

PROOF OF COMMON IMPACT IN ANTITRUST
LITIGATION: THE VALUE OF REGRESSION ANALYSIS

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INTRODUCTION

The class certification stage in private antitrust cases involves large and discontinuous stakes for the parties involved. Failure to have a proposed class certified “may sound the ‘death knell’”¹ of a class action lawsuit as it virtually eliminates the incentive of at least some plaintiffs to pursue their claims because their potential individual damages are small relative to the costs of litigation. In contrast, certification of the class means that defendants typically face significant aggregated claims. Given the uncertainties associated with the legal process, as well as the provisions for treble damages and one-way fee shifting in favor of successful plaintiffs, class certification often results in large settlements.² Hence, the value of the damage claims and the potential deterrence associated with private antitrust enforcement generally shift dramatically with class certification outcomes.

While several factors are relevant,³ class certification in an antitrust case often turns on the plaintiffs’ ability to demonstrate impact from the alleged violation using common proof on a class-wide basis. Courts have often accepted regression as proof of common impact but have also indicated an unwillingness to rely only on the mere assertion that regression analysis is appropriate.⁴ In this light, we explore the value of regression analysis and its role in proving common impact.

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¹ *Newton v. Merrill Lynch, Pierce, Fenner & Smith, Inc.*, 259 F.3d 154, 162 (3d Cir. 2001).

² See *In re Hydrogen Peroxide Antitrust Litig.*, 552 F.3d 305, 310 (3d Cir. 2008) (“In some cases, class certification ‘may force a defendant to settle rather than incur the costs of defending a class action and run the risk of potentially ruinous liability.’” (quoting FED. R. CIV. P. 23 advisory committee’s note to 1998 amendments)).

³ Other factors determining whether a class can be certified include the numerosity of the members of the proposed class, the adequacy of protection for the interests of the members of the proposed class, and the lack of conflict among the members of the proposed class. FED. R. CIV. P. 23(a). For further discussion, see *infra* Part II.

⁴ For example, in *Piggly Wiggly Clarksville, Inc. v. Interstate Brands Corp.*, 100 F. App’x 296 (5th Cir. 2004) (per curiam), the Fifth Circuit “criticiz[ed] plaintiffs’ expert for simply opining that he could find a formula to calculate damages using multiple regressions, rather than offering an actual model for doing so” in affirming the district court’s denial of class certification. Gregory C. Cook &

Part I examines the differences among courts in their views concerning the use of regression analysis methods in class action antitrust cases. This discussion establishes that courts are increasingly less likely to accept expert opinions that regression methods are available to demonstrate common impact as a basis for class certification. Instead, courts are moving from a presumption in favor of plaintiffs to a more critical posture toward establishing proof of class-wide impact.

Part II reviews regression analysis as a set of tools to prove impact on a common basis. To constitute *proof*, the relevant regression estimates should demonstrate impact, identify the class members suffering impact, and provide a method to measure the extent of damages, if any. Part III focuses on the issue of regression results as *common proof* of impact in the sense of representing class-wide evidence. We introduce and explain two concepts that are useful in evaluating regression results: *macro-commonality* and *micro-commonality*. Macro-commonality signifies that the results are robust given the scope of the proposed class, whereas micro-commonality indicates that common issues predominate over individual issues. Part III concludes with two empirical analyses to explore these issues and illustrates the difference between proof of impact and common proof of impact.

Part IV considers the importance of testing for commonality in light of relevant insights from the well-developed literature on cartels. Specifically, even when (1) horizontal competitors entered into an illegal agreement and (2) the agreement impacted some purchasers, courts cannot assume that the illegal agreement impacted all purchasers because of the incentives for individual competitors to deviate from the agreement and the complexity of maintaining and enforcing horizontal agreements.⁵ Thus, even an effective agreement may result in disparate impact across some purchasers and no impact for others.⁶ This discussion underscores the importance of distinguishing between results that constitute proof of impact and those that constitute common class-wide proof of impact.

Part V offers concluding remarks on how courts should evaluate regression analyses in the context of antitrust class action claims. This Part

Charlton A. Rugg, *Tightening Trends in Antitrust Class Certification*, ANTITRUST PRACTITIONER, July 2006, at 2, 4. See also *Weisfeld v. Sun Chem. Corp.*, 210 F.R.D. 136, 143 (D.N.J. 2002) (describing expert's expected results from multiple regression analysis as a naked conclusion), *aff'd*, 84 F. App'x 257 (3d Cir. 2004).

⁵ See RICHARD A. POSNER, ANTITRUST LAW 14-15 (2d ed. 2001) (explaining that cartel members have an incentive to cheat by selling to purchasers at reduced prices); Edward J. Green & Robert H. Porter, *Noncooperative Collusion Under Imperfect Price Information*, 52 ECONOMETRICA 87, 90, 94-95 (1984) (discussing the criteria necessary for stable collusion and suggesting that even significant price fluctuations can be the result of a working cartel).

⁶ MICHAEL D. WHINSTON, LECTURES ON ANTITRUST ECONOMICS 26-38 (2006) (noting that existing literature "offers less evidence . . . than one might expect" that "preventing oligopolists from talking has a substantial effect on the price they charge").

then explains the role that macro- and micro-commonality tests can play in determining whether regression analysis constitutes a common method of proof.

I. LEGAL CONTEXT AND COURTS' WILLINGNESS TO ACCEPT
REGRESSION METHODS AS A MEANS OF PROVIDING COMMON PROOF

Section 4 of the Clayton Act provides a private right of action for anti-trust claims.⁷ The Act provides for treble damages and one-way fee shifting when plaintiffs prove that they have been impacted (i.e., injured by an anti-trust violation), thus “evinc[ing] classic tort principles of injury and damages flowing from and proximately caused by a violation of substantive law.”⁸ Even when there is evidence of a per se antitrust violation such as price fixing, which normally entails criminal prosecution under the Sherman Act, private plaintiffs⁹ must still prove individual injury proximately caused by the price fixing and measurable damages.¹⁰ These cases frequently proceed as putative class actions, with Federal Rules of Civil Procedure 23(a) and 23(b)(3) setting forth the requirements for certification of the proposed class.¹¹ The extent to which questions of law and fact are common to the claims and the extent to which these common elements predominate over individualized elements together form a critical nexus of decision making around class certification.

Courts increasingly scrutinize whether plaintiffs suffered injury by the alleged conduct at the class certification phase, with particular emphasis on whether plaintiffs can show class-wide injury using a common method of proof.¹² The Eighth Circuit defined common proof as follows:

⁷ 15 U.S.C. § 15 (2006).

⁸ Ian Simmons, Alexander P. Okuliar & Nilam A. Sanghvi, *Without Presumptions: Rigorous Analysis in Class Certification Proceedings*, ANTITRUST, Summer 2007, at 61, 61.

⁹ Civil suits often follow the announcement of a guilty plea negotiated with the U.S. Department of Justice.

¹⁰ See, e.g., *In re Hydrogen Peroxide Antitrust Litig.*, 552 F.3d 305, 311 (3d Cir. 2008); *Blades v. Monsanto Co.*, 400 F.3d 562, 566 (8th Cir. 2005); *Simpson v. Union Oil Co. of Cal.*, 311 F.2d 764, 767 (9th Cir. 1963), *rev'd*, 377 U.S. 13 (1964).

¹¹ Under Rule 23(a), plaintiffs must show that: (1) there are questions of law or fact common to class members (i.e., commonality); (2) the claims of the named plaintiffs are representative of those of other proposed class members (i.e., typicality); (3) the number of proposed class members is so large that handling the cases individually would be unworkable (i.e., numerosity); and (4) the named plaintiffs will represent the proposed class adequately (i.e., adequacy of representation). FED. R. CIV. P. 23(a). Under Rule 23(b)(3), plaintiffs must show that: (1) issues of fact and law for the class predominate over issues of fact and law affecting only individual class members (predominance); and (2) a class action is better than alternative means of judicial treatment, such as individual cases or test cases (i.e., superiority). FED. R. CIV. P. 23(b)(3).

¹² See, e.g., *Castano v. Am. Tobacco Co.*, 84 F.3d 734, 744 (5th Cir. 1996) (“Going beyond the pleadings is necessary, as a court must understand the claims, defenses, relevant facts, and applicable

The requirement of Rule 23(b)(3) that common questions predominate over individual questions “tests whether proposed classes are sufficiently cohesive to warrant adjudication by representation.” The nature of the evidence that will suffice to resolve a question determines whether the question is common or individual. If, to make a prima facie showing on a given question, the members of a proposed class will need to present evidence that varies from member to member, then it is an individual question. If the *same* evidence will suffice for each member to make a prima facie showing, then it becomes a common question.¹³

Yet courts have diverged substantially with respect to the burden plaintiffs must satisfy at the class certification stage. Much of this confusion originated from the Supreme Court’s conflicting instructions in a line of class action cases. First, in 1974 the Court held that “nothing in either the language or history of Rule 23 . . . gives a court any authority to conduct a preliminary inquiry into the merits of a suit in order to determine whether it may be maintained as a class action.”¹⁴ Then, in 1982 the Court instructed that a class action “may only be certified if the trial court is satisfied, after a rigorous analysis, that the prerequisites of Rule 23(a) have been satisfied.”¹⁵ In the wake of these rulings, lower courts have struggled to conduct a “rigorous analysis” of class certification elements without engaging in a “preliminary inquiry” about factual and legal issues that bear on the merits of the case.¹⁶

Embedded in this broader struggle was the role of expert opinion at the class certification stage. Until recently, courts outside the Fourth, Fifth, and Eleventh Circuits often undertook little to no analysis of expert opinion.¹⁷ The Third Circuit’s decision in *In re Hydrogen Peroxide Antitrust Litigation*¹⁸ in late 2008 vacated a grant of class certification and served as an important development in the trend of analyzing expert opinion at the class certification stage as part of a broader rigorous analysis.¹⁹ The Third Circuit’s decision was particularly important given its earlier decision in *Bogosian v. Gulf Oil Corp.*,²⁰ in which it held that if plaintiffs prove a nationwide conspiracy resulting in increased prices to a class of plaintiffs, “an

substantive law in order to make a meaningful determination of the certification issues.”); *In re Universal Serv. Fund Tel. Billing Practices Litig.*, 219 F.R.D. 661, 678 (D. Kan. 2004) (“[C]ommon issues do not predominate every horizontal price-fixing antitrust claim”); *Weisfeld v. Sun Chem. Corp.*, 210 F.R.D. 136, 143 (D.N.J. 2002) (“[C]ommon proof [must] adequately demonstrate[] damage to each individual.” (second and third alterations in original) (internal quotation marks omitted) (quoting *Newton v. Merrill Lynch, Pierce, Fenner & Smith, Inc.*, 259 F.3d 154, 180 n.21 (3d Cir. 2001))), *aff’d*, 84 F. App’x 257 (3d Cir. 2004).

¹³ *Blades*, 400 F.3d at 566 (emphasis added) (citations omitted).

¹⁴ *Eisen v. Carlisle & Jacquelin*, 417 U.S. 156, 177 (1974).

¹⁵ *Gen. Tel. Co. of the Sw. v. Falcon*, 457 U.S. 147, 161 (1982).

¹⁶ For a more detailed discussion of this struggle, see *Simmons, Okuliar & Sanghvi*, *supra* note 8, at 62-63.

¹⁷ *See id.* at 63.

¹⁸ 552 F.3d 305 (3d Cir. 2008).

¹⁹ *Id.* at 307.

²⁰ 561 F.2d 434 (3d Cir. 1977).

individual plaintiff could [show damages] simply by proving that the free market prices would [have been] lower than the prices paid and that he made some purchases at the higher price.”²¹ Some courts have applied this “*Bogosian Short-Cut*” to find that plaintiffs can prove impact and injury on a class-wide basis.²² After the Third Circuit’s holding in *In re Hydrogen Peroxide*, courts may no longer apply the “*Bogosian Short-Cut*” as a means of expediting a plaintiff class case based solely on the existence of collusive behavior.

In re Hydrogen Peroxide is further evidence of the trend that has emerged following the Supreme Court’s 1997 ruling in *Amchem Products, Inc. v. Windsor*²³ that “lower courts must take a ‘close look’ at the predominance and superiority factors in Rule 23(b)(3).”²⁴ Since *Amchem*, courts have gravitated toward an emerging consensus that: (1) plaintiffs must show that the Rule 23 requirements are met by a preponderance of the evidence; and (2) the district court must rigorously examine whether the plaintiffs have met these requirements. Review at the class certification stage may include issues related to the merits of the case and competing expert testimony, as well as a requirement that the plaintiffs submit a methodology that can show class-wide impact using common proof in the context of the case.²⁵ For example, the Third Circuit explained that “the court’s obligation to consider all relevant evidence and arguments extends to expert testimony, whether offered by a party seeking class certification or by a party opposing it.”²⁶

As a result, courts are increasingly skeptical of experts who do not offer a functioning model tailored to the facts of the case.²⁷ This trend has manifested itself in a series of cases in which plaintiffs tendered various regression models. For example, in *Freeland v. AT & T Corp.*,²⁸ the district

²¹ *Id.* at 455.

²² *See, e.g., In re Microcrystalline Cellulose Antitrust Litig.*, 218 F.R.D. 79, 87, 89-93 (E.D. Pa. 2003) (following *Bogosian* and granting plaintiffs’ motion for class certification); *In re Mercedes-Benz Antitrust Litig.*, 213 F.R.D. 180, 188-89 (D.N.J. 2003) (same); *In re Linerboard Antitrust Litig.*, 203 F.R.D. 197, 208 (E.D. Pa. 2001) (same), *aff’d*, 305 F.3d 145 (3d Cir. 2002); *In re Art Materials Antitrust Litig.*, 38 Fed. R. Serv. 2d 1509, 1513 (N.D. Ohio 1983) (same); *Rental Car of N.H., Inc. v. Westinghouse Elec. Corp.*, 496 F. Supp. 373, 381-82 (D. Mass. 1980) (same).

²³ 521 U.S. 591 (1997).

²⁴ Simmons, Okuliar & Sanghvi, *supra* note 8, at 63 (quoting *Amchem*, 521 U.S. at 615). This reassessment accelerated when “the standard in Rule 23 for ruling on class certification was changed in 2003 from ‘as soon as is practicable’ to ‘at an early practicable time.’” *Id.* (quoting FED. R. CIV. P. 23(c)(1)(A)).

²⁵ Cook & Rugg, *supra* note 4, at 2; Simmons, Okuliar & Sanghvi, *supra* note 8, at 61.

²⁶ *In re Hydrogen Peroxide Antitrust Litig.*, 552 F.3d 305, 307 (3d Cir. 2008). A good example of a court scrutinizing expert testimony, even when it overlaps with the merits, is *Behrend v. Comcast Corp.*, 264 F.R.D. 150, 156-91 (E.D. Pa. 2010). For summaries of recent rulings in the circuit courts, see Appendix A.

²⁷ *See* Cook & Rugg, *supra* note 4, at 2.

²⁸ 238 F.R.D. 130 (S.D.N.Y. 2006).

court found serious defects in the expert's proposed methodology, explained that his regression model was "so incomplete as to be inadmissible," and denied class certification.²⁹ In *Piggly Wiggly Clarksville, Inc. v. Interstate Brands Corp.*,³⁰ the Fifth Circuit likewise denied class certification and criticized plaintiffs' expert for claiming but not demonstrating that he could use regression analysis to calculate damages because he did not explain how he would model certain factors.³¹ The Second Circuit went a step further with respect to the requirement of conducting a rigorous analysis in *Cordes & Co. Financial Services v. A.G. Edwards & Sons, Inc.*³² After vacating the district court's denial of class certification and rejection of plaintiffs' expert submission, the Second Circuit instructed the district court to push the experts to conduct dry runs of their models using the facts of the case.³³ More recently, the Third Circuit continued this trend by vacating the district court's order certifying the class in *In re Hydrogen Peroxide* and finding plaintiffs' and defendants' expert analyses—including a regression analysis tendered by plaintiffs' expert—"irreconcilable."³⁴ Despite plaintiffs' expert's representation that "sufficient reliable data" existed for the regression analysis,³⁵ defendants' expert countered that the analysis would need to "incorporate a multitude of different 'variables'" given industry-specific facts, thus rendering common proof impossible.³⁶

As courts pay closer attention to the methodologies and arguments offered by economic experts, including assessing more critically the scientific validity and applicability of proposed regression analyses to the individual facts of each case, an understanding of regression's strengths and weaknesses in the class certification context becomes more important. In the next two sections, we address the general value of regression methods in yielding proof of impact and then turn to the issues concerning commonality of proof.

²⁹ *Id.* at 147, 149 (internal quotation marks omitted) (quoting *Bazemore v. Friday*, 478 U.S. 385, 400 (1986)) (observing that the proposed regression analysis omitted important variables and that plaintiffs' explanations for doing so were not persuasive).

³⁰ 100 F. App'x 296 (5th Cir. 2004) (per curiam).

³¹ *Id.* at 299-301 ("Multiple regression analysis is not a magic formula. It is simply a mathematical tool . . . which may or may not yield statistically significant results.").

³² 502 F.3d 91 (2d Cir. 2007).

³³ *Id.* at 107 ("If the plaintiffs' single formula can be employed to make a valid comparison between the but-for fee and the actual fee paid, then it seems to us that the injury-in-fact question is common to the class. Otherwise, it poses individual ones. The district court did not determine which expert is correct. We leave this question for it to resolve on remand.").

³⁴ *In re Hydrogen Peroxide Antitrust Litig.*, 552 F.3d 305, 314, 325 (3d Cir. 2008).

³⁵ *Id.* at 313.

³⁶ *Id.* at 314.

II. PROOF OF IMPACT

In the context of suspected cartel pricing behavior, economists use regression methods to identify various non-collusive factors that may affect the prices paid by members of the proposed class and isolate the potentially common effect of a horizontal conspiracy. In this way, regressions may provide proof of impact despite differences in prices resulting from non-collusive factors, some of which may be specific to individual members of the proposed class.

The most important econometric requirement in this context is that the relevant regression estimates isolate and quantify the effect of the alleged antitrust violation.³⁷ A regression intended to prove the causal effect of a price-fixing conspiracy on prices must reliably estimate the unobserved prices that would have been paid but for the alleged conspiracy.³⁸ To achieve these objectives, the *regression specification*—which refers to the variables included in the regression and the form of the regression—must reflect the facts in the case and the specifics of the industry at issue.³⁹ For example, prices from a benchmark market may serve as a proxy for but-for prices. Similarly, prices paid before (and/or after) the alleged violations may be used in a before-and-after analysis.⁴⁰

Generally, neither competitive benchmarks nor “before” or “after” periods will be identical to an allegedly collusive market. One would not necessarily expect prices in a “before” period to be accurate proxies for current but-for prices due to many factors, including potential changes in demand and supply conditions, product features, and inflation. Any method for identifying a potential conspiracy, including regression, should control for differences that are unrelated to the alleged collusion.⁴¹ When a well-specified regression controls for the differences, the results may indicate impact. However, if the set of control variables is incomplete or poorly defined, experts should explain their choice of the particular factors included, as well as the reasons for not including others.⁴² This requires a thorough

³⁷ See Jonathan B. Baker & Daniel L. Rubinfeld, *Empirical Methods in Antitrust Litigation: Review and Critique*, 1 AM. L. & ECON. REV. 386, 429-30 (1999).

³⁸ See *id.* at 392-98.

³⁹ See, e.g., *id.* at *passim*; Franklin M. Fisher, *Multiple Regression in Legal Proceedings*, 80 COLUM. L. REV. 702, 729 (1980).

⁴⁰ For example, in an antitrust case in which plaintiffs claimed defendants conspired to fix initial public offering underwriting fees, plaintiffs' expert asserted that he could derive a common formula for calculating but-for fees by “establishing a benchmark fee from a set of prices paid in temporal or geographic isolation from the conspiracy” and use regression analysis to “isolate the ‘explanatory variables’ that influence the benchmark fee.” *Cordes & Co. Fin. Servs. v. A.G. Edwards & Sons, Inc.*, 502 F.3d 91, 94, 97 (2d Cir. 2007) (quoting Bamberger Decl. ¶¶ 9, 16).

⁴¹ These controls could include inflation, seasonality variables, and changes in the prices of substitutes and complements.

⁴² One treatise states:

understanding of the industry, pricing practices, and relevant institutional factors. Failure to consider and include factors that influence prices and are positively correlated with the alleged conspiracy will yield an overestimate of the impact of the alleged conspiracy. In basic scientific terms, the regression would fail to isolate the effect of the conspiracy from other unrelated factors and, as a result, concatenate the two effects and fail to provide proof of impact.

To further explore the issue of commonality in the context of class certification, it is necessary to lay some groundwork on alternative regression specifications. In its most basic form, a regression equation may describe a linear relationship between two or more variables:

$$Y_i = a + bX_i + e_i. \quad (1)$$

In equation (1), Y is the dependent variable (e.g., price), a is the constant term that does not vary with different values of Y , X is the independent or explanatory variable (e.g., product characteristics), b is the coefficient that captures the relationship between X and Y , and e is the error term. The subscript i denotes a transaction and indicates that the regression will capture the relationship between X and Y across multiple transactions. The implicit assumption is that X may influence Y .

In a non-antitrust setting, if X measured a person's job experience and Y measured compensation, the equation would specify a relationship in which compensation depends on experience based on observations on i individuals. Estimation of the regression yields the best fit of the available data and thus identifies the central (or average) tendency in the relationship between job experience and compensation. The effect of additional years of experience on compensation is reflected in the estimated coefficient, b , whose positive value might indicate that compensation increases with experience.⁴³ Whether the estimated regression is a good fit would be reflected in the overall explanatory power of the regression, which in this context

Plaintiffs using regressions for class certification purposes will generally try to control for various customer attributes. But these attributes are not always accurately captured in regressions because of data limitations or problems with the specification of the regression. For example, if individual customers qualify for volume discounts at some times but not others, the regression would need to take this into account. Similarly, if delivery charges are added to prices at some times and not others or the types of products offered and purchased change over time, it would be improper to use time-series data or models that do not reflect these variations over time. When there is substantial variation in fundamental aspects of the price data of this type, individual inquiries are necessary to understand the factors that shaped the prices that specific customers paid.

ABA SECTION OF ANTITRUST LAW, *ECONOMETRICS*: 224 (2005).

⁴³ This statement is conditional on how one measures compensation and job experience. When comparing the values of estimated coefficients across regressions, one needs to ensure that he is measuring the underlying variables in the same way.

will be determined in part by the statistical significance of b .⁴⁴ In general, a regression is more likely to yield statistically significant coefficients when there is more systematic variation in the underlying data and when there are more observations in the data set. However, the exclusion of relevant variables, such as gender, age, or other individual characteristics, may result in a misinterpretation of the relationship between compensation and experience.

We now turn to a similar example in an antitrust context where the objective is to assess how product prices in New York compare to those in San Francisco. One might specify a regression similar to equation (1) in which the dependent variable Y represents price and the independent variable X represents an indicator or “dummy variable”⁴⁵ that equals 1 for New York and 0 for San Francisco. Estimating this version of equation (1) is equivalent to calculating the average price in New York and the average price in San Francisco and then computing the difference between the two averages. The intercept term a identifies the average price in San Francisco, whereas the coefficient b represents the difference between the average price in New York and the average price in San Francisco. Finally, the sum of a and b represents the average price in New York.

Regression analysis in this context is particularly valuable because it can facilitate a more sophisticated comparison of averages. Suppose, for example, that the data covered sales of two versions of the product, a basic and an enhanced version. *Multivariate* regression analysis can account for additional factors:

$$Y_i = a + bX_i + cV_i + e_i. \quad (2)$$

Compared to equation (1), this specification adds another dummy variable, V , which equals 1 for the enhanced product and 0 for the basic product, and thereby provides an estimate of the difference between the average prices in New York and San Francisco after controlling for differences in product mix across the two cities.

This case is highly relevant to the issue of common proof in class certification. Equation (2) only specifies a single coefficient, b , to capture the effects of location on prices. Hence, this specification embodies the assumption that the price differences between New York and San Francisco

⁴⁴ Note that in a multivariate regression (i.e., a regression that includes more than one explanatory variable) the overall explanatory power of a regression and the statistical significance of the estimated coefficients may not be closely related. In particular, when the explanatory variables are themselves highly collinear, their estimated coefficients will often be less statistically significant even though the overall explanatory power of the regression may be high.

⁴⁵ A dummy variable indicates the presence or absence of some categorical effect (in this example, location) that may affect the outcome. Use of dummy variables allows a single regression equation to provide information about both locations by switching the location variable on or off.

are the same for the basic and enhanced products. Indeed, this specification actually imposes the *condition* that the price differences between the two products be the same. Put in legal terms corresponding to the framework associated with class certification, *this specification assumes, rather than tests, that there is a common effect of location on the price differences between the two products*. When used to investigate the effects of a conspiracy affecting the sales of both product versions in New York, with San Francisco as a potential benchmark city, the specification of this type of regression would embody the assumption of commonality across the two product types. The regression would produce an *average* of the price differences between the two products across the two cities; it could not reveal whether there were important differences between the price effects of location on the prices of the basic and enhanced products.

In general, an expert opining on class certification issues should test, rather than assume, that the effects of an alleged violation are common across products and other relevant dimensions. A more sophisticated regression model might do so. Equation (3), below, is identical to equation (2) except that it includes an interaction term, which is constructed by multiplying the city indicator (X) by the product indicator (V) and equals 1 for the prices of the enhanced version sold in New York:

$$Y_i = a + bX_i + cV_i + d[X_i * V_i] + e_i .(3)$$

With this specification, an economic expert can separately estimate the differences in the average prices for the enhanced and the basic products across the two cities in a single regression. The coefficient b represents the difference in average prices for the basic version, and the sum of the coefficients b and d represents the difference in average prices for the enhanced version. If d is significantly different from zero, the effect of the conspiracy is different across the two products (assuming that nothing other than the conspiracy explains differences across the two cities). A single estimate of the conspiracy's effect on the products, as would have been generated by equation (2), would not have accurately captured the conspiracy's impact. In our terminology, this result would have failed the macro-commonality test. Specifically, such a simplified test could aggregate injured and uninjured members of the proposed class with no method to distinguish between the two. If, however, equation (3) yielded the result that d was equal to zero, then the assumption of macro-commonality across product types may prove correct. We explore these issues further in Section III.A.

Even when a regression result supports the assumption of macro-commonality across product types, an economic expert can explore whether the central tendency in the data applies to individuals by evaluating two results: (1) how well the regression predicts results for individuals; and (2) how well the regression identifies the effect of the alleged conspiracy on individuals within the proposed class. With one independent variable, this

would involve identifying the prices paid by individual class members for each type of product purchased. If a regression predicts precisely the outcome of interest (here price) for most individual cases, then the estimated relationship would, in our terminology, meet the micro-commonality test. In other words, regression results from equation (1) may precisely identify the individual price each individual member of the class pays across the two cities. If, however, factors such as location of purchase and volume purchased are highly influential and omitted from the regression, the price predicted by the regression based on product type alone could deviate substantially from the observed price. Similarly, even if the regression includes all relevant factors and if the average impact of the city on price masks great variation across members of the class, then the estimated relationship would not meet the micro-commonality test. We explore these issues further in Section III.B.

III. COMMONALITY

As indicated by the preceding section, regression methods yield central tendencies. A well-specified regression might provide proof that prices in one market are higher on average than prices in another market. Class certification, however, requires inquiry into potential differences among proposed class members, not merely evidence of average impact.⁴⁶ An expert should prove that impact as estimated by regression does not combine cases of injury to individual class members with cases of non-injury.⁴⁷ In this section, we focus first on macro-commonality, which relates to the relevance of regression results across proposed subclasses (e.g., different time periods or different products). This section then explains micro-commonality, which focuses on the relevance of regression results for individual members of the proposed class.

A. *Macro-Commonality*

In this subsection, we focus on whether a regression offers common proof of impact in the sense of providing valid results for various *subsets* of observations. Hence, macro-commonality tests attempt to confirm impact from collusive behavior across various subsets of the proposed class. A test

⁴⁶ See *In re Hydrogen Peroxide Antitrust Litig.*, 552 F.3d 305, 311 (3d Cir. 2008); *Newton v. Merrill Lynch, Pierce, Fenner & Smith, Inc.*, 259 F.3d 154, 172 (3d Cir. 2001); *Freeland v. AT & T Corp.*, 238 F.R.D. 130, 142 (S.D.N.Y. 2006).

⁴⁷ See *Hydrogen Peroxide*, 552 F.3d at 311 (“[I]ndividual injury (also known as antitrust impact) is an element of the cause of action; to prevail on the merits, every class member must prove at least some antitrust impact resulting from the alleged violation.”).

of macro-commonality focuses on re-estimating the regression to determine whether the estimates of impact continue across subgroups of the proposed class. If a statistically significant result for the proposed class as a whole does not hold up across subgroups, then the original regression by itself cannot demonstrate common impact.

We explore the issue of macro-commonality using data on actual rum prices across cities in the United States.⁴⁸ The analysis begins with the presumed allegation that manufacturers colluded to raise prices in five cities: Tampa, New York, Denver, Miami, and Boston. Another set of seven cities—Atlanta, Chicago, Kansas, Miami, Minneapolis, San Diego, and San Francisco—constitutes a valid competitive benchmark. Could a regression analysis establish common proof of impact to an indirect purchaser class of rum consumers that is suing for damages? Naturally, the purpose of this regression would be to prove that the prices paid by rum purchasers in the cities subject to the collusive behavior were higher than the prices paid in the benchmark cities.⁴⁹

We first perform a regression analysis of the retail prices on an indicator variable, which equals 1 for the allegedly collusive cities and 0 for the benchmark cities, as well as other independent variables controlling for:

- (1) Year (1996 through 2000);
- (2) Store type (drug store, discount store, liquor store, and Hispanic neighborhood store);
- (3) Brand (Bacardi, Castillo, Captain Morgan, Malibu, Myers's, and Ronrico);
- (4) Liquor type (flavored, gold, light, and proof 151); and
- (5) Product size (375 ml., 750 ml., 1,000 ml., and 1,750 ml.).⁵⁰

⁴⁸ The data is provided by The Nielsen Company and covers weekly dollar and quantity sales volume for forty product specifications of rum liquor from 1996 through 2000. The data is granulated by week, product specification, location, and store type. Quantity units are given in cases of the given specification. Average price per liter is calculated as sales divided by quantity and then divided by the number of liters in a case of the given specification. Case liters are determined by the bottle volume of the specification (e.g., a case of 750-milliliter bottles will contain a total of nine liters or twelve bottles). This price per liter is then adjusted to exclude state sales and excise taxes. Tax data is from FED'N OF TAX ADM'RS, STATE TAX RATES ON DISTILLED SPIRITS (2010), <http://www.taxadmin.org/fta/rate/liquor.pdf>. Price per liter net of tax is adjusted to reflect cross-location differences in food price levels. This data comes from Bettina H. Aten, *Report on Interarea Price Levels* 12-13 (U.S. Bureau of Econ. Analysis, U.S. Dep't of Commerce, Working Paper No. 2005-11, 2005), available at <http://www.bea.gov/papers/pdf/InterareaPriceLevels.pdf>. Note that this data reflects inter-area levels of food (not food and beverage) prices as of 2003. The Bureau of Labor Statistics, Aten's data source, does not publish inter-area figures.

⁴⁹ For ease of exposition, the regression results presented here are based on a simple least squares regression with no effort to correct for potential statistical issues such as heteroskedasticity, autocorrelation, normality of the distribution of the dependent variable, etc. As such, it should be seen only as an example of a potential approach to regression analysis.

⁵⁰ Thus, in equation form, the regression is:

$$P_{it} = \alpha + b_1 Collude_i + b_2 StoreType_i + b_3 Brand_i + b_4 RumType_i + b_5 BottleSize_i + b_6 Year_t + e_{it}$$

The estimated coefficient for the indicator variable for the cities subject to the alleged conspiracy is \$1.10. This estimated coefficient is statistically significant.⁵¹ Thus, accounting for store type, rum type, bottle size, and year, the average price per liter of rum was approximately \$1.10 higher in the allegedly collusive cities than in the benchmark cities. Table 1 reports the regression results.

Assuming that the regression result is valid,⁵² we now evaluate whether it meets the macro-commonality test. The \$1.10 coefficient on the conspiracy variable in the original regression indicates only that prices were higher *on average* in the five cities subject to the alleged collusion as compared to the seven benchmark cities. Should this finding be viewed as common proof of illegal overcharges? Several findings will help answer this question:

- (1) whether the conspiracy led to higher prices in each of the five collusive cities;
- (2) whether the conspiracy led to higher prices on the different branded products subject to the conspiracy; and
- (3) whether the conspiracy led to higher prices in each year during the collusive period.

In other words, did the alleged collusive activity impact the members of the class, irrespective of the year of their purchase, the type of product purchased, or the city in which they purchased the product?

To investigate the first factor, we can estimate an alternative regression with indicator variables for each of the allegedly collusive cities rather than a single indicator for all five cities. This regression, as reported in Table 2, yields the following estimated coefficients, each of which is statistically significant: -\$0.99 for New York; \$0.71 for Boston; \$0.98 for Miami; \$1.36 for Denver; and \$3.47 for Tampa. These results indicate a lack of commonality across cities, with differences between the allegedly collusive cities and the set of benchmark cities that range from -\$0.99 to \$3.47. The original regression, with the single coefficient of \$1.10, dramatically underestimates the alleged conspiracy's impact on average prices in Tampa and dramatically overestimates the impact in New York. These results suggest potential inconsistencies regarding the allegations of conspiracy. For instance, one may question why the alleged overcharges differ so greatly across markets or why but-for prices are higher than the actual prices in New York. The presence of such questions indicates a lack of commonality across the data. Given this lack of commonality, a natural step would be to

Here, *i* refers to the transaction characteristics that correspond to each observed price (i.e., city, store type, brand, liquor type, and product size).

⁵¹ Statistical significance here indicates whether the hypothesis that a given estimate is equal to zero can be rejected at the five-percent level of confidence.

⁵² For example, the data reflects the facts set forth in the evidentiary record.

estimate the alleged conspiracy's effects on prices separately for each of the allegedly collusive cities.⁵³

Regarding the issue of macro-commonality across branded products, the regressions to this point implicitly assume that overcharges are common across products. In principle, however, the alleged conspiracy could have yielded different overcharges across products due to differences in manufacturers' adherence to the terms of the conspiracy. In addition, "pass-on" behaviors by distributors and retailers could vary by brand because of varying demand characteristics.⁵⁴ To further investigate macro-commonality, we modified the regression to estimate *brand-specific overcharges by city*. In the case of Boston, where the city-specific overcharge per liter had been estimated at \$0.71, the estimated overcharges by brand are as follows: \$0.56 for Bacardi; \$1.07 for Castillo; \$1.61 for Captain Morgan; \$0.55 for Myers; and -\$0.26 for Ronrico. As Table 3 indicates, only the coefficient for Castillo is statistically significant. These results indicate, therefore, that the city-specific overcharge is not common and masks the lack of statistically significant results for all but one brand.⁵⁵ The patterns are similar across cities, as some brands show statistically elevated prices, while others do not.⁵⁶

To investigate commonality across years, a regression accounts for year-specific effects. These results are reported in Table 4 for the Castillo brand in each city. Focusing on Boston again, the overcharge is \$1.38 in 1996; \$1.49 in 1997; \$1.20 in 1998; \$0.87 in 1999; and \$0.50 in 2000. Of these, only the 1996 result is statistically significant. The lack of a statistically significant finding in most years may result from insufficient data, but it does indicate that the original regression does not meet the macro-commonality test.

⁵³ Further analysis raises additional doubts in this regard. Suppose that the proposed benchmarks were limited to three cities—Atlanta, Kansas City, and San Diego—and the others were dropped. When the modified regression is estimated, the coefficient on the conspiracy indicator drops to a statistically insignificant value (-\$0.12). Alternatively, if the proposed benchmark cities are San Francisco, Chicago, Los Angeles, and Minneapolis, the coefficient increases to \$2.64 and is statistically significant. Clearly, the choice of benchmark matters, but the regression model itself provides no assistance here—there is no statistical test to differentiate between the three alternative specifications described above. It is important not to confuse the existence of specification tests with the ability to test between alternative competitive benchmarks. For example, a specification test that compares the fit of the three alternative specifications would have no relevance to the choice of competitive benchmark.

⁵⁴ William H. Page, *The Limits of State Indirect Purchaser Suits: Class Certification in the Shadow of Illinois Brick*, 67 ANTITRUST L.J. 1, 12 (1999) ("[T]he problem of proof in an indirect purchaser case is intrinsically more complex, because the damage model must account for the actions of innocent intermediaries who allegedly passed on the overcharge.").

⁵⁵ For Myers's, Bacardi, and Captain Morgan, the estimated difference is greater than zero but not statistically significant, precluding us from rejecting the hypothesis that the true difference is zero. The regression estimates are not precise enough to lead to a meaningful conclusion.

⁵⁶ The lack of statistical significance in some cases may be due to small sample sizes, but it should be noted that each of the regressions uses hundreds of observations.

This empirical analysis illustrates potential challenges associated with using regression in the class certification context, even when the data reflect an overall impact (i.e., a \$1.10 overcharge across all cities, brands, and years). When we estimated city-specific differences, the overcharges ranged from -\$0.99 to \$3.47. Furthermore, when we estimated brand-level differences for one city, Boston, the overcharges ranged from -\$0.26 to \$1.61. Neither of these results was statistically significant, illustrating how the city-level estimate of \$0.71 for Boston masks considerable brand-level differences. When we estimated year-level differences for one brand, Castillo, in Boston, the overcharges ranged from a statistically insignificant \$0.50 to a statistically significant \$1.49, illustrating how the city-brand level estimate of \$1.07 for Castillo masks considerable temporal differences.

Even though the original regression did not constitute proof of common impact given the scope of the proposed class, one might argue that the analysis presented above demonstrates the existence of a common method to demonstrate impact in some cases. From the results in Table 4, for example, one could conclude that purchasers of the Castillo brand in Tampa from 1997 until 2000, in Denver in 1996, and in Boston in 1996 suffered impact from the collusive activity. This line of argument leads, of course, to assessing the value of regression methods in identifying potential subclasses. In a similar vein, one might argue that regression methods could distinguish those in a certified class who suffered impact from collusive activity from those in the class who did not.

The issues associated with using regression for more limited purposes are similar to those associated with using regression to provide common proof of impact on a class-wide basis. In principle, a disaggregated analysis may provide proof of common impact within a subgroup. Our exploration of the macro-commonality issues in this setting, however, points to some hazards in relying on regression for more limited purposes. The empirical inquiry demonstrates that even at fine levels of disaggregation, impact on prices is not common. Further disaggregation (e.g., by size of product) could reveal that some purchasers of Castillo brand rum in 1997 in Tampa suffered impact while others did not. Such findings may lead to the conclusion that determining impact will require an individualized inquiry. The more general implication is that the expert must rely on relevant theory and facts to specify a proper regression and then test whether the average result yielded by the regression reflects commonality across various levels of aggregation, or whether it masks individualized variation not prone to analysis using regression methods.

B. *Micro-Commonality*

To explore the issue of micro-commonality, we analyze a small masked data set from an alleged price-fixing conspiracy in which a regression demonstrates that prices paid by purchasers as a group increased fol-

lowing a collusive price increase announcement. These results do not constitute *common* proof unless they prove impact to *individual members* of the proposed class. This analysis illustrates in vivid terms that coefficients generated by regression methods reflect the central tendencies in data and, even when the results are statistically significant, the findings may not mean that the common result predominates over individual-specific factors.

This alleged conspiracy concerns the effects of announced price increases by four sellers (S-1 through S-4) on purchases by six customers (C-1 through C-6). S-1 and S-2 sell two products while S-3 and S-4 sell only one product. Announced price increases were \$0.03 for S-1's two products and \$0.05 for all other products. The effective dates of the announced price increases are shown in Table 5, which also lists the thirty transactions and includes entries for invoice price, customer, seller, product, and whether the transaction took place before or after the effective date of the announced price increase.

Within the data reported in Table 5, there are natural groupings of observations for which the identities of the customer, the seller, and the product are the same. The first five observations, for example, concern C-1's purchases of Product 1A from S-1. The first three of these transactions occurred before S-1's February 1, 1999 price increase announcement. The fourth and fifth occurred after this announced increase. The indicator variable captures this timing difference: the "Price Announcement" variable in the last column of Table 5 takes on the value 0 if the transaction is before the announcement and 1 if it is after the announcement.

Multiple regression analysis of this data would find the best fit and, in principle, estimate the effect of the price announcements on actual prices paid by the six customers while also accounting for other factors, such as the differing buying powers of the customers.⁵⁷ More precisely, the coefficient on the Price Announcement variable would estimate the average change in transaction prices after announced price increases went into effect. If the underlying data shows transaction price increases after the announcements, the estimated coefficient will be positive. If transaction prices stay the same following the announcements, the estimated coefficient will be zero.

⁵⁷ Given that the allegations concern a conspiracy to increase prices whereby sellers would publicly announce price increases and then implement them, several specification questions arise. Most fundamental is the question of whether it is correct to interpret the results of the regression as evidence that the alleged conspiracy caused the price announcements and, in turn, resulted in illegal overcharges. The regression might be premised on the assumption that the conspiracy existed and that it resulted in announced price increases that were higher than they otherwise would be. A related issue concerns timing and, more specifically, the dating of the beginning of the alleged conspiracy. More general issues concern whether the effects of the price announcements have been isolated from other influences such as changes in the prices of substitutes and complements and overall demand for the products in question.

Table 6 reports regression results. Along with the Price Announcement variable, the regression includes dummy variables for the six customers.⁵⁸ The estimated impact of the price announcements is slightly above \$0.02. This figure is statistically significant and corresponds to about two percent of the average prices before the effective dates of the announced price increases. Therefore, assuming that the announced price increases were the result of an illegal conspiracy, the regression yields statistically significant proof of impact.

However, these regression results and the coefficient on the Price Announcement variable do not reflect common proof of impact. Referring back to the transaction data in Table 5, and even assuming that no relevant factors have been omitted, the regression results reflect an averaging of cases of impact and no impact from the point of view of individual customers. Focusing on observations 6 through 10 and observations 16 through 20, actual prices paid by customers C-2 and C-4 do not increase following the effective dates of the price announcements. When transactional data exhibit these patterns, it is likely that a multiple regression analysis will serve only to mask the underlying mix of impact and no impact cases.

This empirical inquiry illustrates how regression results that offer proof of impact can fail the micro-commonality test: the central tendency estimated by regression methods may not apply to particular observations. Stated in terms of class certification, statistically significant indications of impact do not necessarily apply to individual members of the proposed class. While inspection of the underlying data was sufficient in this case to indicate the problem, the more general approach to testing for micro-commonality requires (1) an evaluation of the overall power of the regression and the strength of the results concerning the alleged violation and (2) a comparison of predictions from the regression for specific individuals with actual prices.

Goodness of fit and the economic and statistical significance of the estimated impact will provide a general indication of the strength of the regression results and the likelihood that common factors predominate over individual factors. The second step, which compares what the regression predicts for individuals with the actual results, provides a more concrete sense of whether the regression represents common proof. For example, in the context of a price-fixing allegation, if there is a substantial frequency of cases in which the actual prices paid by customers are equal to or far below the predicted but-for prices, then the regression results should not be interpreted as common proof of impact.

⁵⁸ C-1 is the omitted variable from the set of customer dummy variables. Given the small size of the data set, it is not possible to include either product dummy variables or seller dummy variables with the customer dummy variables. Limitations on the number of dummy variables are common and arise when data sets are small and when the actual observations do not include sufficient variation across the categories implied by the dummy variables.

C. *Implications of Macro- and Micro-Commonality Tests*

Courts across the country have applied different standards for class certification. Not surprisingly, given this context, economists have not offered or developed a consistent approach to evaluating regression as a test for the existence of a common method of proof. One implication of our analysis is that the question whether regression analysis is useful in the class certification context goes well beyond whether data is available and whether a regression can be specified to accurately yield a single, non-zero coefficient associated with the collusive behavior at issue. Tests of macro-commonality and micro-commonality provide necessary steps to ensure that a regression analysis constitutes a common method of proof.

How should these tests be implemented? Economists should generally test for macro-commonality first, as its results provide guidance for the types of micro-commonality tests that will be most helpful. In the absence of a macro-commonality test, micro-commonality tests may become akin to individualized inquiry, as the economist will not know which groups of potential class members should be selected for micro-commonality testing.

IV. THE ECONOMICS UNDERLYING THE ISSUE OF COMMONALITY

As the empirical inquiries in Part III have demonstrated, proof of impact from a regression does not constitute proof of common impact. On the contrary, economic theory and empirical evidence suggest that antitrust violations are likely to result in a range of impacts—from none for some plaintiffs to significant impact for others—and thus underscore the importance of distinguishing between proving impact and proving *common* impact.

While there is no doubt that collusive agreements can yield greater profits for cartel members,⁵⁹ Nobel laureate George J. Stigler demonstrated in *A Theory of Oligopoly* that horizontal agreements are inherently unstable and, as a consequence, are fundamentally different from competitive equilibria.⁶⁰ Indeed, the same incentives associated with successful horizontal agreement and impact (i.e., the transfer of surplus from customers to sellers) will encourage the horizontal competitors to deviate from the agreement, resulting in less than full and consistent impacts or even zero impact.⁶¹

The deviations from an agreement, either in the form of price or non-price competition, are termed “cheating” in the industrial organization lit-

⁵⁹ See George J. Stigler, *A Theory of Oligopoly*, 72 J. POL. ECON. 44, 44 (1964).

⁶⁰ *Id.* at 49-56.

⁶¹ *Id.* at 46.

erature.⁶² The term—somewhat unfortunate in that it suggests irregularity—simply means competition despite the presence of a horizontal agreement.⁶³ Where one observes the absence of an effective, illegal horizontal agreement, one of the principal reasons for such absence is that competitive divergences from any potential agreement (i.e., cheating) make the agreement unworkable. When competitors reach horizontal agreements, the empirical evidence suggests that many of the agreements work intermittently or incompletely.⁶⁴

Success of a horizontal agreement depends in part on the ability of cartel members to deter those who deviate from the agreement.⁶⁵ Such deterrence requires not only monitoring to detect cheating, but also imposing penalties on those who try to gain additional sales through undercutting the collusively agreed-upon price. Unless the penalty can somehow be targeted to impose costs only on the cheater, the imposition of penalties may end the agreement itself.⁶⁶ The information problems of cartel enforcement identified by Stigler inspired other work, most notably by Professors Edward J. Green and Robert H. Porter in 1984, which cast Stigler's problem in the context of repeated games of incomplete information.⁶⁷ This literature has produced an extensive list of factors that facilitate a collusive agreement, including, for example, product homogeneity and market concentration.⁶⁸ When these factors are present, cartel members may have limited incentives to deviate from the agreement, which will therefore become more stable.⁶⁹

Economic analysis of cartels is a substantial component of the broader literature on oligopoly. The implications of cheating on how regression methods affect antitrust litigation are still incomplete, however. In our

⁶² See, e.g., Jonathan B. Baker, *Mavericks, Mergers, and Exclusion: Proving Coordinated Competitive Effects Under the Antitrust Laws*, 77 N.Y.U. L. REV. 135, 138 (2002).

⁶³ See *id.* at 158-59.

⁶⁴ Stigler, *supra* note 59, at 46; see also Andrew R. Dick, *When Are Cartels Stable Contracts?*, 39 J.L. & ECON. 241, 241-57 (1996) (exploring the legal Webb-Pomerene cartels of the early- to mid-1900s).

⁶⁵ E.g., Guy Sagi, *The Oligopolistic Pricing Problem: A Suggested Price Freeze Remedy*, 2008 COLUM. BUS. L. REV. 269, 276 (“There are three major factors necessary to form successful cooperation among firms: the ability to reach an agreement, the ability to detect cheating, and the ability to swiftly punish deviators.”).

⁶⁶ Ian Ayres, *How Cartels Punish: A Structural Theory of Self-Enforcing Collusion*, 87 COLUM. L. REV. 295, 303 (1987) (discussing how price punishment can damage a “cartel’s rank and file,” while single-firm targeted punishment may be perceived as more credible punishment).

⁶⁷ See Green & Porter, *supra* note 5, at 89-94. See generally Sagi, *supra* note 65, at 275-76 (discussing Stigler’s influence on oligopoly theory and the subsequent game theory developments).

⁶⁸ E.g., Donald S. Clark, *Price-Fixing Without Collusion: An Antitrust Analysis of Facilitating Practices After Ethyl Corp.*, 1983 WIS. L. REV. 887, 894 (1983) (identifying five structural factors which are conducive to cartel pricing, including product homogeneity and market concentration).

⁶⁹ Stability can only result in a dynamic setting with imperfect information, as it is only repeated contexts that allow credible threats of punishment and only imperfect information that gives rise to the possibility of cheating and the importance of monitoring. See Green & Porter, *supra* note 5, at 89-94.

view, the remedy is straightforward: rather than view mixes of impact and non-impact as anomalous, courts should first approach the issue of proof of common impact with a substantial inquiry into the facts of the case, especially with respect to how the alleged colluders maintained and enforced the horizontal agreement. Second, if courts employ regression methods to establish proof of impact on a common, class-wide basis, then they should also conduct the inquiries to determine whether the provisional results meet the macro- and micro-commonality tests proposed here.

V. CONCLUSION

Across most U.S. circuit courts, there is an emerging consensus encouraging scrutiny of methodologies that are intended to provide common proof of impact across proposed class members. As courts evaluate the strengths and limitations of regression analyses, it will become increasingly more apparent that they will not be able to establish class-wide impact using common proof simply because a potential regression can be specified. Regression is a useful tool for controlling potentially confounding variables, but whether regression results constitute a common method of proof requires both a well-designed regression and a testing of both macro-commonality and micro-commonality. Data availability, the selection of a benchmark, and other case-specific elements influence the quality of the regression and whether it yields reliable proof.

Our contribution here concerns the commonality of proof. Which of the two tests that we prescribe is more important? Without proof of micro-commonality, regression analysis generally will not constitute a common method of proof. If micro-commonality exists, then proof of macro-commonality increases the likelihood that regression provides a common method of proof for the entire class, as opposed to subgroups of purchasers within the class. Ultimately, testing of macro- and micro-commonality are necessary, albeit not sufficient, steps to determine whether regression analysis is likely to provide a class-wide proof of impact, as it is also necessary to examine other factors, including data availability, the existence of a benchmark period, and other case-specific elements.

APPENDIX A: JUDICIAL RECOGNITION OF REGRESSION ANALYSIS IN CLASS ACTION CERTIFICATIONS

Recent rulings in the circuit courts are consistent with this emerging consensus with respect to the use of regression. For example:

FIRST CIRCUIT
<i>In re</i> PolyMedica Corp. Sec. Litig., 432 F.3d 1, 6 (1st Cir. 2005) (quoting <i>Waste Mgmt. Holdings, Inc. v. Mowbray</i> , 208 F.3d 288, 298 (1st Cir. 2000)).
<p>The First Circuit explained:</p> <p>[W]hile <i>Eisen</i> prohibits a district court from inquiring into whether a plaintiff will prevail on the merits at class certification, it “does not foreclose consideration of the probable course of litigation,” as contemplated by <i>Falcon</i>. . . . “[A] district court must formulate some prediction as to how specific issues will play out in order to determine whether common or individual issues predominate in a given case.”</p>
SECOND CIRCUIT
<i>In re</i> Initial Pub. Offerings Sec. Litig., 471 F.3d 24, 41 (2d Cir. 2006).
<p>The Second Circuit made many determinations regarding the certification of a class in a securities litigation suit that have important corollaries to antitrust class action litigation:</p> <ol style="list-style-type: none"> (1) a district judge may certify a class only after making determinations that each of the Rule 23 requirements has been met; (2) such determinations can be made only if the judge resolves factual disputes relevant to each Rule 23 requirement and finds that whatever underlying facts are relevant to a particular Rule 23 requirement have been established and is persuaded to rule, based on the relevant facts and the applicable legal standard, that the requirement is met; (3) the obligation to make such determinations is not lessened by overlap between a Rule 23 requirement and a merits issue, even a merits issue that is identical with a Rule 23 requirement; (4) in making such determinations, a district judge should not assess any aspect of the merits unrelated to a Rule 23 requirement; and (5) a district judge has ample discretion to circumscribe both the extent of discovery concerning Rule 23 requirements and the extent of a hearing to determine whether such requirements are met in order to assure that a class action certification motion does not become a pretext for a partial trial of the merits.

THIRD CIRCUIT
<i>In re Hydrogen Peroxide Antitrust Litig.</i> , 552 F.3d 305, 307 (3d Cir. 2008).
The Third Circuit explained: <p>First, the decision to certify a class calls for findings by the court, not merely a “threshold showing” by a party, that each requirement of Rule 23 is met. Factual determinations supporting Rule 23 findings must be made by a preponderance of the evidence. Second, the court must resolve all factual or legal disputes relevant to class certification, even if they overlap with the merits—including disputes touching on elements of the cause of action. Third, the court’s obligation to consider all relevant evidence and arguments extends to expert testimony, whether offered by a party seeking class certification or by a party opposing it.</p>
FOURTH CIRCUIT
<i>Gariety v. Grant Thornton, LLP</i> , 368 F.3d 356, 365 (4th Cir. 2004).
“We conclude that, by accepting the plaintiffs’ allegations for purposes of certifying a class in this case, the district court failed to comply adequately with the procedural requirements of Rule 23.”
FIFTH CIRCUIT
<i>Regents of the Univ. of Cal. v. Credit Suisse First Boston (USA), Inc.</i> , 482 F.3d 372, 381 (5th Cir. 2007) (footnotes omitted).
The court condoned a review of facts beyond the pleadings and reversed a class certification order, citing to its own opinions and to <i>In re IPO, Gariety</i> , and others: <p>Our circuit’s conclusion that review of the factual and legal analysis supporting the district court’s decision is appropriate on review of class certification enjoys widespread acceptance in the courts of appeals, and neither the Supreme Court authority nor the Fifth Circuit caselaw that plaintiffs cite for the proposition that no merits inquiry is permitted is to the contrary.</p>
SEVENTH CIRCUIT
<i>Szabo v. Bridgeport Machs., Inc.</i> , 249 F.3d 672, 676 (7th Cir. 2001) (quoting FED. R. CIV. P. 23(b)(3)).
The Seventh Circuit explained: <p>And if some of the considerations under Rule 23(b)(3), such as “the difficulties likely to be encountered in the management of a class action”, [sic] overlap the merits—as they do in this case, where it is not possible to evaluate impending difficulties without making a choice of law, and not possible to make a sound choice of law without deciding whether Bridgeport authorized or ratified the dealers’ representations—then the judge must make a preliminary inquiry into the merits.</p>

EIGHTH CIRCUIT
Blades v. Monsanto Co., 400 F.3d 562, 575 (8th Cir. 2005).
“[I]n ruling on class certification, a court may be required to resolve disputes concerning the factual setting of the case. This extends to the resolution of expert disputes concerning the import of evidence concerning the factual setting—such as economic evidence as to business operations or market transactions.”
NINTH CIRCUIT
Dukes v. Wal-Mart, Inc., 509 F.3d 1168, 1177-78 n.2 (9th Cir. 2007) (alterations in original) (quoting Hanon v. Dataproducts Corp., 976 F.2d 497, 509 (9th Cir. 1992)), <i>reh’g en banc granted</i> , 556 F.3d 919 (9th Cir. 2009).
“[C]ourts are not only ‘at liberty to’ but <i>must</i> ‘consider evidence which goes to the requirements of Rule 23 [at the class certification stage] even [if] the evidence may also relate to the underlying merits of the case.’”
TENTH CIRCUIT
<i>In re Med. Waste Servs. Antitrust Litig.</i> , No. 2:03MD1546 DAK, 2006 WL 538927, at *7 (D. Utah Mar. 3, 2006).
The district court wrote that plaintiffs could not “prove their case without an exhaustive market-by-market, customer-by-customer, product-by-product, time period-by-time period inquiry” and criticized plaintiffs’ expert for presuming antitrust impact rather than offering a method for demonstrating it.

APPENDIX B: TABLES

Table 1
PLAINTIFF-STYLE REGRESSION ESTIMATE OF ALLEGED
OVERCHARGE

	(1)	(2)	(3)
Overcharge	1.10** (0.20)	-0.12 (0.27)	2.64** (0.32)
Observations	5185	2632	3775
R ²	0.79	0.80	0.81
Notes:			
[1] Standard errors are in parentheses.			
[2] ** p<0.01, * p<0.05.			
[3] The dependent variable is the average price per liter by product specification, location, store type, and half-year. The price per liter is adjusted to exclude state sales and excise taxes using tax data from www.taxadmin.org . The price per liter is also adjusted to reflect cross-location differences in food price levels using data from the B.E.A. working paper "Report on Interarea Price Levels." Aten, <i>supra</i> note 48, at 12.			
[4] Results in column (1) correspond to a specification in which the variable "Overcharge" equals 1 for Tampa, New York, Denver, Miami, and Boston, and 0 for Atlanta, Chicago, Kansas City, Minneapolis, Los Angeles, San Diego, and San Francisco.			
[5] Results in column (2) correspond to a specification in which the variable "Overcharge" equals 1 for Tampa, New York, Denver, Miami, and Boston, and 0 for Atlanta, Kansas, and San Diego.			
[6] Results in column (3) correspond to a specification in which the variable "Overcharge" equals 1 for Tampa, New York, Denver, Miami, and Boston, and 0 for Chicago, Minneapolis, Los Angeles, and San Francisco.			
[7] Regressions also controlled for store type (drug store, discount sample store, Hispanic market, and liquor store), brand (Castillo, Captain Morgan, Malibu, Bacardi, Myers's, and Ronrico), rum type (dark, flavored, gold, light, and proof 151), container size (375 ml, 750 ml, 1000 ml, and 1750 ml), and year (1996, 1997, 1998, 1999, and 2000).			

Table 2
CITY-SPECIFIC REGRESSION ESTIMATES OF ALLEGED
OVERCHARGE

	(1)
Overcharge - Tampa	3.47** (0.30)
Overcharge - New York	-0.99** (0.30)
Overcharge - Denver	1.36** (0.30)
Overcharge - Miami	0.98** (0.30)
Overcharge - Boston	0.71* (0.30)
Observations	5185
R ²	0.83
Notes:	
[1] Standard errors are in parentheses.	
[2] ** p<0.01, * p<0.05.	
[3] The dependent variable is the average price per liter by product specification, location, store type, and half-year. The price per liter is adjusted to exclude state sales and excise taxes using tax data from www.taxadmin.org. The price per liter is also adjusted to reflect cross-location differences in food price levels using data from the B.E.A. working paper "Report on Interarea Price Levels." Aten, <i>supra</i> note 48, at 12.	
[4] Results correspond to a specification in which the benchmark markets are Atlanta, Kansas City, San Diego, San Francisco, Chicago, Los Angeles, and Minneapolis.	
[5] Regressions also controlled for store type (drug store, discount sample store, Hispanic market, and liquor store), brand (Castillo, Captain Morgan, Malibu, Bacardi, Myers's, and Ronrico), rum type (dark, flavored, gold, light, and proof 151), container size (375 ml, 750 ml, 1000 ml, and 1750 ml), and year (1996, 1997, 1998, 1999, and 2000).	

Table 3
 BRAND- AND CITY-SPECIFIC REGRESSION ESTIMATES OF
 ALLEGED OVERCHARGE

	(1) Bacardi Only	(2) Castillo Only	(3) Captain Morgan Only	(4) Malibu Only	(5) Myers's Only	(6) Ronrico Only
Overcharge - Tampa	3.88** (0.43)	1.89** (0.56)	4.07** (0.84)	2.95** (0.72)	4.48** (1.34)	1.82** (0.52)
Overcharge - New York	-1.37** (0.43)	-0.21 (0.47)	-0.30 (0.92)	-1.22 (0.71)	-0.56 (1.33)	-0.41 (0.52)
Overcharge - Denver	1.40** (0.43)	1.13* (0.47)	2.53** (0.84)	1.87** (0.71)	-0.35 (1.33)	0.55 (0.52)
Overcharge - Miami	1.20** (0.43)	0.22 (0.47)	1.44 (0.84)	1.18 (0.71)	0.83 (1.33)	0.45 (0.52)
Overcharge - Boston	0.56 (0.43)	1.07* (0.47)	1.61 (0.84)	1.07 (0.71)	0.55 (1.33)	-0.26 (0.52)
Observations	2663	594	607	492	341	488
R ²	0.82	0.60	0.79	0.78	0.74	0.78
Notes:						
[1] Standard errors are in parentheses.						
[2] ** p<0.01, * p<0.05.						
[3] The dependent variable is the average price per liter by product specification, location, store type, and half-year. The price per liter is adjusted to exclude state sales and excise taxes using tax data from www.taxadmin.org. The price per liter is also adjusted to reflect cross-location differences in food price levels using data from the B.E.A. working paper "Report on Interarea Price Levels." Aten, <i>supra</i> note 48, at 12.						
[4] Results correspond to a specification in which the benchmark markets are Atlanta, Kansas City, San Diego, San Francisco, Chicago, Los Angeles, and Minneapolis.						
[5] Regressions also controlled for store type (drug store, discount sample store, Hispanic market, and liquor store), brand (Castillo, Captain Morgan, Malibu, Bacardi, Myers's, and Ronrico), rum type (dark, flavored, gold, light, and proof 151), container size (375 ml, 750 ml, 1000 ml, and 1750 ml), and year (1996, 1997, 1998, 1999, and 2000).						

Table 4
CITY- AND YEAR-SPECIFIC REGRESSION ESTIMATES OF
ALLEGED OVERCHARGE OF CASTILLO BRAND RUM

	(1) 1996	(2) 1997	(3) 1998	(4) 1999	(5) 2000
Overcharge - Tampa	0.00 (0.00)	2.38* (1.05)	1.74* (0.73)	2.15** (0.73)	1.77** (0.58)
Overcharge - New York	-0.12 (0.54)	0.01 (0.78)	-0.17 (0.66)	-0.24 (0.55)	-0.47 (0.44)
Overcharge - Denver	1.64** (0.54)	1.34 (0.78)	1.03 (0.66)	1.03 (0.55)	0.69 (0.44)
Overcharge - Miami	0.18 (0.54)	0.44 (0.78)	0.20 (0.66)	0.25 (0.55)	0.13 (0.44)
Overcharge - Boston	1.38** (0.54)	1.49 (0.78)	1.20 (0.66)	0.87 (0.55)	0.50 (0.44)
Observations	109	120	128	119	118
R ²	0.76	0.52	0.59	0.62	0.70
Notes:					
[1] Standard errors are in parentheses.					
[2] ** p<0.01, * p<0.05.					
[3] The dependent variable is the average price per liter by product specification, location, store type, and half-year. The price per liter is adjusted to exclude state sales and excise taxes using tax data from www.taxadmin.org . The price per liter is also adjusted to reflect cross-location differences in food price levels using data from the B.E.A. working paper "Report on Interarea Price Levels." Aten, <i>supra</i> note 48, at 12.					
[4] Results correspond to a specification in which the benchmark markets are Atlanta, Kansas City, San Diego, San Francisco, Chicago, Los Angeles, and Minneapolis.					
[5] Regressions also controlled for store type (drug store, discount sample store, Hispanic market, and liquor store), brand (Castillo, Captain Morgan, Malibu, Bacardi, Myers's, and Ronrico), rum type (dark, flavored, gold, light, and proof 151), container size (375 ml, 750 ml, 1000 ml, and 1750 ml), and year (1996, 1997, 1998, 1999, and 2000).					

Table 5
TRANSACTIONAL DATA BEFORE AND AFTER PRICE
ANNOUNCEMENTS

	Invoice Date	Invoice Price	Customer	Seller	Product	Before/After Effective Date of Price Increase
1	06/11/98	\$1.29	C-1	S-1	Product 1A	0
2	07/30/98	\$1.29	C-1	S-1	Product 1A	0
3	12/17/98	\$1.29	C-1	S-1	Product 1A	0
4	03/18/99	\$1.34	C-1	S-1	Product 1A	1
5	09/17/99	\$1.34	C-1	S-1	Product 1A	1
6	09/29/98	\$0.90	C-2	S-2	Product 2A	0
7	12/31/98	\$0.90	C-2	S-2	Product 2A	0
8	02/26/99	\$0.90	C-2	S-2	Product 2A	0
9	03/31/99	\$0.90	C-2	S-2	Product 2A	1
10	09/30/99	\$0.90	C-2	S-2	Product 2A	1
11	08/31/98	\$0.90	C-3	S-3	Product 3	0
12	12/31/98	\$0.90	C-3	S-3	Product 3	0
13	07/02/99	\$0.91	C-3	S-3	Product 3	1
14	10/06/99	\$0.91	C-3	S-3	Product 3	1
15	12/07/99	\$0.91	C-3	S-3	Product 3	1
16	07/17/98	\$1.29	C-4	S-1	Product 1B	0
17	08/12/98	\$1.29	C-4	S-1	Product 1B	0
18	11/11/98	\$1.29	C-4	S-1	Product 1B	0
19	06/10/99	\$1.29	C-4	S-1	Product 1B	1
20	06/25/99	\$1.29	C-4	S-1	Product 1B	1
21	08/28/99	\$1.06	C-5	S-4	Product 4	0
22	12/07/98	\$1.06	C-5	S-4	Product 4	0
23	02/25/99	\$1.11	C-5	S-4	Product 4	1
24	04/14/99	\$1.11	C-5	S-4	Product 4	1
25	12/17/99	\$1.11	C-5	S-4	Product 4	1
26	04/22/98	\$1.19	C-6	S-2	Product 2B	0
27	11/16/98	\$1.19	C-6	S-2	Product 2B	0
28	10/27/99	\$1.21	C-6	S-2	Product 2B	1
29	04/22/99	\$1.21	C-6	S-2	Product 2B	1
30	06/21/99	\$1.21	C-6	S-2	Product 2B	1

Table 6
REGRESSION ANALYSIS OF THE IMPACT OF THE PRICE
ANNOUNCEMENTS

	(1)
(After) Price Announcement	0.0217** (0.0044)
C-2	-0.1856** (0.0075)
C-3	-0.1840** (0.0074)
C-4	0.2243** (0.0075)
C-5	0.2043** (0.0075)
C-6	0.1120** (0.0075)
Constant	1.0770** (0.0059)
Observations	30
Adjusted R ²	0.995
Notes:	
[1] Standard errors are in parentheses.	
[2] ** p<0.01.	