Colluding on the implementation of an environmental standard: Insights from the European truck cartel

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Abstract

Between 1997 and 2011, the members of the European truck cartel coordinated the timing of the introduction of the technologies required by the EURO III to VI emission standards. For EURO III, they agreed not to offer compliant trucks before it became compulsory to do so, and coordination extended to the later standards. Although their trucks complied with the regulations, some of their emissions could have been avoided. We construct two counterfactual scenarios for competitive adoption and use an instrumental-variable strategy to link excess truck-related $PM_{2.5}$ emissions to infant respiratory hospital admissions. Depending on the scenario, estimated excess emissions range from 84,000 to 155,000 tonnes, with a midpoint of approximately 119,000 tonnes, and are associated with 12 to 18 avoidable admissions per 1,000 births per year. Our main contribution is to show that the environmental harm of a cartel that only delays compliance, which neither environmental regulation nor competition fines capture, can be estimated with public data through steps that competition authorities can assess one by one.

1 Introduction

Meeting ambitious environmental goals depends not only on how regulations are designed but also on how firms respond to them. The European truck cartel (Case AT.39824 – Trucks) shows how competitors can coordinate on something other than prices: the date at which cleaner vehicles reach the market.

DAF, Daimler, Iveco, MAN, Scania and Volvo/Renault, which together held nearly 90% of the European truck market, colluded on the timing of the introduction of the emission technologies required by the EURO III to VI standards (European Commission, 2016, Art. 1). They agreed not to offer compliant trucks before it was compulsory to do so (European Commission, 2016, recital 52). This coordination continued up to the EURO VI standard, although the agreed dates did not always coincide with the legal deadlines and were not always respected (General Court, 2022, paras. 272–278).

This part of the case raises a paradox. The trucks sold complied with the regulation in force, yet part of their emissions could have been avoided. Because the standards were met, the damage fell outside the remit of the environmental regulator. Nor is it reflected in the fines of about €3.8 billion imposed in 2016 and 2017, which were calculated on the basis of the value of sales, the gravity and the duration of the infringement (European Commission, 2006).

Our work relates to three strands of literature. First, a growing debate examines how competition policy should take environmental objectives into account (Monti, 2020; Inderst and Thomas, 2023; Holmes, 2024). Theoretical work suggests that cooperation between competitors tends to weaken rather than strengthen incentives to invest in sustainability (Schinkel and Spiegel, 2017), although coordination may in some cases help bring greener products to market (Inderst et al., 2023). The truck cartel offers a documented case in which coordination delayed cleaner technologies. Second, our paper relates to empirical studies of firms' strategic responses to emission standards in the European vehicle market. Reynaert (2021) shows that car manufacturers responded to tighter standards mainly by adopting new technologies, and Alé-Chilet et al. (2026) estimate the welfare cost of the German carmakers' coordination on AdBlue tank sizes, a harm that operated through emissions rather than prices. Earlier assessments of the truck cartel focused on the overcharge and deadweight loss borne by buyers (Beyer et al., 2020), not on its environmental consequences. Third, we draw on the literature linking air pollution, and traffic-related pollution in particular, to infant health (Chay and Greenstone, 2003; Currie and Walker, 2011; Knittel et al., 2016), including evidence from emissions cheating in the European car market (Alexander and Schwandt, 2022).

The question is how this environmental damage can be measured.

This article makes three contributions. First, it highlights a form of collusion that differs from the joint non-compliance studied by Alé-Chilet et al. (2026): firms complied with the standard, but only at the last possible moment. We refer to this as "minimal-compliance collusion". Second, it shows that the resulting harm can be estimated with public data, through a series of steps that each rest on an identifiable assumption and can be challenged separately. Third, it shows that this harm extends beyond the usual scope of competition analysis, because it does not operate through prices. It reaches both buyers, who were deprived of trucks with lower operating costs, and third parties, who bear the health damage.

2 Measuring the damage with a counterfactual

2.1 Counterfactual scenarios

The damage is the gap between observed emissions and those a competitive market would have produced. The latter depend on a date that cannot be observed, namely when competing manufacturers would have adopted each standard. In our article (Dielen, Bougette and Charlier, 2026), we construct this counterfactual following the *but-for* approach used to assess cartel damages (Veljanovski, 2023), from two independent sources.

- The European Commission (EC) scenario. For each standard, the Commission's decisions (2016, 2017) contain evidence that the technologies needed for compliance were available to cartel members earlier, and that this availability was concealed. The scenario introduces each standard from that date.
- The European Environment Agency (EEA) scenario. The EEA dates the first registrations of compliant trucks in Europe, including those of non-cartel manufacturers and imports. The scenario reproduces the pace of adoption observed outside the cartel.

Two assumptions complete the construction. First, under competition, manufacturers would have had an incentive to differentiate by launching compliant trucks ahead of the deadline, a view the Commission itself expressed (European Commission, 2017, recital 286). Second, the number of trucks sold is assumed unchanged, so that only the composition effect of the fleet is measured.

2.2 The steps we follow

Moving from the counterfactual to its health effects takes five steps, all based on public data. Each step rests on an identifiable assumption. This lets the parties

focus their arguments on a specific step, and lets the effect of any challenge on the final result be recalculated.

1. We model the size and composition of the truck fleet using European Automobile Manufacturers' Association (ACEA) registration data from 1993 to 2018, by weight class (3.5 to 16 t and above 16 t). The average lifespan and annual mileage come from the French CNR long-haul survey for 2006 to 2010, which implies about seven overlapping cohorts in the fleet. Each truck is assigned the standard required at the time of registration.

2. We multiply the emission factors from the EMEP/EEA Guidebook 2019 (Tier 2 method, by standard and weight class) by the annual mileage and lifetime above, which gives the lifetime emissions of each truck type. Combined with the fleet composition, this yields yearly emissions. We use highway driving conditions and count only exhaust emissions, as these are what the EURO standards regulate.

3. We repeat steps 1 and 2 with the adoption dates of the EC and EEA scenarios (Section 2.1). Subtracting counterfactual emissions from observed emissions gives the volume of avoidable emissions.

4. We allocate yearly emissions to the 199 NUTS 2 regions with the gridding method recommended by the EMEP/EEA Guidebook EEA (2019). Each region receives a share of total emissions equal to its share of the European highway network. Long-haul trucks pollute along the routes they travel, so this allocates emissions to the regions they cross rather than to their place of registration.

5. We estimate the effect of truck-related $PM_{2.5}$ emissions on infant hospital admissions for respiratory diseases (Eurostat), on a panel of 199 NUTS 2 regions from 2000 to 2018. Emissions are instrumented with the shift-share variable described in Section 2.3. For each region and year, we then compute the share of emissions attributable to the cartel under each scenario. Multiplying predicted admissions by this share gives the number of admissions associated with the cartel, which we express per 1,000 births.

2.3 Identifying the health effect

To assess how emissions impact infant health, a comparison between regions would be misleading. Regions with more economic activity have both more trucks and different healthcare, and emissions are estimated rather than measured. Ordinary least

squares accordingly yields a much smaller coefficient (0.014), which is not statistically significant. We therefore instrument emissions with a shift-share variable that interacts pre-2000 highway density with the entry into force of the EURO standards, set at EU level (following Borusyak, Hull and Jaravel, 2025). The intuition is that regions with a dense highway network carry more truck traffic, so each tightening of the EURO standards cuts their emissions more than elsewhere. The pre-period highway network and the EU-wide regulatory timetable help motivate the identification strategy. Its causal interpretation nevertheless rests on the assumption that differential exposure to regulatory changes affects infant admissions through truck-related emissions rather than other concurrent local changes.

The instrumental-variable estimate indicates that a 1% increase in truck-related $PM_{2.5}$ emissions is associated with an approximately 0.116% increase in infant hospital admissions for respiratory conditions. The coefficient is estimated on a panel of 199 NUTS 2 regions from 2000 to 2018, with region and year fixed effects.

The robustness of this evidence is based on a set of tests, each of which answers a specific objection.

- The instrument is strong (Cragg-Donald statistic of 39.0, Sanderson-Windmeijer F of 10.2), and the Anderson-Rubin test, which remains valid with weak instruments, rejects a zero effect ($p = 0.008$).

- The result survives a partial failure of the exclusion restriction. If up to a quarter of the instrument's effect on admissions ran through channels other than pollution (Conley, Hansen and Rossi, 2012), the estimated effect would remain positive.

- The effect appears only for infants under one year old, not for the rest of the population. The coefficients remain robust when we consider an alternative dependent variable related to health in the same age group. This pattern is consistent with greater biological vulnerability among infants, although it does not rule out other explanations.

- The coefficient stays between 0.07 and 0.12 when extreme regions are excluded or when only the EURO III instrument is used, although it is then less precisely estimated.

2.4 Results

Under the two counterfactual scenarios, estimated excess $PM_{2.5}$ emissions range from 84,000 tonnes (EEA) to 155,000 tonnes (EC), with a midpoint of approximately 119,000 tonnes. Under observed emissions, predicted infant respiratory admissions

average 72.5 per 1,000 births per year. Under the counterfactual scenarios, they fall to about 60 (EEA) and 54 (EC), corresponding to 12 to 18 avoidable admissions per 1,000 births. The estimated effects persist beyond the cartel period, as trucks sold up to 2011 continued emitting in subsequent years, through 2017.

These estimates rely on exogenously specified adoption paths rather than adoption dates and market shares derived from a competitive equilibrium. They do not explicitly model pioneer–follower dynamics or strategic differences across manufacturers. To the extent that the scenarios imply earlier and more widespread adoption than competition would have delivered, they can be viewed as upper-end benchmarks for environmental harm, but not as formally established upper bounds.

These scenarios raise economic questions that the emissions calculations alone cannot resolve. What incentives would lead manufacturers to introduce cleaner trucks before the deadline? How might a public regulatory deadline facilitate coordination? And how could individual adoption decisions affect the counterfactual? We return to these questions in the conclusion.

3 Conclusion and limitations

An agreement on when to implement an environmental standard can harm the environment without breaching environmental law. The European truck cartel shows that such harm can be assessed using publicly available data and counterfactual adoption scenarios. These estimates remain conditional on the assumed timing of adoption and should be read as upper-end benchmarks rather than formal upper bounds.

This damage could also be expressed in monetary terms, along two routes. The first values the excess emissions directly, by multiplying the excess tonnes of $PM_{2.5}$ by a damage cost per tonne, such as those reported in the European Commission's *Handbook on the external costs of transport* (European Commission, 2019). The second values the avoidable infant admissions by a cost per hospital stay. Because it covers only one health outcome, it yields a lower bound. The two routes should not be added together, since damage costs per tonne already include health effects.

A related question, why competitors would choose to wait together, calls for a different kind of analysis. In companion work (Bougette, Charlier and Dielen, 2026), we develop a stylized model in which two manufacturers choose when to adopt a standard that becomes compulsory at a known date. Two mechanisms emerge. Under competition, a manufacturer that launches compliant trucks early can attract the carriers that value fuel savings most, which gives each firm an incentive to move ahead of its rival. Within a cartel, the firm that waits loses customers and would have to be compensated. Without such transfers, the legal deadline, known and

verifiable by all, offers a natural reference point for the agreement, even though the dates actually agreed did not always coincide with it. This framework also points to a more refined counterfactual, in which the identity of the first adopter, the adoption dates and market shares would follow from the firms' choices rather than being set in advance.

These mechanisms raise questions for competition enforcement and environmental regulation. A regulatory timetable announced well in advance, usually seen as a strength, may also make coordination easier. Because an agreement on timing may leave prices and quantities largely unaffected, it may escape screening tools based on prices. Better accounting for the timing of technology adoption could help authorities detect such agreements and take their environmental consequences into account.

Limitations call for caution. Firstly, average emission factors and allocation by highway length abstract from heterogeneity in traffic intensity, driving patterns and non-exhaust emissions. Secondly, only infant respiratory admissions are counted, so the health burden quantified is a subset of the overall impact. Lastly, the theoretical mechanisms are derived in a stylized duopoly and are not used here to produce quantitative estimates. They do not capture the heterogeneity of a multi-firm market or the full range of possible regulatory sequences.

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