Mean dependent var	9.040476
Adjusted R-squared	0.975133	S.D. dependent var	0.255367
S.E. of regression	0.040270	Akaike info criterion	-3.475436
Sum squared resid	0.084326	Schwarz criterion	-3.228949
Log likelihood	109.5254	Hannan-Quinn criter.	-3.379217
F-statistic	380.0621	Durbin-Watson stat	2.038253
Prob(F-statistic)	0.000000		

*Note: p-values and any subsequent tests do not account for model selection.

Dependent Variable: LNLOCAL_PRICE
Method: ARDL
Date: 09/25/20 Time: 17:50
Sample: 2013M01 2017M11
Included observations: 59
Dependent lags: 2 (Fixed)
Dynamic regressors (2 lags, fixed): LNEXPORT_PRICE
Fixed regressors: PROBA3 C

Variable	Coefficient	Std. Error	t-Statistic	Prob.*
LNLOCAL_PRICE(-1)	0.964223	0.137178	7.029009	0.0000
LNLOCAL_PRICE(-2)	-0.210507	0.141079	-1.492120	0.1417
LNEXPORT_PRICE	0.458855	0.134588	3.409337	0.0013
LNEXPORT_PRICE(-1)	-0.029521	0.201111	-0.146791	0.8839
LNEXPORT_PRICE(-2)	-0.243959	0.151424	-1.611093	0.1132
PROBA3	0.069698	0.033634	2.072252	0.0432
C	0.515741	0.430716	1.197402	0.2366

R-squared	0.976616	Mean dependent var	9.040476
Adjusted R-squared	0.973918	S.D. dependent var	0.255367
S.E. of regression	0.041242	Akaike info criterion	-3.427745
Sum squared resid	0.088445	Schwarz criterion	-3.181257
Log likelihood	108.1185	Hannan-Quinn criter.	-3.331526
F-statistic	361.9582	Durbin-Watson stat	2.024992
Prob(F-statistic)	0.000000		

*Note: p-values and any subsequent tests do not account for model selection.

Dependent Variable: LNLOCAL_PRICE
Method: ARDL
Date: 09/25/20 Time: 17:51
Sample: 2013M01 2017M11
Included observations: 59
Dependent lags: 2 (Fixed)
Dynamic regressors (2 lags, fixed): LNEXPORT_PRICE
Fixed regressors: PROBA4 C

Variable	Coefficient	Std. Error	t-Statistic	Prob.*
LNLOCAL_PRICE(-1)	0.877386	0.134083	6.543595	0.0000
LNLOCAL_PRICE(-2)	-0.212136	0.127494	-1.663881	0.1022
LNEXPORT_PRICE	0.722500	0.150037	4.815462	0.0000
LNEXPORT_PRICE(-1)	0.000562	0.189604	0.002964	0.9976
LNEXPORT_PRICE(-2)	-0.269464	0.141453	-1.904973	0.0623
PROBA4	0.086025	0.025829	3.330615	0.0016
C	-1.092246	0.438043	-2.493470	0.0159

R-squared	0.979136	Mean dependent var	9.040476
Adjusted R-squared	0.976728	S.D. dependent var	0.255367
S.E. of regression	0.038956	Akaike info criterion	-3.541762
Sum squared resid	0.078915	Schwarz criterion	-3.295275
Log likelihood	111.4820	Hannan-Quinn criter.	-3.445544
F-statistic	406.7193	Durbin-Watson stat	2.052598
Prob(F-statistic)	0.000000		

*Note: p-values and any subsequent tests do not account for model selection.

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