

Exhibit 4

**IN THE UNITED STATES DISTRICT COURT
FOR THE NORTHERN DISTRICT OF CALIFORNIA**

PEOPLE OF THE STATE OF CALIFORNIA,
et al.,
Plaintiffs,

v.

META PLATFORMS, INC, Instagram, LLC,
Meta Payments, Inc., Meta Platforms
Technologies, LLC., et al.,
Defendants

IN RE: SOCIAL MEDIA ADOLESCENT
ADDICTION/PERSONAL INJURY
PRODUCTS LIABILITY LITIGATION

THIS DOCUMENT RELATES TO:
4:23-cv-05448

MDL No. 3047

Case No. 4:23-cv-05448-YGR

Judge: Hon. Yvonne Gonzalez Rogers

Magistrate Judge: Hon. Peter H. Kang

REBUTTAL TRIAL REPORT OF CARL S. SABA, MBA, CVA, ASA, ABV

February 20, 2026

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I. Summary of Opinions

1. The January 9, 2026, report of Dr. Bruce Isaacson (“Isaacson Report”) includes the following criticisms of my affirmative expert report issued in this matter on November 21, 2025 (“Opening Report”):

- a) My use of the BEEF Survey is flawed and improper because the survey was not designed to measure the absolute level of subjective bad experiences on Instagram.¹
- b) My use of the Thorn Surveys to calculate the number of U13s using Instagram and Facebook is also inappropriate and flawed, due to lack of survey controls and insufficient details.²
- c) I inappropriately extrapolate survey results to time periods other than when the surveys were conducted.³

2. My responses to Isaacson’s criticisms are summarized below, and discussed in further detail in Sections V - IX of this report:

- a) The Isaacson Report narrowly focuses on survey design considerations and does not address whether the BEEF and Thorn surveys can be used as contemporaneous evidence to support reasonable certainty in my calculations.
- b) Isaacson ignores that it is accepted practice for experts calculating economic damages and disgorgement to rely on data not originally prepared for that purpose.

¹ Isaacson Report, pp. 1-2.

² Isaacson Report, pp. 2-3.

³ Isaacson Report, pp. 2-3.

- c) Isaacson raises a number of potential issues with the surveys, some of which are irrelevant to the objective of my calculations. For many others, he does not quantify to what extent the potential issues do in fact exist, how they impact my calculations, and whether they are material to my calculations.
- d) Isaacson ignores that the BEEF survey was prepared by Meta's own expert researcher, Dr. Andrews, and that Thorn's survey work was highly regarded by Meta.
- e) Isaacson ignores significant evidence and supporting analysis that I conducted to extrapolate BEEF results to time periods other than July 2021. Isaacson also misunderstands my method of extrapolating U13 results in the Thorn Surveys to earlier time periods.

3. The January 9, 2026, report of Dr. Justin McCrary ("McCrary Report") includes the following criticisms of my Opening Report:

- a) My report fails to establish that my purported UDAP violations based on time spent or bad encounters result in harm, addictive use, or mental health challenges to Teens.⁴
- b) My methodology for calculating instances of bad encounters is unreliable and overstated. I assume that the BEEF survey results from July 2021 can be extrapolated to the timeframe of February 2018 through March 2024 without

⁴ McCrary Report, pp. 1-3.

regard to potential changes in teen usage patterns and the potential impact of features that Meta rolled out on Instagram after the Survey was conducted.⁵

- c) My calculations of Meta's revenues and profits from Teens and Aged-Up Teens for disgorgement are flawed and unsound.⁶ I assume that subject Teens Users would not generate any revenue at all absent the alleged misconduct.⁷ I fail to net out losses against teens with profits for Aged-Up Teens, and I miscalculate the percentage of 17-year-olds who exceeded certain thresholds of time.⁸
- d) I apply a static 1.35 or 1.15 deflator to MAUs to derive person counts, which ignores the prevalence of bots, spam, and business accounts, particularly around July 2021.⁹

4. My responses to McCrary's criticisms are summarized below, and discussed in further detail in Section X of this report:

- a) I do not opine on what constitutes a violation of Unfair Deceptive Acts or Practices ("UDAP"), or on causation of harm, addictive use, or mental health challenges to Teens. McCrary's discussion of these topics does not respond to or undermine my opinions and appears to fall outside of his areas of expertise.

⁵ McCrary Report, p. 3.

⁶ McCrary Report, pp. 3-4.

⁷ McCrary Report, p. 4.

⁸ McCrary Report, p. 4.

⁹ McCrary Report, pp. 4-5.

- b) My bad encounter calculations based on the BEEF survey provide a reasonable interpretation of the survey results and provide a range to accommodate the possibility that users that experience bad encounters from week to week may not be statistically independent. McCrary ignores considerable evidence that instances of bad encounters at dates before and after the BEEF survey was conducted are internally consistent with survey results. This includes that Teen platform usage was not significantly lower before or after the BEEF survey, and that certain tools and features that Meta rolled out after the survey appeared to be ineffective.
- c) My disgorgement calculations are not intended to determine the incremental profits realized from Teens and Aged-Up Teens, and do not—as asserted by McCrary—assume that absent the misconduct these Teen Users would not generate any advertising revenue.
- d) My calculations of disgorgement of profits from Teens and Aged-Up Teens can easily be netted by summing up the results disclosed in my Opening Report; McCrary is commenting on a superficial presentation choice that has no bearing on the substance of my opinions.
- e) McCrary’s proposed alternative method for determining the percentage of 17-year-olds who exceeded certain thresholds of time suffers from an undercount bias, and his proposed method to estimate cohorts of 17-year-olds as a percentage of total Teens does not provide a more reliable estimate than my methodology.

- f) I do not apply a static 1.35 or 1.15 deflator to MAUs to derive person count as asserted by McCrary. The McCrary report contradicts itself by subsequently acknowledging that I adjust my multiplier in months where person counts would otherwise exceed the Census Teen population.

II. Qualifications

5. My qualifications are stated in my affirmative expert report issued in this matter on November 21, 2025 (“Opening Report”). My current curriculum vitae is attached to this report as Appendix C and provides additional details.

6. My employer Hemming Morse, LLC (“HM”) is being compensated at an hourly rate of \$660 for my time, and at hourly rates ranging from \$255 to \$580 for employees who assisted me on this assignment.

III. Assignment

7. I have been retained through my employer, Hemming Morse, LLC, Financial and Forensic Consultants, by 29 States and Commonwealths of the United States that are a party to this Multi-District Litigation (the “MDL States” or the “MDL Plaintiffs”) and their respective Attorneys General (“Counsel”) in relation to alleged violation by Meta Platforms Inc., Instagram, LLC, Meta Payments, Inc., and Meta Platforms Technologies, LLC (“Defendants” or “Meta” or “Company”) of the Children’s Online Privacy Protection Act (“COPPA”) and of each state’s Unfair and Deceptive Acts and Practices statutes (the “UDAP Statutes”). MDL Plaintiffs allege that Meta violated COPPA and engaged in unfair and / or deceptive acts or practices which violated states’ consumer protection statutes and further

constitute unlawful acts under common law principles.¹⁰ The MDL Plaintiffs are listed in the table below.¹¹

MDL Plaintiffs - States or Commonwealths		
Arizona	Kentucky	Oregon
California	Louisiana	Pennsylvania
Colorado	Maine	Rhode Island
Connecticut	Maryland	South Carolina
Delaware	Minnesota	South Dakota
Hawaii	Nebraska	Virginia
Idaho	New Jersey	Washington
Illinois	New York	West Virginia
Indiana	North Carolina	Wisconsin
Kansas	Ohio	

8. I was asked to perform certain calculations by Plaintiffs for the period from January 2012 through March 31, 2024 (the “Relevant Time Period”). These calculations are contained in my Opening Report. I hereby incorporate by reference my Opening Report into the present report.

9. For the purposes of this report, I have been asked to review and rebut the January 9, 2026, reports of Drs. Bruce Isaacson and Justin McCrary (the Isaacson Report and the McCrary Report, respectively).

10. Capitalized terms referenced in this report have the same meaning as that defined in my Opening Report. This report summarizes my current opinions given the information

¹⁰ Amended Complaint for Injunctive and Other Relief, 29 States and Commonwealths of United States v. Meta Platforms, Inc.; Instagram, LLC; Meta Payments, Inc.; and Meta Platforms Technologies, LLC, No. 23-CV-5448 ECF 207 (June 9, 2025), pp.3, 6, 10. (“Complaint”).

¹¹ I understand that 11 of the MDL States only allege violations of COPPA and the remaining 18 of the MDL States also assert UDAP claims: California, Colorado, Connecticut, Delaware, Illinois, Indiana, Kansas, Kentucky, Louisiana, Minnesota, Nebraska, New Jersey, New York, North Carolina, Pennsylvania, South Carolina, Virginia, and Wisconsin (collectively “UDAP States”).

available to me to date; I may consider any additional materials that become available and amend or supplement my opinions and this report, if appropriate. As of the date of this report, I understand that Meta will produce a revised data set containing daily average time spent data for Facebook and Instagram and MAUs on Facebook and Instagram. I understand that this updated data set will eliminate MAUs that had zero time spent on these platforms for a given month, as well as use a more precise accuracy parameter for calculating approximate percentiles in the data set. When this data is provided, I intend to issue a supplemental report incorporating this revised data and addressing the impact of this revised data on the calculations in my Opening Report.

11. In connection with my anticipated testimony at trial or other proceedings, I may be asked to create, from various documents produced in this litigation and obtained through independent research, additional demonstrative exhibits which refer or relate to the matters discussed in this report. Other than the exhibits appended to this report, I have not yet created such demonstrative exhibits.

12. In my work I have been assisted by others in my firm who have acted under my direction and control. However, the opinions in this report are my own. I recognize that I am an expert witness, not a witness of fact. My understanding of the relevant facts comes from the documents obtained in discovery and materials I gathered.

13. I understand that this report may be made available to other parties in this litigation, to their counsel and experts, as well as to the Court. It has been prepared for use in this action. In all other respects, this report is confidential. It should not be used, reproduced, or circulated for any other purpose, in whole or in part, without my prior written consent. No other party is entitled to rely on this report for any purpose whatsoever.

IV. Evidence Relied Upon

14. A listing of the documents and information considered in deriving my opinions in this report is outlined in Appendix B of my Opening Report and Appendix D of this report.

15. I incorporate by reference my Opening Report, including its schedules and appendices.

V. General Considerations on the Isaacson Report

16. Isaacson identifies himself as an expert in surveys and research on consumer behavior and attitudes and related fields. He defines his professional role in this matter narrowly. He states that he has been retained “...to review and respond to certain opinions in the Saba Report—specifically, Mr. Saba’s interpretation, analysis, and use of the BEEF Survey and the Thorn Surveys.”¹² He further explains that his opinions are based on his “...expertise in survey design and interpretation.”¹³ Consistent with that framing, Isaacson expressly disclaims offering opinions on damages calculations, stating that he is “...not opining on Mr. Saba’s calculations of damages.”¹⁴ His report does not state that he was asked to prepare an alternative damages analysis, quantify damages, or evaluate my work under the reasonable-certainty standard that applies to my calculations as a damages expert.¹⁵

17. Isaacson does not conclude that I failed to apply a reliable methodology to derive my calculations or that my method is incapable of supporting my opinions. Instead, he repeatedly frames his critique around survey design and original purpose. For example, he

¹² Isaacson Report, p. 3.

¹³ Isaacson Report, p. 1.

¹⁴ Isaacson Report, p. 27.

¹⁵ My references to damages in this report means the broad categories of the types of calculations of monetary remedies that damages experts typically prepare in my field, including disgorgement and violations / penalties.

states that the BEEF Survey “...was designed to track progress over time, not to measure absolute levels of subjective bad experiences, and has many limitations...”¹⁶, and that my Opening Report is using the survey “...for a very specific purpose for which it was not designed.”¹⁷ His criticism is directed at the source data and its original design intent, rather than at the merit or consistency of my analytical methodology.

18. Isaacson’s report focuses on whether the surveys were designed to measure outcomes with scientific or experimental rigor, rather than on whether the survey results may assist the factfinder in evaluating aggregate exposure to bad encounters. In that regard, he emphasizes the absence of design features he views as important, noting that the BEEF Survey “...does not include any other elements to control for biases and measurement errors...” beyond its production and holdout structure.¹⁸ His analysis does not address whether estimates of instances of bad encounters derived from such data may nonetheless be informative, relevant and reliable when used with my reasonable and stated assumptions.

19. Isaacson also does not identify or quantify any specific level of error in my Opening Report estimates. While discussing potential limitations, he states that survey responses “may be inflated”¹⁹ or “may be unreliable,”²⁰ that results “may be affected” by respondent behavior²¹, and that “...some survey respondents may guess, may be inattentive,

¹⁶ Isaacson Report, p. 1.

¹⁷ Isaacson Report, p. 2.

¹⁸ Isaacson Report, p. 2.

¹⁹ Isaacson Report, p. 2.

²⁰ Isaacson Report, p. 11.

²¹ Isaacson Report, p. 27.

or may misunderstand survey instructions.”²² He further notes that survey data “...is subject to measurement errors...” due to factors such as bias or false positives.²³ At the same time, Isaacson acknowledges that “...there is no way to know the magnitude of such errors.”²⁴ He does not estimate an error rate, propose a range of potential error, or assess whether any such error would materially affect the aggregate estimates at issue.

20. Finally, Isaacson does not identify any inconsistency with established professional standard applicable to economic damages analysis. His criticisms evaluate the surveys against preferred survey-design attributes that I don’t believe are relevant to my analysis, but he does not state that reliance on secondary survey data, cross-sectional surveys, or assumption-based extrapolation falls outside accepted practices among damages or disgorgement experts. Nor does he assert that my Opening Report assumptions are internally inconsistent or incapable of evaluation. Although Isaacson notes that I rely on assumptions when applying the survey data, he does not opine on those assumptions or state that those assumptions cannot be tested, bounded, or evaluated through alternative data sources or scenarios.²⁵ In this way, Isaacson engages with one piece of my analysis (the survey data inputs) in a vacuum, and does not undermine or rebut my calculations as a whole.

²² Isaacson Report, p. 10.

²³ Isaacson Report, pp. 2, 9.

²⁴ Isaacson Report, p. 2.

²⁵ Isaacson Report, pp. 1-3.

VI. Isaacson Appears to Ignore the Reasonable-Certainty Standard That Is Relevant to the Work of Economic Damages Experts.

1. Guidance and standards relevant to economic damages, disgorgement, and penalty calculations

21. I understand that courts generally require economic damages (which I understand to also include disgorgement or penalty counting calculations, as discussed below), to be proven with reasonable certainty. I further understand that this standard does not require mathematical precision or exact reconstruction of a hypothetical but-for world. Rather, reasonable certainty requires that such an estimate be grounded in evidence and reasoned analysis rather than conjecture. As explained in the AICPA Forensic and Valuation Services *Attaining Reasonable Certainty in Economic Damages Calculations* (the “*Attaining Reasonable Certainty* AICPA Practice Aid”), “...although most courts in the United States require that lost profits be proven with reasonable certainty, there is no absolute test that specifically establishes what constitutes reasonable certainty...” and the determination necessarily depends on the “...facts and circumstances specific to that case.”²⁶

22. I understand that there is no specific, single measurement or level of proof for reasonable certainty. As explained in the *Litigation Services Handbook: The Role of the Financial Expert* (“the *Litigation Services Handbook*”), “...no single measure of reasonable certainty exists,” however reasonable certainty typically allows for the plaintiff to “...estimate the amount of damages, but the court should have confidence in the accuracy of the

²⁶ AICPA, *Forensic and Valuation Services Practice Aid: Attaining Reasonable Certainty in Economic Damages Calculations*, (2018) p. 8.

estimate.”²⁷ The *Litigation Services Handbook* further specifies that the “...plaintiff must use the best available evidence” and the court cannot “...[require] too high [a] level of proof that would encourage unscrupulous actions on their part.”²⁸ These excerpts can be seen in the below screenshot from the *Litigation Services Handbook*.²⁹

(a) Reasonable Certainty

Section 352 of the *Restatement (Second) of Contracts* (1981) states, “Damages are not recoverable for loss beyond an amount that the evidence permits to be established with reasonable certainty.”⁴⁷ Yet while all courts require reasonable certainty for the award of damages, no single measure of reasonable certainty exists. Individual states and federal courts have all issued their own opinions as to the nature of reasonable certainty.⁴⁸

Most cases concur on certain points:⁴⁹

- The court must be certain that some injury to the plaintiff occurred.
- The defendant must not benefit from its wrongful actions, and courts generally resolve doubts in favor of the plaintiff.
- The defendant’s intentional or willful action can result in a court requiring a lesser degree of certainty from the plaintiff.
- The plaintiff can estimate the amount of damages, but the court should have confidence in the accuracy of the estimate.
- For a well-established business, past performance reasonably predicts the future.
- For a new or speculative business, parties can establish the measure of damages with reasonable certainty by the use of expert testimony, business records, economic and financial data, and other verifiable data.
- The plaintiff must use the best available evidence.

Most important, the court must balance its judgment: it cannot favor the defendants to a contract by requiring too high level of proof that would encourage unscrupulous actions on their part, nor should the court favor the plaintiff by requiring too little proof, which would encourage the parties to undertake costly actions to avoid loss to plaintiffs who might file a lawsuit in the future.

²⁷ Robert L. Weil, Daniel G. Lentz & Elizabeth A. Evans, *Litigation Services Handbook: The Role of the Financial Expert* (6th ed. 2017), p. 4.9.

²⁸ Robert L. Weil, Daniel G. Lentz & Elizabeth A. Evans, *Litigation Services Handbook: The Role of the Financial Expert* (6th ed. 2017), p. 4.9.

²⁹ Robert L. Weil, Daniel G. Lentz & Elizabeth A. Evans, *Litigation Services Handbook: The Role of the Financial Expert* (6th ed. 2017), p. 4.9.

23. The reasonable certainty standard is also discussed in the AICPA publication *Essentials of Forensic Accounting*.³⁰

Reasonable Certainty

Another legal principle is *reasonable certainty* that says claimants must prove lost profits damages to a reasonable degree of certainty to be awarded damages. One court described the meaning of reasonable certainty as “the standard for degree of certainty requires that the mind of a prudent impartial person be satisfied with the damages.”²⁰ Moreover, reasonable certainty is different than certainty. Another court said²¹

The second requirement, that damages be reasonably certain, does not require absolute certainty. Damages resulting from the loss of future profits are often an approximation. The law does not require that they be determined with mathematical precision. It requires only that damages be capable of measurement based upon known reliable factors without undue speculation....[citations omitted]

Courts generally apply the reasonable certainty principle to whether a claimant has incurred any damages (the fact of damages), rather than to the amount of damages. Put differently, claimants need to prove with reasonable certainty that they incurred some amount of damages. Damage measurements, however, have a somewhat lower standard because they are estimates. One federal court said²²

The requirement that future profits must be proved with reasonable certainty particularly applies to the fact of damages.... It is particularly in the area of quantifying the amount of lost profits that courts impose the risk of uncertainty on the breaching party whose breach gave rise to the uncertainty.

24. As can be seen above, *Essentials of Forensic Accounting* notes that the courts have tended towards a lower standard for the measurement of damages, as compared to the fact of damages.

25. I understand that the reasonable certainty standard does not apply solely to the calculation of lost profits or economic loss incurred by the plaintiff. As an example, there is a specific reference to a calculation of *defendant's profits* comprising unjust enrichment (i.e. ill-gotten gains) in a section of the *Attaining Reasonable Certainty* AICPA Practice Aid pertaining to best evidence and use of historical financial data to support an expert's calculations.³¹ Therefore, I understand that the reasonable certainty standard would apply to

³⁰ Michael A. Crain et al., *Essentials of Forensic Accounting* (American Institute of Certified Public Accountants 2015), p. 436.

³¹ AICPA, *Forensic and Valuation Services Practice Aid: Attaining Reasonable Certainty in Economic Damages Calculations* (2018), Chapter 3, “Best Evidence”, pp. 63-64.

the calculations I have performed, which would be the basis for the calculation of penalties or disgorgement, if so found by the trier of fact. I will continue to frame my discussion in terms of damages, to include calculation of disgorgement and instances of violations that may result in penalties.

26. Further guidance indicating that reasonable certainty is not solely applicable to the calculation of lost profits or of another type of specific damage can be seen below from the *Litigation Services Handbook*. Damages include “restitution (or disgorgement)” and “[p]unitive damages,” which appears to be analogous with penalties in this use.³²

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- | | |
|----|--|
| 1. | Measures related to the extent of damages |
| | <ul style="list-style-type: none"> • Actual (but-for) damages: caused directly by the defendant’s improper actions • Consequential damages: a natural and foreseeable, but indirect, consequence of the improper actions |
| 2. | Damages measures focused on plaintiff |
| | <ul style="list-style-type: none"> • Compensatory damages: either expectation damages or reliance damages • Expectation damages: the difference between the amount which the plaintiff reasonably expected to receive and the actual amount received; also known as benefit of the bargain (BOB) damages • Reliance damages (RD): an amount the plaintiff lost because it relied on false representations from the defendant |
| 3. | Damages measure focused on defendant |
| | <ul style="list-style-type: none"> • Restitution (or disgorgement) damages (RR): the amount that the defendant foreseeably gained at the plaintiff’s expense |
| 4. | Other damages measures |
| | <ul style="list-style-type: none"> • Punitive damages: intended to punish or dissuade the objectionable behavior; not directly tied to the plaintiff’s loss or defendant’s gains |
-
- Exhibit 4-4. Damages Categories**

27. As can be seen below from the *Litigation Services Handbook*, the calculation of restitution damages includes “ill-gotten gains.”³³

³² Robert L. Weil, Daniel G. Lentz & Elizabeth A. Evans, *Litigation Services Handbook: The Role of the Financial Expert* (6th ed. 2017), p. 4.12.

³³ Robert L. Weil, Daniel G. Lentz & Elizabeth A. Evans, *Litigation Services Handbook: The Role of the Financial Expert* (6th ed. 2017), p. 4.16.

- **Remedy of restitution damages (RR):** For restitution damages, the practitioner must look at this situation from the viewpoint of the defendant and calculate its ill-gotten gains. Assuming the seller joined the competing company that benefited by this breach and they were codefendants, the damages would include the profits earned by the defendant using the competing technology.

28. I also understand from the below excerpt from the *Litigation Services Handbook*, the calculation of damages also includes “[p]unitive [d]amages.”³⁴

(c) Punitive Damages

*Punitive damages (also called exemplary, vindictive, presumptive, and added damages) are special damages awarded in addition to actual damages. They function to penalize the wrongdoer or deter others from engaging in similar wrongful acts. Courts often award them when the defendant exhibited malicious, deceitful, or particularly reckless or reprehensible conduct. Courts do not award punitive damages in breach of contract matters.*⁷²

The court decides on the amount of punitive damages pursuant to Supreme Court guidance. (Chapter 17 discusses the reasons for awarding punitive damages and the reasonableness of amounts awarded.) In some litigation, including willful patent infringement or antitrust violations, the court can award treble damages under statute as a particular type of punitive damages. Judge Richard Posner suggests that one should consider the difficulty of concealment when awarding punitive damages. For example, if the probability of discovery of a tort equals 10 percent and the optimal damages award equals \$60, the amount of punitive damages should equal \$600 (= \$60/0.10).⁷³ Where liquidated damages provided in a contract exceed the actual damages likely to be caused by a breach, the court has discretion to treat some of the damages as punitive damages.

29. *Essentials of Forensic Accounting*, in the context of the application of the reasonable certainty standard, also includes disgorgement of the wrongdoer’s ill-gotten gains and punitive damages among damages models.³⁵

30. I understand that the reasonable-certainty standard does not require damages to be proven with absolute precision. The *Attaining Reasonable Certainty* AICPA Practice Aid

³⁴ Robert L. Weil, Daniel G. Lentz & Elizabeth A. Evans, *Litigation Services Handbook: The Role of the Financial Expert* (6th ed. 2017), p. 4.18.

³⁵ Michael A. Crain et al., *Essentials of Forensic Accounting* (American Institute of Certified Public Accountants 2015), pp. 422 and 427-429.

explains that damages calculations frequently involve estimation and professional judgment, reflecting the practical reality that exact measurement is often not possible in economic damages analyses. As noted in the *Attaining Reasonable Certainty* AICPA Practice Aid, damages calculations “...typically require[] damages experts to consider facts and circumstances specific to that case...,” which necessarily entails approximation rather than exactitude.³⁶

31. I understand that reasonable certainty is closely tied to the use of “best evidence,” meaning the most reliable and relevant data reasonably available under the circumstances. The *Attaining Reasonable Certainty* AICPA Practice Aid explains that evidence plays a critical role in establishing reasonable certainty and that courts examine whether a damages expert has relied on appropriate sources of information, such as historical financial data, contemporaneous market data, or other case-specific evidence, rather than unsupported assumptions or speculative inputs.³⁷

32. I further understand that best evidence does not require perfect or complete data, but it does require that the expert identify, evaluate, and explain why the evidence relied upon is the most appropriate available for estimating damages in the specific factual context of the case, consistent with the reasonable-certainty standard.³⁸

³⁶ AICPA, *Forensic and Valuation Services Practice Aid: Attaining Reasonable Certainty in Economic Damages Calculations*, (2018), p. 8.

³⁷ AICPA, *Forensic and Valuation Services Practice Aid: Attaining Reasonable Certainty in Economic Damages Calculations*, (2018), Chapter 3, pp. 59-60.

³⁸ AICPA, *Forensic and Valuation Services Practice Aid: Attaining Reasonable Certainty in Economic Damages Calculations*, (2018), Chapter 3, pp. 59-60.

33. I understand that the damages expert bears the practical responsibility of developing an analysis that satisfies the reasonable-certainty standard. This responsibility includes applying accepted economic or financial principles to the specific facts of the case and exercising professional judgment where precise calculation is not feasible. The *Attaining Reasonable Certainty* AICPA Practice Aid emphasizes that courts scrutinize whether damages experts have employed an analytical process, rather than assumptions, in developing revenue estimates, growth rates, and other key inputs.³⁹

34. I understand that my role as a damages expert is not to eliminate all uncertainty, but to provide a reasonable estimate supported by evidence and analysis.⁴⁰ The *Attaining Reasonable Certainty* AICPA Practice Aid explains that damages opinions lacking such an analytical foundation—where estimates are “untethered” from any meaningful analysis—fail to meet the reasonable-certainty standard, whereas estimates grounded in case-specific evidence and analysis satisfy that requirement.⁴¹

35. I understand that Isaacson’s criticism of the survey evidence that I rely on must be evaluated in light of the reasonable-certainty standard, which does not require perfect evidence or absolute precision. The *Attaining Reasonable Certainty* AICPA Practice Aid explains that reasonable certainty permits the use of estimation and imperfect evidence so long as the damages analysis is grounded in appropriate evidence and reasoned analysis, and

³⁹ AICPA, *Forensic and Valuation Services Practice Aid: Attaining Reasonable Certainty in Economic Damages Calculations*, (2018), Chapter 1, pp. 11-12.

⁴⁰ AICPA, *Forensic and Valuation Services Practice Aid: Attaining Reasonable Certainty in Economic Damages Calculations*, (2018), Chapter 3, pp. 59-60.

⁴¹ AICPA, *Forensic and Valuation Services Practice Aid: Attaining Reasonable Certainty in Economic Damages Calculations*, (2018), Chapter 1, pp. 11-12.

it expressly recognizes that surveys may constitute “best evidence” when they address issues that are otherwise difficult to observe directly.⁴² I understand that a survey expert such as Isaacson who criticizes the use of surveys by demanding certainty, completeness, or precision beyond what may be achievable in real-world economic analysis fails to consider that reasonable certainty tolerates estimation and judgment. In addition, I consider the Thorn and BEEF surveys to be different from a survey prepared specifically for this or another litigation; rather, I consider these surveys to be contemporaneous and relevant reports prepared at the behest of the defendant Meta (or Thorn) and which Meta itself analyzed and considered. There is no reason to believe that the quality of this research is lacking or that it was somehow designed to reach a biased outcome.

36. Accordingly, I understand that criticisms premised on the absence of absolute precision or complete certainty are inconsistent with the reasonable-certainty standard articulated in the *Attaining Reasonable Certainty* AICPA Practice Aid, and do not address whether the survey evidence, when properly designed and analytically integrated as in my calculations, contributes to a reasoned estimate of damages.

2. Isaacson appears to ignore that damages experts often use data originally and primarily created and intended for other purposes.

37. Isaacson’s critique relies heavily on the proposition that the BEEF Survey should not be used for an analytical task other than the one for which it was “designed” He states, for example, that the BEEF Survey “...was designed to track progress over time, not to measure

⁴² AICPA, *Forensic and Valuation Services Practice Aid: Attaining Reasonable Certainty in Economic Damages Calculations*, (2018), Chapter 3, pp. 79-80.

absolute levels of subjective bad experiences.”⁴³ He then elevates that design-intent point into a conclusion of methodological defect, opining that, “Mr. Saba’s use of the BEEF Survey is flawed and improper.”⁴⁴ He further grounds this criticism in the survey’s internal experimental structure, emphasizing that the survey “...incorporated holdout and production groups...”⁴⁵ and that “...the survey was designed to compare holdout and production groups and does not include any other elements to control for biases.”⁴⁶ He also frames the dispute as one of purpose mismatch, stating that the survey was “not designed” for the task he attributes to me—“...namely to quantify the number of ‘Bad Encounters’ by teen Instagram users...”⁴⁷ In effect, it appears that Isaacson treats original design purpose as dispositive, without analyzing whether data generated for internal monitoring can nonetheless support a reasonable approximation of encounter volume when used with stated assumptions.

38. His focus on holdout-versus-production comparisons, and the absence of “...other elements to control for biases,”⁴⁸ attempts to apply a hypothesis-testing/control-group critique to an analysis that, as I devised and applied it, is not a causal treatment-effect study but rather a quantitative estimation exercise based on reported survey responses. Isaacson’s opinion that the survey could have been structured differently, or oriented toward a different objective, does not by itself establish that the existing survey data is incapable of supporting the specific

⁴³ Isaacson Report, p. 1.

⁴⁴ Isaacson Report, p. 1.

⁴⁵ Isaacson Report, p. 2.

⁴⁶ Isaacson Report, p. 2.

⁴⁷ Isaacson Report, p. 2.

⁴⁸ Isaacson Report, p. 2.

count-based estimates I performed; it establishes only that the survey was designed with a different primary objective.⁴⁹ Isaacson appears to ignore that damages experts routinely rely on data that has not been prepared for the express purpose of being used in damage calculations. For example, a company's financial statements are designed and created to provide financial information to the users of financial statements. I expect that if designed for the purposes of use in a damage calculation such as disgorgement or lost profits, financial statements would contain data that would be perfectly tailored to that effect as a matter of routine. Financial statement data is generally not thus perfectly tailored and yet damages expert routinely rely on it in their analyses.

3. Isaacson appears to ignore that the BEEF and Thorn surveys were designed by someone with survey design expertise.

39. Isaacson's critique gives no meaningful consideration to the fact that both the BEEF and Thorn surveys were designed and implemented by individuals and organizations with survey-design and research expertise. With respect to the BEEF Survey, the record reflects that it was not an ad hoc or litigation-driven instrument but rather was authored by a Meta researcher as part of Meta's internal effort to better understand harmful user experiences on the platform. As discussed in the Opening Report, the BEEF Survey "...was based on data collected over the period June 27 to July 28, 2021, and ... was authored by Dr. Kyle Andrews, a researcher on Instagram's well-being team."⁵⁰

⁴⁹ Isaacson Report, pp. 2–3.

⁵⁰ Opening Report, p. 37.

40. In Dr. Andrews' deposition, he discusses his expertise and professional roles that involved building and analyzing questionnaires, and developing surveys (including large scale surveys), with a focus on quantitative results such as those in the BEEF Survey.⁵¹

Dr. Andrews considers himself to be a subject matter expert on designing surveys, and applied that expertise in designing the BEEF Survey such that it was of high quality.⁵²

41. The survey followed internal planning and analysis reflected in Meta documents, including “Bad Experiences’ Measurement – Plan for a 2021 plan” and an internal presentation titled “Instagram Bad Experiences,” which addressed “...the need for Meta to better understand the extent of bad experiences users were having on Instagram.”⁵³ That internal presentation further recognized that Instagram may need to rethink its existing approach, noting that the current “policy-first” approach “...may not be effective in all circumstances.”⁵⁴ Isaacson does not address the qualifications of the survey's author, the internal vetting and planning that preceded the survey, or the fact that the instrument was designed by Meta's own research personnel for operational and measurement purposes rather than for litigation. Isaacson discusses a standard applicable to a survey designed specifically for litigation, rather than a survey that was prepared for another purpose and may serve as best evidence.

42. Isaacson similarly criticizes reliance on the Thorn surveys without engaging with Meta's own contemporaneous assessment of the quality of Thorn's work. As discussed in the

⁵¹ Deposition of Kyle Andrews, November 19, 2024, 198:1 – 209:25.

⁵² Deposition of Kyle Andrews, November 19, 2024, 205:5 – 210:22.

⁵³ Opening Report, p. 37.

⁵⁴ Opening Report, p. 37.

Opening Report, Meta maintained an ongoing relationship with Thorn and reviewed multiple Thorn studies conducted in partnership with Benson Strategy Group concerning minors' online experiences and safety.⁵⁵ Meta did not characterize this work as speculative or unreliable at the time. To the contrary, in a May 2021 internal email, Meta described Thorn's research as "...always measured, quality and thoughtful."⁵⁶ Isaacson does not address this contemporaneous characterization or explain why survey research that Meta itself regarded as measured, high-quality, and thoughtful should be discounted when evaluating the prevalence of under-13 users. By ignoring both the survey-design expertise underlying the BEEF Survey and Meta's own assessment of the quality of Thorn's research, Isaacson's critique omits material context necessary to fairly evaluate the reasonableness of my reliance on these surveys.

4. Isaacson's opinions are limited to survey design considerations and do not address whether plaintiffs' estimates meet the relevant reasonable-certainty standard.

43. As discussed in Sections VI.2 and VI.3, Isaacson's opinions are confined to survey design, and he does not address whether plaintiffs' estimates satisfy the applicable reasonable-certainty standard. He defines his assignment narrowly as a critique of my "...interpretation, analysis, and use of the BEEF Survey and the Thorn Surveys..."⁵⁷ and frames his conclusions as "...based on my expertise in survey design and interpretation."⁵⁸ Consistent with that framing, Isaacson repeatedly emphasizes that the BEEF Survey "...was

⁵⁵ Opening Report, p. 134.

⁵⁶ Opening Report, p. 134.

⁵⁷ Isaacson Report, p. 3.

⁵⁸ Isaacson Report, p. 1.

designed to track progress over time, not to measure absolute levels of subjective bad experiences...”⁵⁹ and asserts that I am using the survey “...for a very specific purpose for which it was not designed, namely to quantify the number of ‘Bad Encounters’ by teen Instagram users in the 18 UDAP states across a six-year period.”⁶⁰

44. Isaacson mistakenly characterizes this asserted mismatch between original design purpose and application as wholly disqualifying for the purposes of damages calculations (in which he is not expert). He states that “Mr. Saba’s use of the BEEF Survey is flawed and improper.”⁶¹ He grounds that criticism in the survey’s experimental structure, noting that it “...incorporated holdout and production groups...” and was “...designed to compare holdout and production groups and does not include any other elements to control for biases and measurement errors.”⁶² These observations reflect a focus on survey design and hypothesis-testing considerations. Isaacson does not address a more pertinent inquiry: whether data collected for internal monitoring purposes may nonetheless support reasonably certain estimates.

45. Consistent with this limited scope, Isaacson does not offer alternative estimates, propose replacement data sources, or present a competing quantitative framework. He does not attempt to quantify the magnitude of any alleged overstatement, inflation, or bias in the surveys, or their potential or purported impact on my results. To the contrary, when discussing certain potential measurement errors, Isaacson states that “...there is no way to know the

⁵⁹ Isaacson Report, p. 1.

⁶⁰ Isaacson Report, p. 2.

⁶¹ Isaacson Report, p. 1.

⁶² Isaacson Report, p. 2.

magnitude of such errors.”⁶³ He also does not opine that no reasonably certain estimate can be made; rather, his critique remains focused on the surveys’ original design intent and asserted limitations.

46. Notably absent from Isaacson’s report is any discussion of the reasonable-certainty standard or an assessment of whether the purported limitations he discusses preclude reasonable approximation as opposed to precise measurement. His repeated assertion that the surveys were “not designed” for the challenged purpose does not address whether the data they generated can nonetheless be used to estimate encounter volumes with reasonable certainty. As presented, Isaacson’s analysis stops at survey design considerations and does not engage the ultimate question relevant to Plaintiffs’ damages methodology.

VII. Isaacson’s Opinions Do Not Quantify the Impact Or Relevance of Criticisms to Aid the Trier of Fact.

47. Isaacson’s opinions identify potential limitations in the BEEF and Thorn surveys but do not explain how those limitations affect the quantitative counts I derived. Throughout his report, Isaacson discusses a range of hypothesized sources of error and bias, but he does not attempt to confirm their presence or determine their magnitude or quantitative impact. For example, in discussing potential inflation in survey responses, Isaacson states that certain features of the BEEF Survey may result in inflated estimates while also acknowledging that “...there is no way to know the magnitude of such errors.”⁶⁴ His critique is therefore framed

⁶³ Isaacson Report, p. 2.

⁶⁴ Isaacson Report, p. 2.

in theoretical and hypothetical terms, without translating the identified issues into measurable effects on the numerical results.

48. Isaacson similarly raises the possibility of respondent error and bias, stating that “...some survey respondents may guess, may be inattentive, or may misunderstand survey instructions,”⁶⁵ and that survey data “may be skewed”⁶⁶ due to biases, demand effects, or false positives.⁶⁷ With respect to false positives in particular, Isaacson acknowledges that the survey design does not permit him to determine how frequently they occur, stating that “...there is no way to know the magnitude of such errors.”⁶⁸ He does not estimate a false-positive rate, or assess whether any resulting bias would be material to the aggregate estimates at issue.

49. Consistent with these acknowledgments, Isaacson does not attempt to quantify an error rate, provide a range of possible outcomes, or conduct any sensitivity or bounding analysis to assess how the hypothesized limitations would affect encounter or usage counts. Nor does he offer alternative calculations or demonstrate how the results would change under different assumptions. Instead, his opinions remain expressed in conditional language—such as “may” and “no way to know”—and stop short of connecting the identified limitations to any specific quantitative consequence.⁶⁹

⁶⁵ Isaacson Report, p. 10.

⁶⁶ Isaacson Report, p. 18.

⁶⁷ Isaacson Report, p. 2.

⁶⁸ Isaacson Report, p. 2.

⁶⁹ Isaacson Report, p. 2.

50. As a result, while Isaacson identifies theoretical and hypothetical sources of uncertainty that can arise in survey data, his report does not establish that those uncertainties are sufficiently large to undermine the reliability of my estimates. He does not demonstrate that the potential limitations he identifies would preclude forming a reasonably certain estimate, nor does he show that they materially affect the counts derived from the surveys. As presented, Isaacson's opinions describe possible but speculative limitations in precision rather than measured effects on the quantitative analyses I conduct using the BEEF and Thorn surveys.

1. Controls and comparisons that were part of the survey design are not relevant to my Opening Report calculations.

51. Isaacson criticizes my Opening Report for not incorporating the production and holdout group structure that was part of the BEEF Survey's original design. In doing so, he frames the absence of those controls in my calculations as a methodological defect. Specifically, Isaacson asserts that my analysis "...fail[s] to reflect the controls and comparisons that were an integral and important part of the survey's design."⁷⁰ He repeatedly emphasizes that the BEEF Survey "...incorporated holdout and production groups..." and that it "...was designed to compare holdout and production groups..." in order to evaluate the impact of internal initiatives.⁷¹

52. Isaacson explains that this production/holdout structure was intended to assess the causal impact of Meta's internal changes, noting that the survey "...was designed to track

⁷⁰ Isaacson Report, p. 14.

⁷¹ Isaacson Report, pp. 1-2, 22.

progress over time...” and to provide “directional signals” regarding whether product or policy interventions were improving outcomes.⁷² He then concludes that use of the survey without incorporating those controls renders the resulting measures unreliable. For example, he asserts that, absent those controls, the survey “...does not include any other elements to control for biases and measurement errors...”⁷³ and that reported encounter rates “may be inflated” to an unknown degree.⁷⁴

53. What Isaacson does not do, however, is distinguish between a) controls necessary for causal inference and b) data sufficient to support frequency or incidence estimation under stated reasonable assumptions. His critique treats my decision to not use causal controls as dispositive, without analyzing whether such controls are required for the analytical task performed in my Opening Report. My analysis does not attempt to measure the incremental effect of an intervention by Meta to change a process, compare a but-for world to an actual world, or isolate causation. Rather, as described in the Opening Report, my assignment was to estimate the number of bad encounters in the actual world based on survey responses, i.e., a raw count exercise.⁷⁵

54. Isaacson’s report does not engage with that distinction. He does not explain why controls designed to assess causality between production and holdout groups are necessary to count reported encounters experienced by users. Nor does he address that, in Meta’s own internal reporting, the BEEF Survey results were used to measure and monitor the prevalence

⁷² Isaacson Report, pp. 1-2.

⁷³ Isaacson Report, pp. 1-2.

⁷⁴ Isaacson Report, p. 2.

⁷⁵ Opening Report, Sections XIII, XVI–XVII.

of bad experiences across the platform, including by examining reported experiences within each group, not solely the difference between groups.⁷⁶ As I explained in my Opening Report, Meta used the survey to understand “...the extent of bad experiences users were having on Instagram...” reflecting an interest in absolute levels as well as trends.⁷⁷ Indeed, Meta concluded in the “Results” section of its slide deck on the BEEF survey that “...[y]ounger people report higher rates of every [bad experience] issue.”⁷⁸ My analysis uses the BEEF Survey data in a manner that is fundamentally similar to how Meta used it at the time.

55. Isaacson also does not analyze the reported levels of bad encounters within the production and holdout groups themselves. His report does not dispute that both groups reflected substantial rates of bad encounters, nor does he assess whether the prevalence of such encounters across the platform is relevant to evaluating alleged deceptive practices. Instead, he treats the absence of a causal comparison as rendering the entire dataset unreliable, without explaining directionally how incorporation of the controls would change the counts or whether it would reduce them.

56. More broadly, Isaacson conflates the lack of causal controls with an asserted inability to estimate encounter rates at all. He emphasizes that the survey’s controls were intended to assess causality for internal initiatives,⁷⁹ and then asserts that using the survey without those controls renders the measures unreliable,⁸⁰ without addressing whether causal

⁷⁶ Opening Report, p. 37.

⁷⁷ Opening Report, p. 37.

⁷⁸ BEEF Survey, p. 21; META3047MDL-004-00015029 at 5049. Opening Report, p. 47 (citing META3047-MDL-00015029 at –5048)

⁷⁹ Isaacson Report, p. 2.

⁸⁰ Isaacson Report, p. 14.

controls are necessary to estimate frequency or incidence under stated reasonable assumptions. He does not explain how, or whether, the absence of such controls affects the numerical counts I derived, nor does he provide alternative calculations incorporating the controls to show a different result.

57. As presented, Isaacson's critique rests on the premise that data designed with causal controls cannot be used for non-causal estimation tasks. His report does not distinguish between (a) controls necessary to establish causation for a task specific to Meta and irrelevant to the matter at hand and (b) data sufficient to estimate the prevalence or frequency of reported experiences. Nor does his report explain why respondents' reported experiences are not informative in and of themselves for purposes of counting encounters, even if the same data was also used by Meta for causal comparisons in a different context. Isaacson is merely speculating that there may be an effect.

2. Isaacson identifies potential sources of bias but does not quantify their magnitude or materiality.

58. Isaacson asserts that the absence of certain survey controls may introduce acquiescence bias, demand effects, inattentive responding, and false positives into the BEEF and Thorn survey results, but he does not quantify the magnitude of any such effects or assess whether they would materially affect aggregate encounter counts. Early in his report, Isaacson states that the surveys do "...not include any other elements to control for biases and measurement errors..." and warns that survey results "...may be inflated..." as a result.⁸¹ He explains that such inflation could arise because "...some survey respondents may guess, may

⁸¹ Isaacson Report, p. 2.

be inattentive, or may misunderstand survey instructions...” and that survey data may be skewed by factors including acquiescence bias, demand effects, and false positives.⁸² He again uses the “unknown” framing when applying bias concepts to BEEF: “[t]hese kinds of responses undermine the survey’s validity ... particularly because the rate of these kinds of responses is unknown.”⁸³

59. Isaacson discusses acquiescence bias in great detail in the report. He explains that “...another type of bias that can affect survey responses is called ‘acquiescence bias’ or ‘yea-saying,’” and describes it as a tendency for respondents to agree with survey items “regardless of content.”⁸⁴ He notes that acquiescence bias can arise when respondents answer affirmatively to questions about experiences that “...would not have otherwise occurred to them if the question had not been asked of them.”⁸⁵ Although Isaacson discusses the mechanism of acquiescence bias, he does not attempt to measure whether, or to what extent, it affected responses in the BEEF or Thorn surveys. Nor does he contextualize this critique to the issues asked about the survey or consider whether countervailing forces exist that could lead to underreporting. As detailed below in Section VIII below, the 2022 Thorn survey reviewed by Isaacson explain that “participants lean into more socially desirable responses”

⁸² Isaacson Report, pp. 10-11.

⁸³ Isaacson Report, p. 17.

⁸⁴ Isaacson Report, p. 11.

⁸⁵ Isaacson Report, p. 11.

and “[e]ntrenched stigma and sensitivity surrounding the topic may lead to an undercounting of scale and frequency.”⁸⁶

60. Throughout the report, Isaacson repeatedly acknowledges that he cannot determine the magnitude of the potential errors he identifies. In discussing false positives and related measurement issues, he states that the surveys are “...subject to measurement errors such as false positives,...” and concedes that “...there is no way to know the magnitude of such errors.”⁸⁷ He reiterates this limitation when addressing extrapolation across time, again stating that “...there is no way to know...” how results would differ in other periods.⁸⁸

61. Consistent with these acknowledgments, Isaacson does not calculate a false-positive rate, estimate an inflation factor, or provide a bounded range for any alleged bias. Instead, he repeatedly characterizes the impact as unknown. For example, in criticizing my use of the BEEF Survey without incorporating production/holdout comparisons, Isaacson states that “...these numbers are inflated to an unknown degree.”⁸⁹ He does not, however, assess the extent of that unknown inflation—the numbers may not be inflated all—nor does he evaluate whether it would materially affect the aggregate encounter counts derived from the surveys.

⁸⁶ “CHALLENGE — Entrenched stigma and sensitivity surrounding the topic may lead to an undercounting of scale and frequency.

Action: Asking individuals — especially kids — to open up about a subject as delicate and personal as sharing nude images of themselves likely activates self-report bias where participants lean into more socially desirable responses.” See “Self-Generated Child Sexual Abuse Material: Youth Attitudes and Experiences in 2021.” Thorn & Benenson Strategy Group. October 2022, p. 5.

⁸⁷ Isaacson Report, pp. 2-3.

⁸⁸ Isaacson Report, p. 23.

⁸⁹ Isaacson Report, p. 14.

62. The same pattern appears throughout Isaacson's later discussion of survey reliability. He continues to rely on conditional language, asserting that survey measures "could be inflated" or "may be affected" by various biases, while repeatedly acknowledging that the extent of any such effect cannot be determined.⁹⁰ At no point does Isaacson translate these asserted possibilities into quantitative adjustments, alternative estimates, or sensitivity analyses.

63. Across his report, Isaacson therefore identifies theoretical and hypothetical sources of bias⁹¹ but does not quantify their impact, bound their effect, or assess whether they would meaningfully alter the aggregate encounter counts at issue. His critique remains framed in terms of possibilities rather than measured effects, and he repeatedly acknowledges that he does not know the magnitude of the hypothesized biases. Isaacson is merely speculating that there may be an effect.

3. Isaacson critiques subjective survey wording and the involvement of respondent recall without concluding that responses are unreliable or internally inconsistent.

64. Isaacson critiques the BEEF Survey's use of undefined and subjective wording, but he does not conclude that respondents in fact misunderstood the questions, in fact answered inconsistently, or in fact produced responses unsuitable for aggregation. In his discussion of the BEEF questions, Isaacson states that "...[t]hese and other questions in the BEEF Survey leave much to the respondent to interpret."⁹² He explains that "...the questions

⁹⁰ Isaacson Report, pp. 27, 32.

⁹¹ Isaacson Report, pp. 9-11.

⁹² Isaacson Report, p. 15.

do not define terms such as ‘false’ or ‘misleading,’ and they do not define elements such as what it means to leave someone out, what constitutes a ‘disturbing’ image, or how to define nudity or an image that someone did not want to see.”⁹³ He then frames the consequence of this wording in conditional terms, stating that “[t]he wording of these and other questions may have primed some respondents to answer affirmatively, particularly...” because the survey “...only asked about negative experiences.”⁹⁴ He further states that “...when surveys present lists of items, respondents may answer affirmatively to items that do not apply to them;” and that the BEEF Survey “...presented a list of negative experiences, which would increase the possibility of false positive responses.”⁹⁵

65. Although Isaacson raises these survey-design concerns, his language remains focused on what “may” occur and what “...would increase the possibility...” of certain response patterns,⁹⁶ rather than on identifying demonstrated misunderstanding or internal inconsistency in the actual responses. His report does not point to contradictory response patterns or internal inconsistencies in the BEEF data. Instead, he situates the concern within his broader discussion of acquiescence bias, which he defines elsewhere as “‘acquiescence bias’ or ‘yea-saying,’” where “...some respondents tend to select the affirmative response, even if that response does not actually apply to them.”⁹⁷ Consistent with that framework, when he applies the concept to my use of BEEF, he characterizes the BEEF measures as

⁹³ Isaacson Report, p. 15.

⁹⁴ Isaacson Report, p. 15.

⁹⁵ Isaacson Report, p. 15.

⁹⁶ Isaacson Report, p. 15.

⁹⁷ Isaacson Report, p. 11.

“...highly subject to acquiescence bias that creates false positive responses...” but again, Isaacson does not quantify or demonstrate that such bias occurred at any particular rate in the responses at issue.⁹⁸

66. In short, Isaacson identifies the subjectivity of terms such as “false,” “misleading,” and “disturbing,” and he asserts that the phrasing and negative-experiences context “...may have primed...” affirmative responses and “...would increase the possibility of false positive responses.”⁹⁹ He does not, however, state that respondents actually misunderstood these terms, that responses were internally inconsistent, or that the respondents’ subjective experience is an inappropriate construct for assessing the rate or frequency of bad encounters, or that a survey could be designed without any subjectivity on the part of respondents; his critique remains framed as a survey-design possibility rather than a demonstrated flaw in the response data.

67. Similarly, Isaacson states that the response options for the BEEF and Thorn surveys are “...demanding in terms of the recall or the memory they require from the respondent.”¹⁰⁰ He does not identify whether this has any impact on the response rates at all, or the direction of such impact. Isaacson is merely speculating that there may be an effect.

4. Isaacson raises concerns regarding missing age data but does not assess their empirical effect on results.

68. Isaacson raises concerns regarding missing age data in the BEEF Survey, but he does not assess the empirical effect of that issue on the resulting estimates. Early in his report,

⁹⁸ Isaacson Report, p. 17.

⁹⁹ Isaacson Report, pp. 15-17.

¹⁰⁰ Isaacson Report p. 16 (regarding BEEF) and p. 29 (regarding Thorn).

Isaacson asserts that the BEEF Survey is “...unreliable as a means to estimate bad encounters by teens on Instagram because it is missing age and state data and does not account for differences in time spent on Instagram.”¹⁰¹ He similarly states that “...the survey is missing data on age, geography, and time spent on the platform...” and that as a result “...there is no way to know whether the survey database appropriately represents the universe of people relevant to this matter.”¹⁰²

69. Isaacson discusses missing age data in greater detail later in his report. He states that, “The BEEF Survey is missing data on age for about one-third of respondents,”¹⁰³ explaining that “...[a]ge was the last question asked, so we only have age data for the 67.5% of respondents who completed the survey.”¹⁰⁴ Based on this, Isaacson concludes that “it is impossible to verify whether the ages of teens who participated in the survey are consistent with the true population of teenage Instagram users in the real world,” because “...there is no way to assess whether respondents who answered the final question, which asked about age, are similar to or different from respondents who did not answer the age question.”¹⁰⁵

70. What Isaacson does not address is how I actually used the available age data, or the fact that the BEEF Survey expressly supports teen-specific analysis. Isaacson is merely speculating that there may be an effect. In my Opening Report, I explain that I was asked “...to calculate the number of instances of Bad Encounters that Teens experienced on

¹⁰¹ Isaacson Report, p. 18

¹⁰² Isaacson Report, p. 2.

¹⁰³ Isaacson Report, p. 19.

¹⁰⁴ Isaacson Report, p. 19.

¹⁰⁵ Isaacson Report, p. 19.

Instagram, using data contained in the BEEF Survey.”¹⁰⁶ In doing so, I report teen-specific results derived from respondents who did provide age information, including explicit age-band rates. For example, I report that “54.1% of 13-15 year-olds and 57.3% of 16-17 year-olds” experienced at least one bad encounter in the prior week.¹⁰⁷ As discussed earlier in this report, I analyze the BEEF data in a way that is fundamentally similar to how Meta did contemporaneously.

5. Isaacson identifies geography-related limitations without quantifying their relevance to the estimates at issue.

71. Isaacson identifies geography-related limitations in the BEEF Survey but does not quantify their relevance to the estimates at issue. He states that the BEEF Survey “...may include responses from Instagram users who live in non-UDAP states...” and asserts that this “...may have affected Mr. Saba’s data relating to instances of bad experiences.”¹⁰⁸ He explains that California was the most populous state in 2021 and that other large states such as Texas, Florida, Georgia, and Michigan are not UDAP states, and therefore “...it is likely that some unknown portions of teen Instagram users from Texas, Florida, Georgia, and Michigan participated in the BEEF Survey even though they do not reside in states that are relevant to this matter.”¹⁰⁹

72. Isaacson further states that my analysis of the BEEF Survey did not provide “...any way to analyze the results ... by state to explore the magnitude of state-related error,

¹⁰⁶ Opening Report, p. 119.

¹⁰⁷ Opening Report, pp. 45-46.

¹⁰⁸ Isaacson Report, p. 21.

¹⁰⁹ Isaacson Report, p. 21.

or even to understand which states are reflected in the survey.”¹¹⁰ Based on this limitation, he concludes that “[t]he failure to account for potential differences in teen user experiences across states makes the BEEF Survey an unreliable source of data for Mr. Saba to calculate Bad Encounters on Instagram.”¹¹¹ He similarly notes that the BEEF Survey includes a “Country” field and therefore “...may have included Instagram users from countries other than the US.”¹¹²

73. While Isaacson identifies these potential geography-related issues, he does not estimate the proportion of respondents who reside outside the United States or outside the UDAP states. He does not quantify how inclusion of such respondents would affect the estimated number of encounters, nor does he assess whether their exclusion would increase or decrease the results. His critique identifies the possibility of geographic contamination but does not evaluate its magnitude or materiality.¹¹³

74. I address geography differently. In my Opening Report, I explain that my task was to calculate estimates “for each UDAP State,” stating that “[t]he following methodology was performed for each UDAP State.”¹¹⁴ I further state that I was asked “...to calculate the number of instances of Bad Encounters that Teens experienced on Instagram, using data contained in the BEEF Survey...” and that this calculation was performed “...for each MDL

¹¹⁰ Isaacson Report, p. 21.

¹¹¹ Isaacson Report, p. 21.

¹¹² Isaacson Report, p. 19.

¹¹³ Isaacson Report, pp. 19-21.

¹¹⁴ Opening Report, p. 119.

State.”¹¹⁵ In doing so, I rely on U.S. Census data and Meta-produced usage data to scale results to the relevant state populations, including “U.S. Census Data on 13-17 Year Old Population 2012-2023, by State” and “Instagram Teen Saturation 2012-2024.”¹¹⁶ There is no reason to believe that the survey respondents experience would be different across states, and Isaacson does not provide any reason. And again, the survey was designed by an expert in the field for a bona fide non-litigation purpose for Meta.

75. Isaacson does not quantify whether inclusion of non-UDAP respondents would materially alter the estimates I derive and apply to UDAP states. As presented, Isaacson’s critique raises geography-related uncertainty but does not assess its empirical effect (if any) on the encounter volumes calculated in my Opening Report. Isaacson is merely speculating that there may be an effect.

6. Isaacson criticizes the absence of time-spent controls without analyzing whether this affects aggregate exposure estimates.

76. Isaacson states that BEEF did not control for time spent on Instagram and that heavier users may have been more likely to encounter the survey and report negative experiences.¹¹⁷ He does not assess whether heavier users were more likely to participate in the survey. He does not assess whether they are an inappropriate population for estimating exposure counts, nor does he quantify whether this issue would overstate or understate aggregate encounters.

¹¹⁵ Opening Report, pp. 80, 119.

¹¹⁶ Opening Report, p. 201.

¹¹⁷ Isaacson Report, pp. 21-22.

77. In my Opening Report, I was asked to quantify the total number of instances of certain bad encounters between February 2018 and March 2024. In doing so, I applied the rate of bad encounters from the BEEF Survey responses to Teen DAP counts. I intentionally used a metric of Teen persons who were active *daily*, not monthly, knowing that the survey posed questions regarding experiences over a one-week timeframe. Daily active Teen persons would on average be heavier users than monthly active Teen Persons or less than monthly active Teen persons. Daily Active Teen persons would also represent a lower total count than monthly Teen active persons. Even if one accepts Isaacson’s assumption that heavier users were more likely to encounter the survey and report negative experiences, he does not establish that the responses would fail to reasonably reflect those of Teen DAPs to which they were applied, or that the resulting number of instances of bad encounters would be overstated. Isaacson is merely speculating that there may be an effect.

7. Isaacson challenges temporal extrapolation but does not provide an alternative adjustment or bounding methodology.

78. Isaacson criticizes extrapolating the BEEF Survey outside its June–July 2021 fielding period and ties that criticism to the view that conditions (including usage intensity) could have differed materially before and after 2021. He states, as a headline conclusion, “[t]here is no evidence that the BEEF Survey provides reliable measures across a six-year period.”¹¹⁸ He explains that using “...a survey designed to gather a snapshot of Instagram user experiences at a single time to make assumptions about other years ignores any possible

¹¹⁸ Isaacson Report, p. 23.

differences across these years.”¹¹⁹ He then grounds the temporal critique in the specific circumstances of mid-2021, stating: “[t]he BEEF Survey was fielded in June and July of 2021, when many consumers stayed home to avoid the COVID pandemic and spent more time on activities such as social media.”¹²⁰ On that basis, he asserts: “Mr. Saba has not offered any evidence that a survey conducted in 2021 can provide data relevant to consumer experiences over a six-year period.”¹²¹ He further characterizes the period as atypical, stating: “However, 2021 was a singular and unique year, and the measures from the survey may not apply to other years.”¹²²

79. I address this temporal point directly in my Opening Report, both by disclosing the assumption applied and by identifying evidence to support its reasonableness, including the continued ineffectiveness of Meta’s policy and product interventions over time. I state: “I have been asked by Counsel, for the purposes of my calculations surrounding Bad Encounters on Instagram by Teens in UDAP States, to assume that the nature, rate, and frequency of Bad Encounters during the period from February 2018 through March 2024 were consistent with those reported and measured in the BEEF Survey.”¹²³ I further write: “I find this to be a reasonable assumption.”¹²⁴ I support the reasonableness assessment by explaining: “I have seen no documents or evidence that the nature, rate, and frequency of Bad Encounters were

¹¹⁹ Isaacson Report, p. 24.

¹²⁰ Isaacson Report, p. 24.

¹²¹ Isaacson Report, p. 24.

¹²² Isaacson Report, p. 24.

¹²³ Opening Report, p. 50.

¹²⁴ Opening Report, p. 50.

substantially lower prior to the BEEF Survey or that they declined substantially following the BEEF Survey.”¹²⁵

80. In addition, I cite evidence that Meta’s policy-based and tool-based “fixes” were not adopted by users of the platform or did not work as implemented, which is directly responsive to Isaacson’s implied premise that conditions may have materially improved in other years. For example, I explain, citing Mr. Bejar’s testimony, that the “...the parental tools were opt-in features requiring users to navigate into the settings,” and that Mr. Bejar testified ““...that the feature would not be adopted and then as such would not be effective as a safety feature.””¹²⁶ I further report that Mr. Bejar testified that, when he asked safety professionals “...if anyone in the audience had used Instagram’s or [Meta’s] other products’ parental controls and found them useful...” “...nobody raised their hand.”¹²⁷ I also report testimony addressing Meta’s later “Teen Accounts” initiative, stating that Mr. Bejar testified that with Teen Accounts, Instagram “...make[s] these big promises about teen safety to parents and to teens and that the product as implemented does not live up to those promises.”¹²⁸ The Isaacson Report contains no reference to Mr. Bejar, including in Exhibit 2 – Materials Reviewed, indicating that Isaacson did not consider any of Mr. Bejar’s testimony during in his review, analysis, and development of his opinions.

81. Taken together, Isaacson’s temporal critique rests on the proposition that a short, cross-sectional 2021 survey should not be extrapolated because “...2021 was a singular and

¹²⁵ Opening Report, p. 50.

¹²⁶ Opening Report, p. 55.

¹²⁷ Opening Report, p. 55.

¹²⁸ Opening Report, p. 55.

unique year...” and the BEEF measures “...may not apply to other years.”¹²⁹ Isaacson ignores that I expressly disclosed in my Opening Report the assumption provided to me by Counsel that the nature, rate, and frequency of Bad Encounters during the period from February 2018 through March 2024 were consistent with those reported and measured in the BEEF Survey.¹³⁰ I also stated that I considered this assumption reasonable and cited evidence consistent with the persistence of the underlying conditions across time, including evidence that Meta’s policy/tool interventions were “...not ... effective as a safety feature...”¹³¹ and that Teen Accounts “...do[es] not live up to those promises...”¹³² Isaacson does not quantify how much rates would need to differ across years to materially alter aggregate encounter counts, while I cite evidence to support that there is “...no ... evidence” of substantially lower rates before the BEEF period or substantial declines afterward.¹³³

82. Again, Isaacson is merely raising the possibility of there being an issue, without showing with reasonable certainty that it is occurring, and while appearing to ignore evidence that contradicts his position even though such evidence is cited in my Opening Report.

¹²⁹ Isaacson Report, p. 24.

¹³⁰ Opening Report, p. 50.

¹³¹ Opening Report, p. 55.

¹³² Opening Report, p. 55.

¹³³ Opening Report, p. 50.

VIII. Isaacson Opinions on the Thorn Survey are Similarly Flawed.

1. Isaacson critiques the Thorn Surveys' lack of controls without quantifying the resulting error.

83. Isaacson frames his principal Thorn critique as a controls problem tied to false positives, while expressly acknowledging he cannot determine the effect size. He states: “The Thorn Surveys did not include any control measures, so survey biases and noise may inflate the measures for U13s. There is no way to determine the effect of any such flaws on the survey measures.”¹³⁴ In the Thorn section, he adds that the Thorn format “...appears to have no control,” and that the Thorn Survey Reports “...do not explain how these questions were phrased or what response options were provided...” so “...there is no way to know whether either encouraged the possibility of false positives.”¹³⁵ He then states his conclusion in terms of the absence of controls and an unknown false-positive rate: “Without a control, the Thorn data may include false positives...” and “...these kinds of responses undermine the survey’s validity, particularly because the rate of these kinds of responses is unknown.”¹³⁶

84. I support my use of the Thorn survey by quoting Meta’s own internal assessment: “Meta has characterized Thorn’s research as ‘important’ and ‘always measured, quality, and thoughtful.’”¹³⁷ I also explains that the Thorn surveys are surveys “among minors,” publishing whether 9–12-year-old respondents had “ever used” Instagram or Facebook and

¹³⁴ Isaacson Report, p. 27.

¹³⁵ Isaacson Report, pp. 28-29.

¹³⁶ Isaacson Report, p. 29.

¹³⁷ Opening Report, p. 134.

whether they used each platform “at least once per day.”¹³⁸ Finally, I note that Thorn’s design omits younger children and therefore “...likely understates...” unique U13 counts: “...minors as young as 7 were using the Facebook and Instagram platforms,” but those individuals “...would have been too young to have been included in the Thorn study,” so “this methodology likely understates the total number of unique U13 persons during the Relevant Time Period.”¹³⁹ Isaacson does not quantify the Thorn false-positive rate he posits,¹⁴⁰ nor comment on Meta’s favorable endorsement of Thorn’s quality and the likelihood of undercounting due to excluded younger ages.¹⁴¹

85. Moreover, Isaacson ignores Thorn’s detailed consideration and explanation of the survey design, which directly contradicts Isaacson’s false-positive theory: “...participants lean into more socially desirable responses...” and “...[e]ntrenched stigma and sensitivity surrounding the topic may lead to an undercounting of scale and frequency.”¹⁴²

86. In addition, I consider the Thorn survey to be different from a survey prepared specifically for litigation; rather, I consider the Thorn survey to be a contemporaneous report prepared to measure general platform use among minors. There is no reason to believe that

¹³⁸ Opening Report, p. 134.

¹³⁹ Opening Report, p. 138.

¹⁴⁰ Isaacson Report, p. 29.

¹⁴¹ Opening Report, pp. 133, 138.

¹⁴² “CHALLENGE — *Entrenched stigma and sensitivity surrounding the topic may lead to an undercounting of scale and frequency.*
Action: Asking individuals — especially kids — to open up about a subject as delicate and personal as sharing nude images of themselves likely activates self-report bias where participants lean into more socially desirable responses.” See “Self-Generated Child Sexual Abuse Material: Youth Attitudes and Experiences in 2021.” Thorn & Benenson Strategy Group. October 2022, p. 5.

that the quality of this research is lacking or that it was somehow designed to reach a biased outcome.

2. Isaacson challenges the level of methodological detail in Thorn Survey reports without concluding that the data are unusable.

87. Isaacson repeatedly criticizes the Thorn reports for missing methodological details about recruitment, panel controls, and respondent universe, and he ties that absence to an asserted inability to evaluate reliability. He states: “The Thorn Survey Reports do not describe how the survey respondents for each survey were recruited,...” beyond noting use of “...an unnamed survey panel provider...,” and he adds: “The Thorn Survey Reports provide no information regarding the identity of the panel provider...,” including “quality control measures” and procedures “...to confirm that respondents are who they claim to be (particularly in terms of variables such as age and location)...”¹⁴³ He then states the consequence in categorical terms: “Because the panel provider is unidentified, there is no way to evaluate the source of respondents from the Thorn Surveys.”¹⁴⁴ He also characterizes the Thorn reporting deficit as preventing assessment of downstream analyses: “Without this information, there is no way to evaluate the reliability of the resulting data or of any analysis that relies on these data.”¹⁴⁵

88. Again, I cite to Meta’s own engagement with Thorn. I describe that Meta had a managed relationship with Thorn: “Meta had a managed ‘relationship with Thorn,’ ‘work[ed]

¹⁴³ Isaacson Report, pp. 29-30.

¹⁴⁴ Isaacson Report, p. 30.

¹⁴⁵ Isaacson Report, p. 30.

with them weekly,’ and ‘supported them on a number of research projects in the past.’”¹⁴⁶

And I again anchor my reliance in Meta’s own characterization: “Meta has characterized

Thorn’s research as ‘important’ and ‘always measured, quality, and thoughtful.’”¹⁴⁷

Isaacson’s criticism that missing methodological detail means “...no way to evaluate...”

reliability,¹⁴⁸ ignores that Meta itself treated Thorn as a recurring research partner whose work was “quality” and “thoughtful.”¹⁴⁹

3. Isaacson’s critique of my extrapolation of U13 data from the Thorn report to other periods ignores that I had reviewed patterns for market saturation for teens and looked at relationship of teens to U13.

89. Isaacson highlights year-to-year variability in Thorn results, focusing on 2020, and he explicitly computes alternative averages with and without 2020. He states:

“...percentages of 9- to 12-year-olds who reported using Facebook and Instagram were considerably higher in 2020 than in the other four years...” and then provides the bounding arithmetic: “...the average is 31.25% if 2020 is excluded ... compared with 34.0% if 2020 is included...” for Facebook, and for Instagram “...the average is 28.75% if 2020 is excluded ... compared with 31.0% if 2020 is included...”¹⁵⁰ He uses this as part of a broader point that my analysis “...applies data from those surveys to all years in the Relevant Time Period...” including “2012 to 2018,” even though “...conditions likely changed.”¹⁵¹

¹⁴⁶ Opening Report, p. 134.

¹⁴⁷ Opening Report, p. 134.

¹⁴⁸ Isaacson Report, p. 30.

¹⁴⁹ Opening Report, pp. 133–134.

¹⁵⁰ Isaacson Report, p. 33.

¹⁵¹ Isaacson Report, p. 33.

90. Yet Isaacson appears to ignore that I used Instagram teen saturation to correct for the issue that Isaacson raises. My Opening Report describes: “For years prior to 2019, the Thorn study did not publish results for these metrics. Therefore, for Instagram, I estimate the percentage of 9 to 12 year olds on the platform with reference to Instagram’s saturation rate among Teens from 2012 to 2018.”¹⁵² I go on to state the empirical relationship used from the overlapping years: “...9-12 year olds using Instagram on a daily basis averaged approximately 35% of the platform’s Teen saturation rate from 2019 to 2023.”¹⁵³ I describe the application: “I used this percentage with reference to Instagram’s Teen saturation rate from 2012 to 2018 to calculate the percentage of 9-12 year olds using Instagram at least daily for those years.”¹⁵⁴ Isaacson misunderstands or ignores that my extrapolation is anchored to teen saturation rates, rather than being unanchored.¹⁵⁵

91. I note that my resulting estimate of the percentage of 9-12 year olds using Instagram ranges between 10% in 2012 to 27% in 2017. These are all lower than McCrary’s computation of the 28.75% survey average “if 2020 is excluded.” Furthermore, even for 2018, where my estimate is 33%, it is much closer to the Thorn 2019 survey published figure of 34%, than it is to the 2020 survey percentage of 40%.¹⁵⁶ Similarly, for Facebook, my estimated proportion of 9-12 year olds who were using the platform is 29% for 2012 to 2018, well below the Thorn survey published 45% for 2020.

¹⁵² Opening Report, p. 134.

¹⁵³ Opening Report, p. 134.

¹⁵⁴ Opening Report, pp. 134-135.

¹⁵⁵ Opening Report, pp. 134-135.

¹⁵⁶ Opening Report, Schedule 7.1.

IX. Isaacson Repeatedly Opines on How a Damages Expert May Quantify, Calculate, Extrapolate, and Draw Numerical Conclusions, Despite Disclaiming Any Opinion on Damages and Without Establishing Expertise in Damages Analysis or Reasonable-Certainty Estimation.

92. As with the BEEF survey, Isaacson repeatedly characterizes alleged limitations in the Thorn surveys as creating uncertainty of unknown magnitude, while simultaneously opining that those same limitations preclude me from calculating or drawing numerical conclusions at all. Across multiple sections of his report, Isaacson does not determine the size or direction of the effects he alleges. For example, in the Thorn discussion, he states: “There is no way to determine the effect of any such flaws on the survey measures.”¹⁵⁷ In the same discussion, he emphasizes that false positives are problematic “...particularly because the rate of these kinds of responses is unknown.”¹⁵⁸ Again, Isaacson is merely speculating that there may be an effect.

93. Isaacson’s report does not apply or discuss a reasonable-certainty standard, nor does it quantify how large the posited uncertainties would need to be in order to preclude forming numerical estimates. His critique is structured as the identification of potential sources of error, coupled with repeated acknowledgments that the magnitude or rate of those errors is “unknown” or cannot be determined,¹⁵⁹ rather than a quantified showing that estimation is impossible or unreliable as a matter of economic inference.

¹⁵⁷ Isaacson Report, p. 27.

¹⁵⁸ Isaacson Report, p. 29.

¹⁵⁹ Isaacson Report, pp. 2, 17, 27, 29.

94. At the same time, despite expressly disclaiming any opinion on damages, Isaacson repeatedly frames his conclusions in terms of whether I may calculate, quantify, extrapolate, or draw numerical conclusions at all. He states: “I am not opining on Mr. Saba’s calculations of damages in this matter. However, I am offering opinions relating to Mr. Saba’s use of the Thorn Surveys as a basis for his computations.”¹⁶⁰

95. Isaacson states: “Mr. Saba’s use of the Thorn Surveys to calculate the number of under 13 year olds (‘U13s’) using Instagram and Facebook is also inappropriate and flawed.”¹⁶¹ He later states more broadly: “The Thorn Surveys do not provide valid or reliable measures that would allow Mr. Saba to make conclusions or calculations in this matter.”¹⁶² These statements go beyond identifying survey limitations and instead assess whether a damages expert may permissibly draw numerical conclusions.

96. Isaacson further critiques my computational pathway itself. For example, he states: “I also believe that Mr. Saba used these two average usage rates ... as inputs into his calculations of U13 DAP for the years 2012 through 2018...” and then describes what I “conclude” from those inputs regarding the number of under-13 users active during the relevant period.¹⁶³ He also criticizes my aggregation and extrapolation choices, stating: “Mr. Saba did not account for possible COVID-related increases in the self-reported Instagram and Facebook usage data in 2020,” and that inclusion of those data “...resulted in higher average usage rates, which he then extrapolated to all years in the Relevant Time

¹⁶⁰ Isaacson Report, p. 27.

¹⁶¹ Isaacson Report, p. 2.

¹⁶² Isaacson Report, p. 29

¹⁶³ Isaacson Report, p. 27.

Period.”¹⁶⁴ Throughout the report, Isaacson repeatedly describes my approach as using survey data “...to attempt to calculate...” specific quantitative outputs, including that “Mr. Saba used the data from these respondents to attempt to calculate U13 DAP...” and asserts that limitations in the Thorn materials mean there is “...no way to evaluate the reliability ... of any analysis that relies on these data.”¹⁶⁵ He further criticizes the assumptions underlying my extrapolation, stating that I “...used data from these surveys to support [my] estimate...” for earlier years, and “...provide[s] no evidence to support this assumption...” and that the assumption is “likely not valid.”¹⁶⁶ Isaacson is merely speculating that there may be an effect.

97. Taken together, Isaacson repeatedly acknowledges that he does not know the magnitude of alleged bias or error, while simultaneously opining that I cannot calculate, extrapolate, or draw numerical conclusions from the data at all. Those opinions evaluate the permissibility of damages-oriented quantification, input selection, aggregation, and extrapolation, notwithstanding Isaacson’s express disclaimer that he is not opining on damages and his lack of any articulated reasonable-certainty framework. What stands out to me is that Isaacson based on his qualification does not appear to be a damages expert, does not appear to understand the reasonable certainty standard that a damages expert works under, appears to be ignorant of the fact that damages experts often work with less than perfect information and fails to provide alternative calculations or methodologies to account for the alleged biases and errors he opines may exist (without confirming their existence or magnitude, and yet speculating that there may be an effect).

¹⁶⁴ Isaacson Report, p. 34.

¹⁶⁵ Isaacson Report, pp. 27, 30.

¹⁶⁶ Isaacson Report, p. 34.

X. Rebuttal of the McCrary Report

98. As described in Section 0 of this report, the McCrary Report contains 5 opinions.

In this section, I address each of McCrary's opinions.

1. The McCrary Report does not provide affirmative opinions or alternative methodologies to aid the trier of fact.

99. The McCrary Report provides several criticisms of my calculations and methodology for which he provides no quantification of the impact of the purported flaw, or alternative methodology to aid the trier of fact. In the limited instances where the McCrary Report provides an alternative calculation, his methodology is less reliable, or no more reliable than the approaches that I have applied in my Opening Report.

100. One example of this is McCrary's criticism that I assume the BEEF Survey bad encounters are statistically independent, and that the BEEF survey results provide a reasonable approximation of the rate of occurrence of bad encounters over the Relevant Time Period. McCrary does not provide evidence that BEEF Survey bad encounters are not statistically independent, or any alternative cumulative probability calculation that would quantify the extent to which the encounters are not independent. McCrary does not provide an alternative calculation of bad encounters that would address to what extent changes in Teen usage patterns of social media or safety features rolled out on Instagram impact the rate of bad encounters during the Relevant Time Period other than when the BEEF Survey was conducted.

101. Similarly, McCrary states that my disgorgement profit calculations are conceptually flawed because I have assumed absent the alleged misconduct, Teen Users would not generate any advertising revenue. Aside from the fact that McCrary fundamentally

misunderstands the purpose of my calculation—which I address later in this report—he does not provide any alternative calculation of profits that would incorporate the but-for world he claims should apply.

102. In addition to not providing alternative calculations or quantifying the impact of several issues that McCrary purports to be flawed in my Opening Report, he provides in limited instances alternative calculations that are flawed and would be less reliable than my methodology. Examples include McCrary’s alternative calculations of how many 17-year-olds exceeded certain time spent thresholds, or his use of predicted vs. stated age to estimate what proportion of Teens are 17-year-olds. I address these criticisms and alternative calculations in the sections that follow.

2. Time spent and bad encounter calculations in my Opening Report do not assume consumer harm.

103. McCrary states that “Mr. Saba provides tallies of (i) monthly instances in which Teens on average exceed certain thresholds of time per day and (ii) instances of ‘bad encounters’ that Teens reportedly experience on Instagram, suggesting these events are associated with consumer harm.”¹⁶⁷ This statement is false, my opening report does not assume the time spent instances which I have calculated represent consumer harm. I state that MDL Plaintiffs claim that these instances may represent or be the result of violations of the UDAP statutes during the Relevant Time Period.¹⁶⁸

¹⁶⁷ McCrary Report, p. 26.

¹⁶⁸ Opening Report, p. 10.

104. In addition, my bad encounter calculations relate to alleged unlawful practices asserted by MDL Plaintiffs, such as Meta's alleged deceptions with regard to the safety of its platforms and the occurrence and disclosure of bad experiences occurring on its platforms. I understand MDL States allege that these are practices that are likely to mislead consumers and thus result in a UDAP violation, but which may not necessarily result in the harm described by McCrary. McCrary's insistence on establishing harm in every instance of my calculations is inconsistent with the purpose of my calculations, and with the assertions of UDAP violations made by MDL Plaintiffs.

3. My Opening Report does not contain any opinions on causation.

105. The McCrary Report faults my Opening Report for not linking any of my instances of time spent above certain thresholds or bad encounters to "widespread consumer harm"¹⁶⁹ or "...negative effect on the mental health of Teens in UDAP States."¹⁷⁰ In doing so, McCrary misunderstands the purpose of my report and the scope of my assignment. I was not asked in my Opening Report to conduct an analysis on Teen mental health, or to determine what factors or proof would result in a UDAP violation. Instead, as described in my Opening Report, I was asked to calculate the number of instances that would result if certain parameters (such as the amount of time spent on Facebook or Instagram) constitute a violation of the UDAP Statues. My Opening Report therefore provides calculations that are contingent on certain assumptions regarding what constitutes a violation, which were provided to me by

¹⁶⁹ McCrary Report, p. 26.

¹⁷⁰ McCrary Report, p. 26-27.

Counsel.¹⁷¹ To the extent that the MDL Plaintiffs prove their claims that Meta’s conduct violated the UDAP Statutes, my calculations provide methods for quantifying the number of instances of such violations.

106. In fact, it is accepted practice for damages experts to *assume liability*, and to quantify the damages that arise from the purported wrongful act, if reasonable to do so.¹⁷² I understand that here, the State AGs will present evidence, including the opinions of other experts, on the issues of unfair and deceptive practices and teen mental health that may relate to the violation counts in my Opening Report.

107. Another flaw with McCrary’s extended criticism in his Opinions 1 and 2 regarding my lack of analysis on Teen mental health, is that he focuses on his understanding that there must be actual “...consumer harm from the alleged conduct...”¹⁷³ to establish a violation. I understand that the MDL States allege that Meta has violated consumer protection statutes by engaging in unfair and / or deceptive acts or practices.¹⁷⁴ And that to the extent Meta did so, Meta may be ordered to pay penalties for violations.

108. Another issue with McCrary’s criticism is that he asserts: “If the vast majority of Teens experienced declining psychological health, one would expect to see a corroborating result when examining aggregate indicators of mental health among Teens in UDAP

¹⁷¹ Opening Report, pp. 9-11.

¹⁷² AICPA Forensic & Valuation Services Practice Aid, *Calculating Lost Profits* (New York: Association of International Certified Professional Accountants, 2019), Ch. 2, “Causation,” p. 10: “A plaintiff’s damages expert may be asked to assume causation for the purposes of his or her work in situations in which other evidence or expert testimony may be offered on this topic.”

¹⁷³ McCrary Report, p. 1.

¹⁷⁴ Complaint, p.3.

States.”¹⁷⁵ McCrary then claims that “...most mental health indicators have not consistently deteriorated in recent years...”¹⁷⁶ McCrary does not explain the basis for his selection of certain mental health indicators to illustrate his point. Also, importantly, McCrary’s premise is flawed. My calculations are not dependent on, and do not assume, declining Teen mental health in recent years. They also do not assume that the “vast majority of Teens” experienced declining psychological health, I note that my time spent calculations capture a subset of total Teens in the UDAP states. While the BEEF survey bad encounter instances I quantify in my Opening Report are numerous, they represent how many times Teens were exposed to such encounters over a given time period, not that the vast majority of such Teens experienced declining psychological health, an assertion that McCrary falsely attributes to my calculations.

4. My calculations use a reasonable interpretation of the BEEF Survey.

109. The McCrary Report states that the manner in which I made use of the BEEF Survey results in a flawed and overstated rate at which a respondent would experience a bad encounter. As an example, McCrary states that my extrapolation of the monthly rate at which respondents stated that they experienced a “negative comparison” is 62.4%. He compares this to the 42.5% rate at which Teens *ever* experienced negative comparisons on Instagram, and notes that my rate is higher than the *ever* rate.¹⁷⁷

110. McCrary’s assertion that one can combine two of the responses to survey questions “Yes, during the last 7 days” and “Yes, but more than 7 days ago” to determine the

¹⁷⁵ McCrary Report p.2.

¹⁷⁶ McCrary Report p.2.

¹⁷⁷ McCrary Report, p.89.

rate at which a Teen *ever* experienced a particular bad encounter is problematic for at least two reasons. Taking the bad encounter “negative comparison” as an example, the rate at which age 16-17 respondents stated they had this bad encounter “in the last 7 days” was 19.5%. The rate at which they had this bad encounter “more than 7 days ago” was 22.0%. Given that “more than 7 days ago” would cover the entire period that these Teens were on the Instagram Platform minus just the last 7 days, this could reasonably be expected to be a significantly longer time period than the “last 7 days.” Yet the survey respondents provide a similar rate at which the same bad encounter occurred “in the last 7 days” as compared to “more than 7 days ago.” The latter rate is only modestly higher than the former, for the majority of the responses to the survey, across the 13-15 age group and the 16-17 age group.

111. McCrary does not consider that one question asks the respondents to recall what bad encounters occurred very recently over a clearly defined time period (in the last week), versus what bad encounters occurred for a much longer undefined time period (for the entire time period that they have been on the Instagram platform, beyond one week). Another problem with McCrary’s desire to rely on the rate of response to “yes, more than 7 days ago” is that this response, unlike the response to “yes, during the last 7 days” is for an unknown and undefined time period for each respondent. One cannot infer the rate at which the bad encounter occurs between February 2018 and March 2024 using a “yes” response that relates to an undefined time period in the survey. For these reasons, I disagree with McCrary’s approach to interpreting the rate of bad encounters in the BEEF survey.

112. My Opening Report interprets the BEEF survey response “yes, during the last 7 days” to occur at random in each successive week within a month. For example, 20.20% of Teen respondents stated that they had encountered a “negative comparison” in the last week. I

assume in one version of my Opening Report calculations¹⁷⁸ that in each successive week, that 20.20% of respondents at random experience “negative comparison,” and therefore that there is not a correlation between the 20.20% of respondents that experience it in the first and second week of a month. McCrary criticizes my assumption of random exposure to bad encounters in portions of my calculation.¹⁷⁹ I note however, that the BEEF survey does not provide data that conveys to what extent the same Teens experience a particular bad encounter week after week. The referenced BEEF survey data does not report to what extent experiencing one type of bad encounter increases the probability of experiencing another type of bad encounter. In addition, McCrary does not conduct any analysis to establish that there is significant correlation between one type of bad encounter and another. Accordingly, the assumption of random exposure is a reasonable method of interpreting the survey within the bounds of the information that it provides.

113. In order to address the possibility that there is some correlation between the Teens who experience a particular bad encounter in each successive week during a month, my Opening report includes an alternative second calculation of bad encounter violations and penalties in Schedule 5.1.2. These calculations very conservatively assume that there is 100% correlation between the Teens who experience a particular bad encounter in each successive week. For example, of the 20.20% of respondents that experience a “negative comparison” bad encounter in the first week of each month, I very conservatively assume that the *same* 20.20% of respondents experience “negative comparison” in each successive week of the

¹⁷⁸ Opening Report, Schedule 5.1.1.

¹⁷⁹ McCrary Report, pp. 90-91.

month. Therefore, under this calculation, the weekly and monthly rate of occurrence of each bad encounter is the *same*. While this calculation likely overstates the correlation between respondents who experience bad encounters from one week to the next and therefore understates the number of affected persons, it provides a lower bound estimate of instances of bad encounters from the survey responses. This alternative calculation also results in a monthly occurrence rate that is below McCrary's "ever" rate.

114. The use of alternative calculations to arrive at a range is typical, as can be seen by the excerpt below from *Litigation Services Handbook*.¹⁸⁰

(b) Use of Ranges

Some courts allow or prefer the presentation of damages as a range of results. Because damages claims often use but-for scenarios based on analysis of events that might have occurred but for an alleged wrong, they are inherently uncertain. Some courts find the presentation of a single damages figure objectionable when a certainty of knowledge of what would have occurred is usually impossible. In these cases, practitioners often modify key assumptions to generate a range of likely outcomes based on the probabilities of various assumptions coming to pass. This also allows practitioners some flexibility in explaining how they developed their damages conclusions. They should discuss with counsel the court's conventions and preferences when deciding whether to use ranges versus specific damages figures.

115. I have not, in addition to assuming 100% correlation between the Teens who experience a particular bad encounter in each successive week, also assumed a high correlation between different types of bad encounters. Such a calculation would result in an unreasonably low estimate of the number of encounters, by making overly conservative assumptions. McCrary raises the possibility that different types of encounters may be correlated, without providing any supporting evidence or quantification of the impact to my calculations.

¹⁸⁰ Robert L. Weil, Daniel G. Lentz & Elizabeth A. Evans, *Litigation Services Handbook: The Role of the Financial Expert* (6th ed. 2017), p. 4.22.

116. McCrary also draws a comparison between the BEEF survey and a 2021 Thorn Survey as purported evidence that the BEEF survey is unreliable. He states that the Thorn survey reported that 83% of *9–17-year-olds* who had an *online sexual interaction* reacted by reporting / blocking / muting. He compares this to the BEEF survey where 66.5% of *survey respondents* who experience *unwanted sexual advances* blocked or muted the account.¹⁸¹ McCrary makes numerous inappropriate comparisons between the survey respondents in the BEEF and Thorn surveys.

117. First, McCrary appears to be comparing the *response* of respondents to certain experiences. This is separate and distinct from my analysis, which analyzed the *rate and frequency* of incidents. McCrary's comparison is entirely irrelevant to my calculations.

118. Second, the Thorn survey did not differentiate between platforms, while the BEEF Survey was specific to the Instagram platform. The Thorn survey likely includes online activity experienced by respondents on platforms other than Instagram or Facebook. For example, the Thorn survey reported activity on at least 38 different platforms, including Fortnite, Minecraft, OnlyFans, Snapchat, TikTok and YouTube. While the Thorn survey may have included participants and experiences on the Instagram platform, the BEEF Survey was *exclusively* focused on the Instagram platform.

119. Third, the McCrary inappropriately compares responses from 9-17 year olds in the Thorn Survey with *all respondents* (ages 13-45+) in the BEEF Survey. A different composition of relative age groups of respondents, among other factors, could easily explain

¹⁸¹ McCrary Report, p. 89.

the difference between rate at which they blocked or muted their account.

120. Fourth, the Thorn survey question cited by McCrary includes respondents that “... reacted by reporting.” The BEEF Survey did not include this reaction. Therefore, it is not surprising that the Thorn survey reports a higher percentage.

121. Fifth, the questions were different between the two surveys. For example, the Thorn survey asked about certain sexual interactions without asking whether the interactions were wanted.¹⁸² The BEEF Survey asked about *unwanted* advances. Therefore, not only were the categories of interactions different, but also the subjective classification required to distinguish between wanted vs. unwanted advances. Further, a participant may interpret some interactions as advances and not others. The differences between questions may contribute to different response rates.

122. Another criticism by McCrary is that I apply BEEF Survey findings to other time periods (February 2018 to March 2024) than when the BEEF Survey was conducted in July 2021. McCrary states that “Meta rolled out new features on Instagram both before and after the BEEF Survey period.”¹⁸³ McCrary then suggests that these updates may have improved user experiences, potentially rendering survey findings inapplicable to subsequent periods,

¹⁸² “[Q19B] Please indicate if you have ever experienced any of the following on a platform? Please select all that apply and remember that your answers are anonymous: ...asked me to send a nude photo or video; ...asked me to go 'on cam' with a nude or sexually explicit stream; ...shared a nude photo or video of themselves with me; ...shared a nude photo or video of another kid with me; ...sent me sexual messages.”
https://info.thorn.org/hubfs/Research/Responding%20to%20Online%20Threats_2021-Full-Report.pdf, p. 10.

¹⁸³ McCrary Report p. 91.

that may have improved the experience of its users,¹⁸⁴ and infers that such features would have reduced the rate of bad encounters relative to the BEEF survey. His inference that I ignored such features is false, because my Opening Report discusses certain features that Meta claims to have introduced to address Teen usage and well-being issues, including a time management dashboard, Take a Break, Quiet Mode, Parental Supervision Tools, and Teen Accounts.¹⁸⁵ I further discuss in my Opening Report Bejar's testimony regarding the lack of effectiveness of these features, and Mr. Sayed Otaru's testimony that affirms these are optional features that don't set hard time limits or block access to applications.¹⁸⁶

123. Regarding the specific features that McCrary cites in his report—"Take it Down" and Teen Accounts (the latter of which I mentioned in my Opening Report)—McCrary does not conduct any analysis to establish that these features had the effect of reducing bad encounters. I cite Bejar's testimony in my Opening Report in which he explained that there is no way for a teen to set an enforceable time limit through the Teen Accounts feature. In addition, the adoption rate of this feature was low, 0.18% of users predicted to be Youth and predicted to reside in the U.S. were enrolled in Teen Accounts on Facebook between April 8, 2025 and May 27, 2025.¹⁸⁷ Another problem with McCrary suggesting that these three features would reduce bad encounters, and cause my reliance on the BEEF survey to overstate bad encounter rates, is that these features were rolled out at the end of the period over which I conducted my bad encounter calculations.

¹⁸⁴ McCrary Report p. 91.

¹⁸⁵ Opening Report, p. 56.

¹⁸⁶ Opening Report pp. 61-64.

¹⁸⁷ Opening Report p. 60.

124. Appendix H of the McCrary Report lists Take it Down as having been implemented in February 2023, which is within the last year of my calculation time period. Instagram Teen Accounts are listed with an implementation date of September 2024, which is *six months after the end of my calculation period*. That means that even if these features were effective in reducing bad encounters, which McCrary has done nothing to establish, Take it Down would capture less than 20% of the time period over which my analysis was conducted, and the Teen Accounts feature would have zero impact during that period. As such, these features are largely immaterial to my analysis, and McCrary has not shown otherwise.

125. In addition to the factors discussed above, McCrary ignores other evidence that supports the applicability of BEEF Survey results to the time period from February 2018 to March 2024. In my Opening Report, I provide an analysis of trends in time usage by MDL States Teens during this period. As an example, page 73 of my Opening Report shows a chart with Teen MAU average daily time spent at the 50th to 99th percentiles for Instagram. I note that time spent is generally flat (with some fluctuations) at most percentiles between February 2018 and July 2021—the survey date. In July 2021 there is an abrupt drop in time spent which is an artifact of Meta’s data set, which introduces a large number of inactive or low activity users, as discussed in my Opening Report. After July 2021, there is a trend of rising time spent at the various percentiles through March 2024.

126. Given that the low point of time spent during this date range is on the survey date, it suggests that before and after the survey, Teens were more, not less, exposed to the Instagram platform, thus likely increasing rather than reducing the opportunities for bad encounters as compared to the survey period. Therefore, my applying rates of bad encounters from the July 2021 survey to other time periods on this chart would be more likely to result in

an undercount rather than overcount of bad encounters due to this factor. Even if one adjusts for the data anomaly in July 2021, there is no evidence that Teens were spending unusually high amounts of time on Instagram in July 2021 that far exceeded other time periods spanning my calculations.

5. McCrary's interpretation of time spent data trends is flawed.

127. McCrary erroneously suggests that my time spent charts show a decline in time spent by Teens between February 2018 and March 2024, by ignoring the impact of a regime shift in the data around July 2021. In fact, McCrary himself makes reference to this “July 2021 anomaly”¹⁸⁸ in the dataset multiple times throughout his report, yet ignores this obvious shift in the data to conclude that he sees “static or declining slopes” of the percentage of Teens that fall into each time spent thresholds.¹⁸⁹ This statement is followed by a chart in Figure 4 of the McCrary Report¹⁹⁰ that shows generally static line for California Teens in the 0.5, 1, and 2 hour thresholds between February 2018 and July 2021. There is a sharp drop in July 2021 due to the data anomaly of Meta introducing a large amount of inactive or low activity accounts into their data set. Thereafter the proportion of Teens at each time threshold are rising between July 2021 and March 2024. McCrary attempts to draw a line that is a poor fit with the data to infer a declining trend.

128. That is not an appropriate interpretation of the data due to the July 2021 introduction of a large number of additional accounts by Meta, which artificially reduces the

¹⁸⁸ McCrary Report, p. 101.

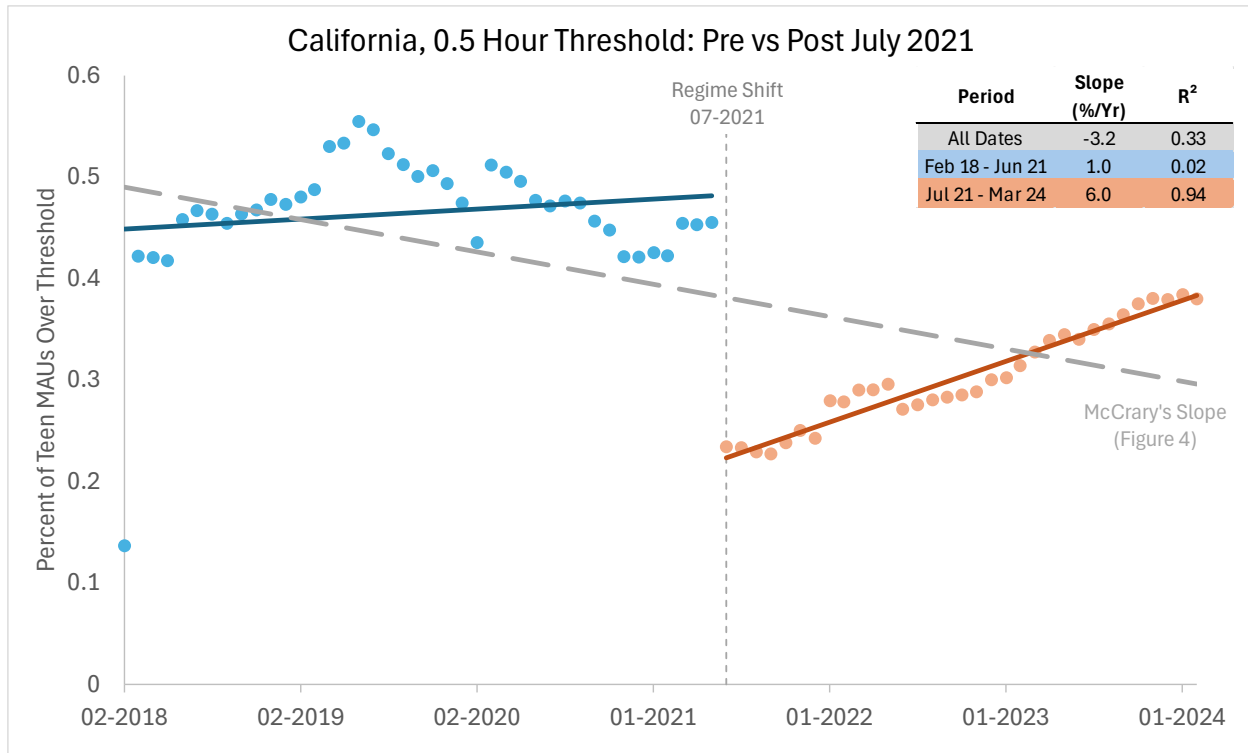
¹⁸⁹ McCrary Report, p. 38.

¹⁹⁰ McCrary Report, p. 39.

percentage of Teen Users at each time spent threshold. A more appropriate interpretation of McCrary's Figure 4 chart is to view the February 2018 – June 2021 time periods, and July 2021 – March 2024 time periods as containing two distinct and separate data sets. Viewed in this manner, the trend is flat or slightly rising between February 2018 – June 2021 (with some fluctuations) and rising between July 2021 and March 2024.

129. Below is a chart reflecting the 0.5 Hour Threshold for California—the same data McCrary presents in Figure 4 of his report.¹⁹¹ While McCrary claims that the overall trend is a downward slope of -3.2% per year, the slope indicates a 1% positive and upward trend from February 2018 to June 2021 (shown in blue) and 6% positive and upward slope from July 2021 to March 2024 (shown in orange). The grey dotted line reflects that McCrary's slope is obviously a bad fit, yet it is the basis for his erroneous and misleading opinion that the data demonstrates a downward trend for this set of users.

¹⁹¹ McCrary Report, p. 39.



130. This pattern holds true for other time spent thresholds in California as well, as shown in the table below. McCrary's calculations ignore the "July 2021 anomaly" in the dataset that he explicitly identified in his initial report. For this reason, McCrary's resulting calculations and conclusions regarding trends across each of the time-spent thresholds are flawed and unreliable.

		0.5 Hr	1 Hr	2 Hrs	3 Hrs	4 Hrs
All Dates	Slope	-3.2	-1.1	0.3	0.2	0.0
	R ²	0.33	0.12	0.05	0.12	0.01
Feb 18 - Jun 21	Slope	1.0	1.2	1.3	0.7	0.2
	R ²	0.02	0.06	0.48	0.54	0.03
Jul 21 - Mar 24	Slope	6.0	4.7	2.8	1.1	0.8
	R ²	0.94	0.92	0.86	0.83	0.55

131. In McCrary Figures 2 and 3, McCrary reports average and median time spent on Instagram and Facebook for California, New York, Illinois, and Pennsylvania. Using the

charts, he concludes that average and median time spent “...does not consistently increase over time but rather varies during the period...”¹⁹² These figures suffer from similar flaws described for McCrary Figure 4.

132. His charts do not consider the “July 2021 anomaly” which is clearly observable for Instagram. For McCrary Figures 2 and 3, there is a clear upward slope in time spent for all 4 states in both median and average after July 2021. Additionally, I note that my analysis of time spent focused on Teens with daily usage above certain thresholds and not an analysis of the average or median values.

133. Finally, as noted above, I understand that Meta will be providing a revised data set on daily average time spent on Facebook and Instagram, and that this updated data set will eliminate MAUs that have zero time spent on these platforms for any given month, as well as using a more precise approximation function for reporting percentiles in the data set. Because McCrary’s Figure 2 and Figure 3 charts include users with no time spent, these figures understate the average and median daily time spent across the Instagram and Facebook platforms.

6. Other Surveys and Studies Supporting BEEF Extrapolation

134. In addition to the BEEF Survey, other studies measuring Teens’ experiences on the Instagram platform in October 2019 and August 2025 indicate high rates of bad experiences. In the 2019 “Negative Experiences Survey” conducted by Instagram, Teens reported negative experiences they encountered on Instagram in the past week.¹⁹³ The survey

¹⁹² McCrary Report, p. 35.

¹⁹³ Deposition of Arturo Bejar, April 7, 2025, Exhibit 9.

highlighted that “...negative experiences on Instagram is quite high” and that “[o]verall, teens have significantly more negative experiences than adults.”¹⁹⁴ Notably, 27% of Teens reported witnessing “hate” within the last week in the 2019 study¹⁹⁵ as compared to 27.5% reported in the BEEF Survey in 2021.¹⁹⁶ 9% of Teens reported being bullied or harassed within the last week in the 2019 Negative Experience Survey¹⁹⁷ as compared to 10.1% in the BEEF Survey¹⁹⁸ and 26% reported seeing in nudity or sexual content in the 2019 study¹⁹⁹ as compared to 18.7% in the BEEF Survey.²⁰⁰ This reinforces the reasonableness of my assumptions regarding the BEEF Survey and supports the conservative estimates of instances computed in my Opening Report.

135. Arturo Bejar worked on a later study with Heat Initiative, a research firm, which quantified bad experiences by young teens on the Instagram platform.²⁰¹ The report concluded that young Teen users have bad encounters at “alarmingly high rates.” I note that there are certain differences between the 2025 study and the BEEF Survey. For example, the former covers user experiences in August 2025, which is 17 months from the last month of my calculations in the Opening Report. Additionally, the 2025 study was conducted on only 13-15 year olds with parental permission. The study also reported a limited set of bad

¹⁹⁴ Deposition of Arturo Bejar, April 7, 2025, Exhibit 9, pp. 6-7.

¹⁹⁵ Deposition of Arturo Bejar, April 7, 2025, Exhibit 9, p. 7.

¹⁹⁶ Opening Report, Schedule 5.3, data from BEEF Survey Respondent Data, worksheet “Issue rates by subgroups” META3047MDL-019-00099822.

¹⁹⁷ Deposition of Arturo Bejar, April 7, 2025, Exhibit 9, p. 7.

¹⁹⁸ Opening Report, Schedule 5.3.

¹⁹⁹ Deposition of Arturo Bejar, April 7, 2025, Exhibit 9, p. 7.

²⁰⁰ Opening Report, Schedule 5.3.

²⁰¹ Deposition of Arturo Bejar, December 11, 2025, 348:6-24 and Exhibit 13, p. 3.

encounter types, so there was not perfect overlap with the BEEF Survey. Nevertheless, the 2025 study indicates a high rate of bad encounters persisted among Instagram Teens well after the latest dates of my analysis.

7. McCrary misunderstands my Aged-Up Teen profit calculation in suggesting a “but-for” world.

136. The McCrary Report asserts that my “...analysis of Meta’s revenues and profits from Teens and Aged-Up Teens... is conceptually flawed and methodologically unsound.”²⁰² McCrary faults my calculations for not including a hypothetical “but-for” world, and that I assumed that “...absent Meta’s alleged violations of UDAP Statutes, Meta would not have earned any profits associated with Aged-Up Teens Users.”²⁰³

137. There are two problems with McCrary’s assertion. The first is that McCrary misunderstands my assignment, which was to calculate profits associated with Teens, and Aged-Up Teens that exceeded certain time spent thresholds. My assignment was not to calculate incremental profits relative to a “but-for” world as suggested by McCrary. I note that the calculation of Meta’s Teen revenue in my Opening Report²⁰⁴ is conservative because it applies the lower ARPU value to certain Teens (i.e., Teens with a stated age over 18), for whom Meta was earning higher ARPU.²⁰⁵ The effect of this conservative approach is that my Opening Report understates the revenue and overstates the losses associated with Teen users.

²⁰² McCrary Report, p. 3.

²⁰³ McCrary Report, p. 94.

²⁰⁴ Opening Report, Section X.2.

²⁰⁵ Meta’s analysis reflects that global revenue would be 0.6% lower if “actual teens” earned revenue based on predicted age rather than stated age. Meta’s analysis also found that “predicted youth / stated adults have roughly 40% the ARPU of average adults.” See META3047MDL-277-00007522-7526 at 7522 and 7525.

138. Secondly, McCrary does not provide any methodology or support to quantify a “but-for” world absent Meta’s alleged violations. In such a world, where Meta did not engage in Plaintiff’s alleged unfair and deceptive practices, McCrary would need to consider how that may have altered the company’s business model and operating results between 2012 and 2024. That would include impact on myriad factors, such as user acquisition and retention rates, network effects on its social media platforms, competitive position, advertiser acquisition and retention rates, and resulting revenues and cost structure.

139. McCrary suggests that I should engage in speculation on this hypothetical “but-for” world, yet he provides no plausible framework or alternative calculation to address this purported issue. He only suggests that I should have assumed certain “Teens would have just spent *less* time on Instagram and Facebook...”²⁰⁶ in the “but-for” world. In doing so, McCrary does not explain how *much less time* should have been assumed in this hypothetical, or the basis for that particular assumption, in order to quantify the impact on my calculations. McCrary also makes a faulty implicit assumption that the only difference if Meta did not engage in the alleged unfair and deceptive practices would be that the subject Teen cohorts would have spent less time on the platform, instead of considering the broader impact to the company’s business model and financial results. For example, had Teens spent less time on the platform, it may have impacted Meta’s relationships and revenue generation from advertisers, Meta’s capital spending plans, scale economies, network effects across Teens and other age groups, economic competition, market share, investment and market capitalization, acquisition and growth, among other factors. McCrary did not analyze these factors and made

²⁰⁶ McCrary Report p. 94.

the erroneous simplifying assumption that Meta's business would have remained exactly the same. Regardless, my calculations of Meta's profits from Teens and Aged-Up Teens do not require me to construct a "but-for" world.

8. McCrary's criticisms of my profit calculation methodology are misplaced, and his alternative calculations are less reliable than my Opening Report.

140. McCrary also claims that my calculations of Teen and Aged-Up Teen profits are flawed, because I did not net profits on Aged-Up Teens against losses on Teens. I have disclosed both sets of figures in my Opening Report, and the Court can easily determine the net figure by summing them, if desired or appropriate. I was asked by Counsel to present the results of Teen and Aged-Up Teen profits separately in my Opening Report, and that choice of presentation—which has no bearing my methodology—does not render the underlying calculation flawed, as McCrary asserts.

141. McCrary also finds fault with my calculation of how many 17-year-olds spend time above defined thresholds, in order to determine gross profits.²⁰⁷ He acknowledges that due to limitations in the percentile data that Meta provided on Teen average daily time spent, I am unable to calculate how many teens exceeded certain time thresholds for certain months.²⁰⁸ He states that when I calculate the average count of Teens in each month, I "...fail[] to include these months where the number of Teens is zero..."²⁰⁹ in my calculation. McCrary's statement is false, and his adjustment to my calculation to address this purported

²⁰⁷ McCrary Report, pp. 96-98.

²⁰⁸ These are instances where the time threshold is above the 99th percentile in the data set. Meta has not provided time spent data on percentiles exceeding the 99th percentile.

²⁰⁹ McCrary Report, p. 97.

issue is unsound. I explain in my Opening Report that *I cannot provide values* for how many Teens exceed certain time thresholds if they are above the 99th percentile, *not that the values are zero*.

142. McCrary’s purported correction to my calculation falsely assumes that wherever I have insufficient data to estimate Teen Users, they must necessarily all be zero. McCrary then proceeds to adjust my calculation by introducing “...zero for months previously shown to have MAU counts that were not able to be interpolated.”²¹⁰ McCrary’s resulting “corrected” calculation in Figure 22 of his report is unsound. It introduces downwards bias to gross profits by undercounting the average amount of Teens exceeding certain time thresholds in each year, by forcing arbitrary zero values into months where data is unavailable, to result in artificially low averages. Even with this unreasonable and biased assumption, McCrary’s calculations only result in a less than 3% reduction in my combined gross profits for Instagram and Facebook as shown in Figure 22.

143. Another purported issue that McCrary cites with my profits calculations is that I use the proportion of Teens that are 17 in October 2021 through December 2021 based on *stated age* variable to estimate what proportion of total Teen MAUs are 17 for 2013 – 2017, and 2018 – September 2021, where data by granular age was not provided by Meta. McCrary states that I “...could have instead used the proportion of Teens that are 17-year olds in 2022 based on the *predicted age* variable...”²¹¹ In doing so, McCrary fundamentally

²¹⁰ McCrary Report, Figure 22, footnote, p. 98.

²¹¹ McCrary Report, p. 99.

misunderstands my calculation and cherry-picks a value which has the effect of purportedly lowering profits for Aged Up Teens.

144. As described in my Opening Report and reflected in the Schedules, I consistently used the Adult Classifier and granular predicted age data when available. When these age dimensions were not produced for certain time periods and user groups, I used stated age to the extent it was produced. To calculate the percentage of 17-year-old users in 2021, I computed the percentage of 17-year-olds and 13-16-year-olds using all available data from the earliest year in which Meta provided age data. For 2021, the only available Teen age data was by stated age and being the earliest date, my assumption is reasonable.

145. McCrary suggests an alternative calculation under the assumption that using 2022 predicted age is a superior estimate of the percentage of 17-year-olds. However, McCrary's alternative calculation is flawed and misleading for multiple reasons. First, McCrary cherry-picked 3 months of data in 2022 as the source of his alternative calculations,²¹² while ignoring that 2022 predicted age results in a low-point for many states across the 2021-2024 available data. For many of the states, the percentage of 17-year-olds as compared to 13-16 years olds is lowest in the months McCrary selects, which results in an artificially low value used by McCrary. For many states, the values used in my calculations are more consistent with the trends observed within the state-by-state predicted age data. For example, in California, the 17-year-olds as a percentage of Teens were the same 25.9% for both 2021 (when only stated age data was produced) and in 2024 (using predicted age data). McCrary does not appear to

²¹² Predicted age data was supplied by Meta only starting in October 2022.

have done any trend analysis to evaluate his assumption, but rather cherry-picked 3 months in 2022, which happened to be a low-point in the trend.

146. Second, McCrary’s alternative calculation wrongly suggests that data from 2022 is more reliable than Meta’s actual reported user data from 2021. It is common practice to utilize data from the earliest time period when extrapolating calculations to earlier periods. In this case, however, McCrary suggests that one should depart from this common practice and ignore Meta’s 2021 produced age data (which is actual reported user data), ignore Meta’s 2022 produced age data for the nine months ended September of that year, and instead make the assumption that 2021 and earlier periods matched 2022 figures in the three months of October – December of that year. For this reason, McCrary’s opinion and alternative calculation is flawed.

9. My Opening Report calculations of Teen Users is reliable and does not apply a static “deflator” as asserted by McCrary.

147. The McCrary Report states that my “...estimate of Teen Users improperly includes non-human traffic and is therefore likely unreliable.”²¹³ To support this opinion, McCrary wrongly asserts that my calculation applies a static 1.35 or 1.15 deflator to estimate persons for Instagram and Facebook.²¹⁴

148. I did not apply a static deflator value as claimed by McCrary. Instead, as described in my Opening Report, I varied the deflator to account for time periods where the 1.35 and 1.15 deflator would produce MAP counts that exceed 100% of annual census data

²¹³ McCrary Report, p. 4.

²¹⁴ McCrary Report, p. 4.

for Teens in the same states and time periods.²¹⁵ I also explain that this adjustment was particularly relevant to July 2021 and subsequent months, where Instagram DAU and MAU counts are substantially increased due to the addition of a large number of inactive or low activity accounts, which McCrary asserts (without support or data) may have been caused by non-human traffic.

149. McCrary contradicts his assertion that I apply a static 1.35 or 1.15 deflator when he acknowledges later in his report that I applied “manual adjustments” to my deflator in 2021.²¹⁶ He then makes another false statement that while I adjust my deflator in 2021, I do not attempt to address the presence of Teen bot traffic (which would inflate DAU and MAU counts) in “most years.”²¹⁷ However, I described in my Opening Report that “I have applied an adjustment as needed to the user to person ratio...”²¹⁸ Furthermore, if McCrary had more carefully reviewed Schedule 2.1 to my Opening Report, he would have noted that I make adjustments to my default Instagram deflator of 1.35 for certain months in 2018, 2020, 2021, 2022, and 2024 across many of the MDL States.

150. McCrary also asserts that despite my adjustments to my universal multiplier, I fail to explain why this issue would be limited to Instagram alone.²¹⁹ In my Opening Report, I stated that “...the pattern described above for Instagram user counts does not manifest in the

²¹⁵ Opening Report, p. 126.

²¹⁶ McCrary Report, pp. 100-101.

²¹⁷ McCrary Report, pp. 100-101.

²¹⁸ Opening Report, p. 126.

²¹⁹ McCrary Report, p. 101.

Facebook user counts provided by Meta.”²²⁰ I did not see dramatic and sudden increases or decreases in MAU or DAU for Facebook, as I had identified for Instagram, which would require an adjustment to my default multiplier to yield reasonable person count estimates. In addition, the default multipliers that I developed for Instagram and Facebook are based on several data points that reflect *Meta’s own internal estimates* of user to person ratios, which would implicitly account for inflation of user accounts which McCrary claims is bot activity. The supporting data points are summarized on Schedule 2.7 in my Opening Report. I reject McCrary’s assertion that my Teen MAP counts are “tainted,” given my reliance on objective data to establish the applicable ratios, and my additional adjustments to the ratios as warranted for certain time periods.

151. In fact, despite working for Meta—which maintains MAP data—and having substantial time to request and analyze MAP data, McCrary does not rely on MAP data to substantiate his unsupported opinion on my person counts. As described in my Opening Report, Meta maintains historical MAP data in its systems and did not produce the data despite requests.²²¹ McCrary was aware, or should have been aware, of the existence of Meta’s MAP data. Nevertheless, McCrary criticized my estimates of person counts without providing an alternative methodology using data that he could have sought from Meta, to aid the trier of fact.

²²⁰ Opening Report, p. 127.

²²¹ Opening Report, p. 79.

XI. The Isaacson and McCrary Reports Contain Contradictions Within and Across their Respective Reports

152. It is important to note the glaring inconsistency between the Isaacson and McCrary reports. On one hand, Isaacson opines that the Thorn surveys are unreliable and that “...it is not possible to know how to interpret the data from the surveys.”²²² I respond to Isaacson’s criticisms in Section VIII above. On the other hand, McCrary references Thorn research to support his own opinions about bad experiences among Teens.²²³ The McCrary Report states: “...a 2021 Thorn survey reports that “83% of 9-17 year olds who had an online sexual interaction reacted [by] reporting/blocking/muting...” and that “...37% of 9-17 year olds who had an online sexual interaction told a parent/caregiver/trusted adult/peer.””²²⁴

153. McCrary’s reliance on Thorn’s research contradicts Isaacson’s contention that the Thorn studies are unreliable and deficient.

Dated: February 20, 2026



Carl S. Saba, MBA, CVA, ASA, ABV

²²² Isaacson Report, p. 31.

²²³ McCrary Report, pp. 89-90.

²²⁴ McCrary Report, pp. 89-90.

The undersigned hereby certifies their understanding that they owe a primary and overriding duty of candor and professional integrity to help the Court on matters within their expertise and in all submissions to, or testimony before, the Court. The undersigned further certifies that their report and opinions are not being presented for any improper purpose, such as to harass, cause unnecessary delay, or needlessly increase the cost of litigation.

A handwritten signature in blue ink, appearing to read "Carl Saba", is positioned above a horizontal line.

Carl S. Saba, MBA, CVA, ASA, ABV

Appendix C



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Executive Summary

Carl Saba is a Partner in the Forensic and Financial Consulting Service Group at Hemming Morse.

He is a recognized leader within the business valuation community, with over twenty nine years of experience advising companies on complex financial analysis and valuation issues for litigation, mergers and acquisitions, tax, and financial reporting matters. His valuation expertise spans business valuation, valuation of intellectual property and other intangible assets, and valuation of options and other derivatives.

Carl has led in excess of 800 valuation engagements over the last fifteen years across a broad range of industries with niche expertise in the areas of Technology, Life Sciences, and Medical Device. He has assisted clients as a valuation expert in initial public offerings, acquisitions, corporate restructure transactions, and bankruptcy reorganizations with transaction values exceeding \$1 billion. He has also assisted clients with resolving valuation disputes with the Internal Revenue Service (IRS), and addressing valuation inquiries and reviews by the Public Companies Oversight Board (PCAOB), and Securities and Exchange Commission (SEC).

On litigation matters, Carl has served as an expert and testified on a wide range of complex business disputes involving economic damages. These have included shareholder dissolution actions, business interruption, unfair competition, trade secret misappropriation, patent infringement, alter ego, lost wages, and fraud claims.

Carl also has significant financial advisory experience in mergers and acquisitions due diligence and turnaround management. He has lead due diligence efforts that have assisted his clients in negotiating key deal terms, negotiated with creditors to recapitalize companies, and helped management teams define strategic direction.

Contributing to thought leadership within the valuation community is something Carl is passionate about. He co-founded and formerly Chaired the Executive Committee of the Fair Value Forum, a business valuation expert group dedicated to defining best practices within the profession. He also served a term as President of the Valuation Roundtable of San Francisco and was a board member for several years. Carl has authored several articles on cutting edge valuation topics, and teaches and lectures on the topic frequently.

Carl has an MBA from the Marshall School of Business at the University of Southern California where he graduated with Honors. He earned his Bachelor's degree at U.C. Berkeley's Haas School of Business. He is a Certified Valuation Analyst with the National Association of Certified Valuators and Analysts. He is also an Accredited Senior Appraiser with the American Society of Appraisers, and Accredited in Business Valuation with the American Institute of Certified Public Accountants.



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Employment & Education

2013 – Present	Hemming Morse Forensic and Financial Consultants Partner
2004 – 2013	Burr Pilger Mayer, Inc. Certified Public Accountants and Consultants Shareholder, Consulting Practice Group Leader
2003 – 2004	Comerica Bank, Palo Alto Vice President / Team Leader
2003	University of Southern California MBA, Finance Emphasis – Graduated in top tier of class with honors – Extensive graduate level coursework in finance theory, valuation, options and decision analysis, statistics, and business strategy
2002	Decision Education Foundation, Menlo Park Strategy Consultant, Strategic Decisions Group (Summer Internship)
1999 – 2001	Comerica Bank, Palo Alto Vice President / Corporate Banking Officer
1996 – 1999	Manufacturers Bank, San Jose Assistant Vice President / Corporate Banking Officer
1995	University of California, Berkeley Bachelors degree in Business Administration and Finance



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American Institute of Certified Public Accountants
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American Society of Appraisers
- Certified Valuation Analyst (CVA)
National Association of Certified Valuators and Analysts
- Graduate of Leadership San Francisco
Class of 2008

Professional Affiliations

- Fair Value Forum
 - Co-Founder
 - Chair, Executive Committee, 2012-2023
 - Executive Committee, 2006-2023
- Valuation Roundtable of San Francisco
 - President, 2011-2012
 - Board Member, 2009-2014
- National Association of Certified Valuation Analysts
- American Society of Appraisers
- American Institute of Certified Public Accountants
- Community Legal Services, East Palo Alto
 - Executive Committee Board Member, 2014-2023
 - Treasurer, 2014-2023
- Beta Gamma Sigma – National Business Honor Society

Publications

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- Co-author of the valuation section of The 409A Administration Handbook, Thomson Reuters, 2013 Edition
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- “Purchase Price Allocations Under ASC 805”, A Guide to Allocating Purchase Price for Business Combinations, BPM Insights, July 2012
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Publications continued

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- "Complexities and Challenges of Business Valuation in Litigation", VPS Straight Talk Series Webcast, October 2023
- "Preparing your Business for a Successful 2022", Associated General Contractors (AGC) California, June 2022
- "Experts In Uncharted Waters", Association of Business Trial Lawyers Conference , October 2021
- "Auditing IRC 409A and ASC 805 Valuations", OUM & CO, September 2020
- "Fair Value Forum Case Study – Unpacking Differences Between Diverse Valuation Opinions", American Society of Appraisers 2018 Fair Value Summit, November 2018
- "Case Studies in 409A Valuations", American Society of Appraisers 2017 Fair Value Summit, November 2017
- "To Dissolve or Not to Dissolve, Navigating the Waters of Shareholder Disputes", Beverly Hills Bar Association, June 2017
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Instructions and Seminars continued

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- “What CPAs Should Know About Business Valuation for Estate and Gift Tax Matters”, Crawford Pimentel, January 2016
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- “Damages and Valuation for New or Unestablished Businesses”, Winston & Strawn, May 2014
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- “Auditing Fair Value Measurements under IRC 409, ASC 718, and ASC 805”, OUM & Co. LLP, September 2013
- “Valuation Issues in Chapter 11 Reorganizations, Inns of Court”, San Jose Federal Courthouse, July 2013
- “Panelist on Valuation Issues in Bankruptcy and Financial Reporting”, Association of Insolvency and Restructuring Advisors National Conference, June 2011
- “Alternative Investments, Fair Value Issues”, San Francisco Nonprofit Roundtable, 2009
- “The Guideline Public Company Valuation Method and Minority versus Control Value Conclusion”, Valuation Roundtable of San Francisco, 2009
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Instructions and Seminars continued

- “FAS 157 and Mark-to-Market or Mark-to-Make Believe Accounting?”, Golden Gate University, 2008
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- “Valuations of Early Stage Companies”, Frost, and Sullivan Medical Devices Conference, 2007
- “Complex Capital Structures – DCF with Future Capital Requirements and the Impact of Existing Shareholders”, Valuation Roundtable of San Francisco Annual Seminar, 2007
- “Audits of Investments in Private Equity Securities, Are you Ready?”, San Francisco Nonprofit Roundtable, 2007
- “Valuation and Accounting under FAS 123R”, Cal Society East Bay Business & Industry Group, 2006
- “Panelist on Implementation and Valuation Considerations Under FAS 123R”, Cal Society Life Sciences Industry Group, 2006

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- Estate of William Eric Wickersham (2025), Superior Court, Santa Clara County, California, Case No. 21PR190051
- Cleveland-Cliffs Burns Harbor LLC, et al. v. Boomerang Tube, LLC, et al. (2025), Court of Chancery, Delaware, Case No. 2022-0378-LWW
- Michael Andreacchi v. Norwest Venture Partners XIV (2025), American Arbitration Association (AAA) Santa Clara County, California, Case No. 01-24-0002-5248
- StemExpress LLC v. Molly Kent, AllCells LLC (2025), Superior Court, San Diego County, California, Case No. 37-2021-00000872-CU-NP-CTL
- Preston Clark and Growthpoint Global Inc. v. Windsail Credit Fund L.P. (2025), Superior Court, Orange County, Civil Complex Center, California, Case No. 30-2021-01191735-CU-BT-CJC
- DTL Transport, Inc. v. Jasdeep Singh Sidhu (2025), Superior Court, Fresno County, California, Case No. 22CEG00117
- Carl B. Barney v. Commissioner of Internal Revenue (2024), United States Tax Court, Los Angeles, California, Docket No. 5310-22
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Testimony continued

Trial and Arbitration continued

- Anthony Giarratano v. University of San Francisco et al. (2024), Judicate West Arbitration, San Francisco, California, Case No. A294998
- ASM Medical Ventures, LLC v. Tien Trinh, M.D.; Akira MSO, LLC. (2023), JAMS Arbitration, San Jose, California, Case No. 1130008359
- Jack Pandol v. International Fruit Genetics et al. (2023), AAA Arbitration, Bakersfield, California, Case No. 01-22-0001-1523
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- Facebook, Inc. & Subsidiaries v. Commissioner of Internal Revenue (2022), United States Tax Court, San Francisco, California, Docket No. 21959-16
- Dr. Albert Cha v. Vivo Capital, LLC and Vivo Ventures VII, LLC (2022), JAMS Arbitration, San Francisco, California, Case No. 1100110703
- Jaspindar Sandhu v. Eximius Design, LLC, et. al. (2021) JAMS Arbitration, Case No. 1100104731
- Shasikant Patel v. Nitin Desai, Town Green Enterprises, LLC, Windsor Hospitality Group. LLC (2021) JAMS Arbitration, San Francisco, California, Case No. 1100107540
- Yuhui Chen v. Zining Wu, InnoGrit Corporation, Shanghai Yingren Chuang Information Technology Co., Ltd. (2020) JAMS Arbitration, San Jose, California, Case No. 1110024169
- Omega Electric Supply, LLC, et al. v. Estate of Todd G. Lewis, et al. (2019) JAMS Arbitration, Case No. 1100091778
- David Senescu v. The Keating Group, Inc., et al. (2019), JAMS Arbitration, Case No. 1110022437
- San Jose, California Unlimited Prepaid, Inc. v. Air Voice Wireless, LLC (2018), JAMS Arbitration, Case No. 1220055749
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- Roxanne E. Doherty v. Michael Doherty (2015), Superior Court, Calaveras County, California, Case Number 11CV37584



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Testimony continued

Trial and Arbitration continued

- Lehman Brothers Holdings Inc., as Assignee of Lehman Brothers Inc. v. Christopher J. Clifford (2014), Financial Industry Regulatory Authority (FINRA), San Francisco County, California, Arbitration No. 10-04109

- Evan MacMillan v. Groupon, Inc. (2014), American Arbitration Association, San Francisco County, California, Case Number 74 460 00054 13

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- State of Tennessee v. Meta Platforms, Inc. and Instagram, LLC (2025), Chancery Court, Davidson County, Tennessee Case No. 23-1364-IV

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- **Marriage of Mark and Anne Woolway (2023)**, Superior Court, Contra Costa County, California, Case No. D20-03053
- **Jack Pandol v. International Fruit Genetics et al. (2023)**, AAA Arbitration, Bakersfield, California, Case No. 01-22-0001-1523
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- **Dr. Albert Cha v. Vivo Capital, LLC and Vivo Ventures VII, LLC (2021)**, JAMS Arbitration, San Francisco, California, Case No. 1100110703
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- **Anthony Scott Levandowski v. Uber Technologies, Inc. (2021)**, United States Bankruptcy Court, Northern District of California, San Francisco Division, Case No. 20-30242 (HLB)
- **Graystone Mortgage, LLC v. Network Funding, L.P. (2021)**, United States District Court, District of Utah Central Division, Case No. 2:19-cv-00383-JNP
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- **Yuhui Chen v. Zining Wu, InnoGrit Corporation, Shanghai Yingren Chuang Information Technology Co., Ltd. (2020)**, JAMS Arbitration, San Jose, California, Case No. 1110024169
- **Matthew Pliskin, Trustee of ICPW Nevada Trust v. BDO USA, LLP (2020)**, American Arbitration Association (AAA) Dallas, Texas, Case No. 01-19-0000-4459
- **Zwick Partners, LP and Aparna Rao v. Quorum Health Corporation, et al. (2019)**, United States District Court Middle District of Tennessee, Case No. 3:16-cv-02475
- **MD Anis Uzzaman and Fenox Venture Capital Inc. v. Brandon Hill (2019)**, Superior Court San Mateo County, California, Case No. 17-CIV-02443
- **Omega Electric Supply, LLC, et al. v. Estate of Todd G. Lewis, et al. (2019)**, JAMS Arbitration, Case No. 1100091778
- **Donald Norman v. Patrick Strateman, et al. and Intersango LLC (2019)**, Superior Court San Francisco County, California, Case Number CGC-17-556483
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- Unlimited Prepaid, Inc. v. Air Voice Wireless, LLC (2018), JAMS Arbitration, Case No. 1220055749
- Julia Bernstein, et al. v. Virgin America, Inc, et al. (2018), United States District Court, Northern District of California, Case No. 15-cv-02277-JST
- Domain Associates, L.L.C, et al. v. Nimesh S. Shah (2017), Court of Chancery Delaware, Case No. 12921-VCL
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- Lloyds TSB Bank, PLC v. Michael J. Kilroy (2015), Superior Court, Riverside County, California, Case Number INC 1202040
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- **Saul R. Flores v. Group One Construction Inc (2014)**,
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- **John K. Palladino v. John Palladino Jr. (2014)**,
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Case Number CIV512247
- **Roxanne E. Doherty v. Michael Doherty (2014)**,
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Case Number 11CV37584
- **Evan MacMillan v. Groupon, Inc. (2013)**, American
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California, Case Number 74 460 00054 13
- **Margery Raffanti v. Estate of Robert Raffanti (2010)**,
Superior Court, Santa Clara County, California
- **Scomas Restaurant, Inc. (2009)**
San Francisco County, California

Appendix D

States v. Meta

Appendix D

Rebuttal Trial Report of Carl S. Saba

List of Items Considered

Legal Filings and Depositions

Amended Complaint for Injunctive and Other Relief, 29 States and Commonwealths of United States v. Meta Platforms, Inc. et al. No. 23-CV-5448 ECF 207 (June 9, 2025)
Deposition of Kyle Andrews, November 19, 2024 and Exhibits
Deposition of Arturo Bejar, December 11, 2025 and Exhibits
Meta And State AGs' Stipulation On Data Produced In Response To Plaintiffs' Third Set Of Requests For Production, Request No. 150, February 2, 2026

Expert Reports

Expert Report of Carl S. Saba, November 21, 2025 and all Appendices, Schedules, Citations and Items Considered
Expert Report of Dr. Bruce Isaacson, January 9, 2026
Expert Report of Justin McCrary, PhD, January 9, 2026

Research Materials

AICPA Forensic and Valuation Services Practice Aid: Calculating Lost Profits, 2019
AICPA Forensic and Valuation Services Practice Aid: Attaining Reasonable Certainty in Economic Damages Calculations, 2018
Michael A. Crain, William S. Hopwood, Carl Pacini, and George R. Young, *Essentials of Forensic Accounting*, AICPA, 2015.
Roman Weil, Daniel Lenz and Elizabeth Evans, *Litigation Services Handbook: The Role of the Financial Expert*, Wiley, 6th Edition, 2017

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