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PHOENIX CENTER FOR ADVANCED LEGAL & ECONOMIC PUBLIC POLICY STUDIES

A Review of the Internet Association's Empirical Study on Network Neutrality and Investment

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In a recent paper published by the Internet Association, a trade group representing Internet edge companies, Dr. Christopher Hooton commented on my earlier work on the investment effects of the Federal Communications Commission's ("FCC") Open Internet regulations.[1] In addition, Dr. Hooton presents his own empirical study of investment effects, concluding that his analysis indicates "no (negative) impact from either the 2010 or 2015 [Net Neutrality] actions."[2] Dr. Hooton's conclusions differ materially from my research, which finds large negative impacts on telecommunications infrastructure investment following the FCC's regulatory actions in 2010 and 2015.[3]

As for Dr. Hooton's criticism of my work, I will address them in this PERSPECTIVE and demonstrate why they are invalid. Moreover, I will consider Dr. Hooton's own empirical contribution on the investment effects of Net Neutrality regulation. While Dr. Hooton's analysis is fatally flawed (as he admits), his work is important in a few respects.

First, Dr. Hooton's paper affirms the necessity of using a counterfactual analysis to assess the investment effects of Net Neutrality. None of the existing analysis of investment effects done by proponents of Net Neutrality have paid as much as lip service to the need for a counterfactual. Second, Dr. Hooton concurs with my choice of 2010 as a proper treatment date for studying Net Neutrality, stating that the "2010 treatment date is a more accurate implementation year," a choice other proponents of Net Neutrality have criticized.[4] In these two respects, Dr. Hooton certainly ups the ante on the level of analysis by pro-Net Neutrality advocacy.

… Dr. Hooton's empirical work suffers from a number of fatal and sometimes shocking defects, including making up a significant part of his data (though he readily and properly concedes this aspect of his work is "a flawed approach"). Unfortunately, all his counterfactual analysis is infected with fabricated data, leaving mostly a cursory analysis as meager as the work advanced earlier in the Net Neutrality debate.

That said, Dr. Hooton's empirical work suffers from a number of fatal and sometimes shocking defects, including making up a significant part of his data (though he concedes this aspect of his work is "a flawed approach").[5] Unfortunately, all his counterfactual analysis is infected with fabricated data, leaving mostly a cursory analysis as meager as the work advanced earlier in the Net Neutrality debate.[6]

While Dr. Hooton appears to believe a finding of "no effect" is good news for Net Neutrality, it is, in contrast, an indictment of policies ostensibly intended to spur increased broadband deployment via the Commission's "virtuous circle" theory of investment.[7] In light of the Congress's goal to expand broadband infrastructure as directed by Section 706 of the Telecommunications Act of 1996, Dr. Hooton's work points to the need for new policies for the broadband ecosystem.[8]

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First, a Response to Criticism

Dr. Hooton's paper is wide ranging, covering investment, adoption, broadband speeds, employment, and even patents, as well as taking shots at a number of earlier attempts to quantify the investment effect of Net Neutrality. His econometric analysis, and his criticisms of my work, focus on investment, so I will limit my attention to that topic. That said, I have conducted econometric work on broadband speeds and employment, both of which reveal sizeable negative effects on these outcomes from the FCC's regulatory revival.[9] Dr. Hooton does not address my analysis on those topics and his coverage of these issues is too simplistic to shed much light on the issue. The topic *du jure*, then, is investment.

Dr. Hooton's review of my work is limited to one paper: *Net Neutrality, Reclassification and Investment: A Counterfactual Analysis*.[10] His comments on that work include the following points: (a) I considered only a 2010 treatment date and not a 2015 treatment date; (b) I failed to consider "any other regulations, incentives, or business cycles that may affect the observed outcomes"; and (c) I employed an inappropriate control group.[11] I will address each in turn.

Treatment Date

Dr. Hooton is correct in noting that I did not consider a 2015 treatment date, and for good reason. The BEA data I used in my study, which is a rich dataset on investment activity in the U.S., ends in 2015.[12] Thus, it is impossible to consider a 2015 treatment date.

I confess, however, to some puzzlement regarding Dr. Hooton's criticism about using a 2015 treatment date. In his paper, he states that the "2010 treatment date is a more accurate implementation year," and observes, "any study for 2015 impacts should be interpreted cautiously given the inherent lag of infrastructure investment decisions and policy reactions."[13] His own analysis of the 2015 treatment date relies entirely on projected and not actual investment data. If a 2010 treatment date is "more accurate" and tests using a 2015 treatment date must be "interpreted cautiously" or require a "flawed approach," then it seems to me (he is arguing) that the 2010 treatment date is preferred.

In a more recent paper, *Reclassification and Investment: A Statistical Look at the 2016 Data*, I do consider a 2015 treatment date, analyzing the investment activity in 2016 with what statistical methods are appropriate for so little data.[14] I found a large and statistically-significant decline in investment in communications infrastructure. As there is only one year of data after 2015 and insufficient data to construct properly a counterfactual, the analysis should be viewed as preliminary. Nonetheless, public data on capital spending by broadband providers in 2016, including data from Net Neutrality proponent Free Press, suggest a significant decline in spending, both in terms of size and statistical significance.[15]

Potential Confounders

Any empirical analysis that involves time-series data runs the risk of conflating time with treatment. It is unavoidable. In my work, however, care was taken to address other potential determinants of investment. As is common, my regression model includes both time and sector fixed effects, both of which account for a host other factors that may influence capital spending. Fixed effects regression is the standard empirical tool to address such concerns.

Dr. Hooton mentions "interest rates" as a possible confounding factor. In fact, the period fixed effects capture the effects of broad economic dynamics like interest rates, Gross Domestic Product, and other factors that are common to telecommunications and the control sectors. It is impossible to include an interest rate variable in the presence of period fixed effects, since the interest rate variable would be collinear with the fixed effect dummies.[16] Also, my regression models sometimes include other, sector-specific factors including capital stock and sales, without any material change in the results.[17]

Finally, my controls followed similar investment trends to the telecommunications sector in the pre-treatment period, which implies the telecommunications and control sectors respond similarly to changes in broader economic activity.

Control Group Selection

In a controlled experiment, researchers are at liberty to choose their control group, setting aside a portion of their sample for a placebo. In quasi-experimental work, researchers do not have that luxury. As a consequence, controls must be carefully selected, as I did in my research. No doubt, in an ideal setting, the controls would have been BSPs not subject to Net Neutrality regulation but otherwise identical to regulated BSPs. That option was foreclosed by the broad coverage of the regulation.

Consequently, I looked to other industry sectors not affected by the regulation for controls, choosing only those sectors whose trends in investment activity paralleled that of the telecommunications sectors in the quarter-century prior to the treatment. This approach is not unlike many papers in the literature that compare outcomes across, for instance, different races. There is no theoretical incoherency in this approach. Theoretically, all that is required of the control group is that parallel paths assumption is satisfied, which simply means that the average change in the outcome variable for the telecommunications sector in the absence of treatment is equal to the equivalent average change in the outcome variable for the sectors that make up the control group.[18]

My control group was assessing using another recommended approach from the literature. As detailed in the paper, the telecommunications and control sectors had no statistically-significant difference in outcomes prior to the treatment, using the pseudo-treatment period 2005-2009.[19] This test, along with the analysis of pre-treatment trends, offers solid evidence on the validity of the control group.

As was explicit in my research, the parallel paths assumption was evaluated (though it cannot be formally tested) using visual inspection and regression tests in the pre-treatment period, as is standard in the literature. Controls included sectors from manufacturing and transportation. Through investment booms and busts, recessions and expansions, the sectors in my sample exhibited similar investment trends for twenty-five years prior to 2010. There is, consequently, no reason to expect dissimilar investment behavior in the five years following 2010, the realization of which is the only theoretical requirement for the control group.

Also, in my study, investment is measured for U.S. economic sectors only, which I consider an advantage. I naturally considered using other countries as controls (say, OECD member states), but rejected that option for numerous reasons. First, inter-country variations may introduce all sorts of complications, including the "other regulations, incentives, or business cycles" mentioned by Dr. Hooton.[20] Other nations engage in all sorts of regulatory treatments (including Net Neutrality) and have varying levels of government control of telecommunications infrastructure. The OECD investment data is also taken from the individual members states, so it is collected and reported using disparate methodologies.

Second, the data is quite limited (ending in 2013). Third, the investment trends of individual OECD member states sometimes vary widely from year to year and, with few possible exceptions, would not satisfy the parallel paths assumption.[21] Fourth, OECD membership changes over time, making the reported aggregate investment level unusable. In light of these concerns, the use of international data was dismissed. It may be possible to use the data, but it would require a great deal more analysis than offered by Dr. Hooton to render a meaningful control group.

In direct contrast to the Dr. Hooton's claim, my analysis is theoretically coherent and the control group is sensibly chosen.

Possible Other Criticisms

While not specifically directed at my work, Dr. Hooton states,

> Rather than claiming that any single analysis proves [a] policy impact [from Net Neutrality] (such as other reports have done), this paper utilizes a series of tests that approaches the question with numerous variations to build a more robust and accurate picture.[22]

I find it hard to believe this "single analysis" critique was directed at my work. Across my multiple papers on investment effects, I employ many statistical methods and subject the results to a variety of robustness checks. I've been called many things for my work (including being likened to "the gals who used to work for Heidi Fleiss in Hollywood"), but being parsimonious in my statistical analysis is not one of them.[23]

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Dr. Hooton's Investment Analysis

Dr. Hooton employs essentially two types of statistical models in his paper: the DiD estimator and regression discontinuity, both of which are based on regression models. The DiD estimator (δ) is:

$$\delta = (Y_1^T - Y_0^T) - (Y_1^U - Y_0^U)\,, \tag{1}$$

where δ is the difference-in-differences estimator, the Y^T are the outcomes of the treated group and the Y^U the control group. The subscripts 0 and 1 indicate, respectively, the outcomes before and after the treatment. The equation has three differences: (1) the difference in outcomes between two periods when a treatment is rendered in the second period; (2) the difference in outcomes between two periods when a treatment is not rendered in the second period; and (3) the difference between these two differences. Put simply, the estimator adjusts the difference for the treated group by the difference that would occur absent the treatment as measured by the control group.

Take, for instance, the effect of a drug on cholesterol. A group of 100 patients is randomly divided into a treated group of 50 persons that receive the drug and a control group of 50 persons that receive a placebo. Before the experiment, the average outcome across the two groups is equal at 200 mg/dL. After taking the drug, cholesterol is measured for the treated group and the average is found to be 160. This 40 point reduction does not quantify the causal effect of the drug, however. The control group is now observed to have an average cholesterol level of 195, a slight decline over the same period. Thus, the true causal effect of the drug is a reduction of 35 points, or

$$\delta = (160 - 200) - (195 - 200) = -35\,. \qquad (2)$$

The DiD estimation approach based on standard experimental design by relying on a counterfactual as established by the control group. Regression analysis is one method by which to calculate and test the DiD estimator.

Dr. Hooton's regression discontinuity approach, alternately, is simply a means-difference test computed using regression analysis. The resulting estimator, labeled θ, is simply

$$\theta = Y_1^T - Y_0^T\,, \qquad (3)$$

which is the first term of Equation (1). Regression discontinuity produces a difference in average outcomes without reference to a counterfactual established by the control group. The resulting estimator, θ, is thus a potentially biased estimate of the effect of interest, δ (compare Eq. 3 to Eq. 1).[24] For instance, in the hypothetical example above, Equation (3) has the treatment effect of the drug at 40 units, where in fact it is only 35 units. Only if the control group experienced no difference would the two estimators be equal.

Investment levels change, sometimes dramatically, over time, so the discontinuity approach is certain to render a biased estimate (especially if one fails to adjust for inflation). Consequently, the only potentially meaningful results offered by Dr. Hooton are from two regressions that apply a counterfactual analysis using the DiD methodology. As such, I will focus my attention largely on those results, all of which rely on projected data for the treatment period.

Put simply, Dr. Hooton has simply made his data up. In fact, these projections, possibly from multiple sources, account for 70% of his investment data during the treatment period (7 of 10 years). Consequently, Dr. Hooton's analysis has no prospect of meaningfully quantifying the investment effects of Net Neutrality, save by sheer coincidence.

DiD Statistical Analysis

Dr. Hooton's counterfactual analysis appears to rely on a mix of data, including USTelecom and OECD measures of investment, as well as his own forecasts of investments and forecasts from PricewaterhouseCoopers and Oxford Economics. Actual investment data covers the period 1996 through 2013, and forecasts are used over the period 2014-2020.[25]

Rather than limit the analysis to actual investment data or use richer datasets, Dr. Hooton chooses instead to run some regressions to produce forecasts of investment for much of the treatment period. Put simply, Dr. Hooton has simply made his data up. In fact, these projections, possibly from multiple sources, account for 70% of his investment data during the treatment period (7 of 10 years). Consequently, Dr. Hooton's analysis has no prospect of meaningfully quantifying the investment effects of Net Neutrality, save by sheer coincidence. Recognizing this fact, Dr. Hooton admits his chosen method is a "flawed approach," which is correct though perhaps a bit of an understatement.[26] He admits to the error and deserves credit for doing so.

Aside from the fatal error of using made-up data, there are other problems with the analysis. The impacts of the errors on the results are difficult to quantify given that replication, and often interpretation, of the analysis is precluded by Dr. Hooton's vague description and poor documentation of his empirical work.

Aside from the fatal error of using made-up data, there are other problems with the analysis. The impacts of the errors on the results are difficult to quantify given that replication, and often interpretation, of the analysis is precluded by Dr. Hooton's vague description and poor documentation of his empirical work. He lists, for instance, five separate data sources for his DiD analysis yet provides no clear description as to how the data is combined.[27] While Dr. Hooton claims to provide an "'apples-to-apples' comparison," his mixing of data from USTelecom, OECD, PwC, Oxford Economics, and his own forecasts, perhaps applied inconsistently between the U.S. and OECD, is a mix of not only many fruits but some meats and cheeses too.

Dr. Hooton also states that he "remov[ed] all countries that have [Net Neutrality] or have discussed it in their legislative bodies," but no listing of countries excluded (or included) is provided.[28] As for Net Neutrality, the European Union ("EU") has been working on Net Neutrality for years (affecting many non-EU members as well), as have many of the non-EU members including South Korea, Canada, Israel, and Chile.[29] What countries make up his controls is a mystery. This lack of detail is a problem given that the addition or subtraction of even one country might dramatically alter the results.

Dr. Hooton likewise fails to address the changing membership of the OECD over the sample period.[30] Chile, Estonia, Israel, the Slovak Republic, Slovakia, all joined during the sample period. For all these countries, investment-per-capita is below the OECD average (some substantially so), reducing the OECD average over time and thereby distorting the comparison to the U.S. investment data.[31]

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Perhaps most importantly, Dr. Horton fails to address the parallel paths assumption at all. In fact, the assumption is never mentioned in his paper, which is a serious omission given that the validity of the results rests upon satisfying it.

Since it is unclear what data he has used or how he has combined it, I am unable to evaluate the plausibility of parallel paths.

Perhaps most importantly, Dr. Horton fails to address the parallel paths assumption at all. In fact, the assumption is never mentioned in his paper, which is a serious omission given that the validity of the results rests upon satisfying it.

Based on his admittedly flawed analysis that relies on fabricated data, Dr. Hooton concludes there is no measureable impact of Net Neutrality on investment. Finding "no effect" may merely be the result of a poor model, bad data, or bad controls—all of which plague Dr. Hooton's analysis. Besides, the absence of evidence is not evidence of absence, and there is plenty evidence of a negative investment effect of Net Neutrality.

I admit to being somewhat perplexed by the "no investment effect" argument from pro-Net Neutrality advocates. A finding of "no effect" is nearly as bad as finding a negative effect. In light of Section 706, telecommunications policy should be aimed at encouraging infrastructure investment, not normalizing it.[32] The *2015 Open Internet Order* states that Net Neutrality will lead to "expanded investments in broadband infrastructure," which Dr. Hooton's analysis (among others) reveals is not the case.[33] Thus, Dr. Hooton's work is evidence against Net Neutrality, not for it.

Regression Results

For the sake of completeness, I will briefly review the regression results presented in Appendix B. Only Tables B1 and B2 are relevant, since the remainder of the tables summarize means difference test. It also appears Dr. Hooton did not adjust for inflation, thus conflating real investment changes with the changing value of the dollar. I also note that the regression tables are difficult to link to the description of the analysis in the text.[34] To me, the paper seems rushed and thus incomplete, which is unfortunate.

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Tables B1 and B2 summarize the DiD regressions. Since the results are based largely on projections of investment data during the treatment period, they are meaningless. Note that with a 2010 treatment data, the DiD estimator reported in Table B1 is negatively signed, though statistically insignificant. With a 2015 treatment date, the sign on the estimator switches to positive, but all the treatment-period data in this regression are projections and not actual investment levels . The sign change is not unexpected, since the near straight-line forecasts (see Figure 1) have different slopes (with the U.S. data being more positive). As detailed above, given the severe errors in the analysis, I can afford no credibility to the estimate.

The remainder of the tables report only means differences based on the chosen treatment date.

Table B3's results apply, it appears, to Kagan data on the capital stock (not investment) of cable operators. There is no indication the data was adjusted for inflation and it appears (from Figure 2) that the capital stock may be calculated (for the most part) using a deterministic linear trend. So, the results are meaningless on a variety of grounds.

For reasons unknown and unspecified, Dr. Hooton appears to be testing for a means difference after 2010 in capital spending on roads, railroads, inland waterways, maritime ports and airports. Net Neutrality does not apply to the transportation sector.

The regression summarized in Table B4 also includes U.S. investment alone as reported by USTelecom. While not stated, the treatment data is presumably 2010, since the USTelecom data ends in 2015 (and the observation count suggests no forecasts were used). None of the regressions reported are statistically significant (by the reported F-statistics). Given that, and since the model is simply a means difference calculation, the results are meaningless in terms of a policy effect.

Table B5 summarizes regression results of a most peculiar nature. The dependent variable is labeled "Total Inland Infrastructure Investment," which for the OECD measures spending on transportation infrastructure.[35] For reasons unknown and unspecified, Dr. Hooton appears to be testing for a means difference after 2010 in capital spending on roads, railroads, inland waterways, maritime ports and airports. Net Neutrality does not apply to the transportation sector. Unless Dr. Hooton has mislabeled the data, this regression is utterly pointless.[36]

In Appendix C, some figures are provided without any discussion as to what they mean, other than they are constructed using the *CausalImpact* program for the R statistical package. These causal impact procedures are sophisticated and sensitive to specification, so the lack of either a meaningful description or discussion of the analysis is unfortunate. Given the weaknesses of the regression analysis overall, I have little reason to think this "causal impact" analysis was conducted properly. In any case, the figures are uninterpreted and uninterpretable with the information provided.

It is daft to think a regulatory assault on companies that build broadband networks is the best way to get infrastructure deployed. Fortunately, the nation has survived that witless approach, and now perhaps it can shift to a largely hands-off regulatory regime suitable for the times, with a watchful eye for truly anticompetitive conduct.

In all, the empirical analyses offered by Dr. Hooton are poorly considered, weakly documented, and improperly implemented. Consequently, none of the reported results are meaningful or policy-relevant.

Conclusion

A Chinese proverb states, "the man who removes mountains begins by carrying away small stones." While the empirical analysis contained in Dr. Hooton's recent paper is flawed, at least we can appreciate the fact an advocacy shop for Net Neutrality has taken a small step in the right direction by attempting, though failing, to apply a proper counterfactual analysis to the regulation's effect on investment.

At best, Dr. Hooton's empirical analysis leads him to the conclusion that there is no measureable impact of Net Neutrality on investment. Under the FCC's "virtuous circle" theory of investment, however, Net Neutrality is intended, by some mysterious mechanism, to stimulate increased infrastructure investment.[37] Dr. Hooton, among others, have determined that it has not, suggesting it may be time for new policies.

It is daft to think a regulatory assault on companies that build broadband networks is the best way to get infrastructure deployed. Fortunately, the nation has survived that witless approach, and now perhaps it can shift to a largely hands-off regulatory regime suitable for the times, with a watchful eye for truly anticompetitive conduct. It is plainly a more sensible tact. If that path fails, then we can try something else.

NOTES:

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[1] C. Hooton, *An Empirical Investigation of the Impacts of Net Neutrality*, Internet Association (2017) (available at: https://cdn1.internetassociation.org/wp-content/uploads/2017/07/InternetAssociation_NetNeutrality-Impacts-Investigation.pdf).

[2] *Id.* at p. 14.

[3] Dr. Hooton focuses on G.S. Ford, *Net Neutrality, Reclassification and Investment: A Counterfactual Analysis*, PHOENIX CENTER POLICY PERSPECTIVE NO. 17-02 (April 25, 2017) (available at: http://phoenix-center.org/perspectives/Perspective17-02Final.pdf); *see also* G.S. Ford, *Net Neutrality, Reclassification and Investment: A Further Analysis*, PHOENIX CENTER POLICY PERSPECTIVE NO. 17-03 (May 16, 2017) (available at: http://phoenix-center.org/perspectives/Perspective17-03Final.pdf); G.S. Ford, *"Regulatory Revival" and Employment in Telecommunications,* PHOENIX CENTER POLICY PERSPECTIVE No. 17-05 (June 12, 2017) (available at: http://phoenix-center.org/perspectives/Perspective17-05Final.pdf); G.S. Ford, *Reclassification and Investment: A Statistical Look at the 2016 Data* PHOENIX CENTER POLICY PERSPECTIVE No. 17-08 (July 13, 2017) (available at: http://phoenix-center.org/perspectives/Perspective17-08Final.pdf).

[4] Hooton, *supra* n. 1 at pp. 7, 13; H. Buskirk, *Phoenix Center Renews Economic Assault on Net Neutrality Rules*, COMMUNICATIONS DAILY (May 17, 2017); J. Brodkin, *Comcast Accuses Net Neutrality Advocates of Not "Living in the Real World"*, ARSTECHNICA (July 18, 2017) (available at: https://arstechnica.com/tech-policy/2017/07/comcast-accuses-net-neutrality-advocates-of-not-living-in-the-real-world).

[5] Hooton, *supra* n. 1 at p. 10.

[6] G.S. Ford, *Below the Belt: A Review of Free Press and the Internet Association's Investment*, Claims, PHOENIX CENTER POLICY PERSPECTIVE No. 17-06 (June 20, 2017) (available at: http://phoenix-center.org/perspectives/Perspective17-06Final.pdf); G.S. Ford, *Reclassification and Investment: An Analysis of Free Press' "It's Working" Report*, PHOENIX CENTER POLICY PERSPECTIVE No. 17-04 (May 22, 2017) (available at: http://phoenix-center.org/perspectives/Perspective17-04Final.pdf); G.S. Ford, *Finding the Bottom: A Review of Free Press's Analysis of Network Neutrality and Investment*, PHOENIX CENTER PERSPECTIVE No. 09-04 (October 29, 2009) (available at: http://phoenix-center.org/perspectives/Perspective09-04Final.pdf).

[7] *See, e.g.,* G.S. Ford, *Revisiting the ''Virtuous Circle'' Two Years Later,* BLOOMBERGBNA (July 10, 2017) (available at: https://www.bna.com/revisiting-virtuous-circle-n73014461420).

[8] 47 U.S.C. § 1302.

[9] G.S. Ford, *Broadband Speeds Post-Reclassification: An Empirical Approach*, PHOENIX CENTER POLICY PERSPECTIVE No. 17-07 (June 27, 2017) (available at: http://phoenix-center.org/perspectives/Perspective17-07Final.pdf); G.S. Ford, *"Regulatory Revival" and Employment in Telecommunications*, PHOENIX CENTER POLICY PERSPECTIVE No. 17-05 (June 12, 2017) (available at: http://phoenix-center.org/perspectives/Perspective17-05Final.pdf).

[10] *Supra* n. 3.

[11] Hooton, *supra* n. 1 at p. 6.

[12] Hooton claims the Phoenix Center "itself claims the 2015 Title II ruling was unexpected and caused a sudden shift in practice for ISPs," citing the Center's Amicus Brief in *United States Telecom Association et al. v. Federal Communications Commission and United States of America* (2015) (available at: http://www.phoenix-center.org/PhoenixCenterAmicusBrief.pdf). *Id.* at p. 6. Dr. Hooton's claim is incorrect—we have never said such a thing.

[13] Hooton, *supra* n. 1 at p. 10.

[14] *Reclassification and Investment: A Statistical Look at the 2016 Data*, *supra* n. 3.

[15] P. Brogan, *Broadband Investment Heads in the Wrong Direction*, USTELECOM BLOG (May 5, 2017) (available at: http://www.ustelecom.org/blog/broadband-investment-heads-wrong-direction). More detail was provided in a telephone conservation with Patrick Brogan (May 19, 2017) including a preliminary estimate for 2016; *Annual Year-End 2016 Top-Line*

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Survey Results, CTIA (May 2017) (available at: https://www.ctia.org/docs/default-source/default-document-library/annual-year-end-2016-top-line-survey-results-final.pdf?sfvrsn=2); *Reclassification and Investment: An Analysis of Free Press' "It's Working" Report*, *supra* n. 6.

16 As a result of collinearity, either the model will not estimate or the statistical package will exclude the interest rate variable in the estimation.

17 *Net Neutrality, Reclassification and Investment: A Counterfactual Analysis*, *supra* n. 3 at pp. 9-10; *Net Neutrality, Reclassification and Investment: A Further Analysis*, *id.* at p. 3-4.

18 *See, e.g.*, B.D. Meyer, *Natural and Quasi-Experiments in Economics*, 13 JOURNAL OF BUSINESS & ECONOMIC STATISTICS 151-161 (1995); J.D. Angrist and J.S. Pischke, MOSTLY HARMLESS ECONOMETRICS: AN EMPIRICIST'S COMPANION (2008); J.D. Angrist and A.B. Krueger, *Empirical Strategies in Labor Economics*, in HANDBOOK OF LABOR ECONOMICS (Volume 3A (1999) O. Ashenfelter and D. Card, eds.) at Ch. 23; *see also* D. Card, *The Impact of the Mariel Boatlift on the Miami Labor Market*, 43 INDUSTRIAL AND LABOR RELATIONS REVIEW 245-257 (1990); S. Galiani, P. Gertler, and E. Schargrodsky, *Water for Life: The Impact of the Privatization of Water Services on Child Mortality*, 113 JOURNAL OF POLITICAL ECONOMY 83-123 (2005) (available at: http://sekhon.berkeley.edu/causalinf/papers/GalianiWater.pdf).

19 *Net Neutrality, Reclassification and Investment: A Counterfactual Analysis*, *supra* n. 3.

20 Hooton, *supra* n. 1 at p. 6.

21 South Korea is not a suitable control for numerous reasons, including the fact much of the country is supplied service over a government-funded fiber optic network, nearly half the population lives in the capitol city of Seoul, and the population density is 501 persons per square mile (versus 84 in the United States).

22 Hooton, *supra* n. 1 at p. 10.

23 H. Feld, *FCC Last Call — Part I: Cable,* WETMACHINE BLOG (December 26, 2006) (available at: http://www.wetmachine.com/tales-of-the-sausage-factory/fcc-last-call-part-i-cable/#more-676).

24 The means difference is conditional on the included factors, but there is no reason to believe the included factors establish a valid counterfactual. For a discussion of the bias of this type of regression, *see* Angrist and Pischke, *supra* n. *17* at pp. 22-3 and Ch. 3.

25 Hooton, *supra* n. 1 at p. 13.

26 *Id.* at p. 10.

27 *Id.*

28 *Id.* at p. 13.

29 *See, e.g.*, T. Williams, *Here's How Other Countries are Addressing Net Neutrality*, MARKETWATCH (November 13, 2014) (available at: http://www.marketwatch.com/story/heres-how-other-countries-are-addressing-net-neutrality-2014-11-13); L. Walker, *How is Net Neutrality Working for the Countries That Have It*, NEWSWEEK (September 10, 2014) (available at: http://www.newsweek.com/how-net-neutrality-working-countries-have-it-269632); *Net Neutrality in Various Countries*, BUSINESS STANDARD (January 8, 2016) (available at: http://www.business-standard.com/article/international/net-neutrality-in-different-countries-116010800847_1.html).

30 Accession dates for OECD members are provided here: http://www.oecd.org/about/membersandpartners/list-oecd-member-countries.htm.

31 OECD Digital Economy Outlook 2015, Table 2.31 — Public Telecommunication Investment Per Capita (available at: www.oecd.org/sti/ieconomy/deo2015data/2.31-InvestCapita.xls).

32 *Supra* n. 8.

NOTES CONTINUED:

[33] *Protecting and Promoting the Open Internet*, REPORT AND ORDER ON REMAND, DECLARATORY RULING, AND ORDER, FCC 15-24, 30 FCC Rcd 5601 (rel. Mar. 12, 2015) at ¶ 7 (hereinafter "*2015 Open Internet Order*"); *aff'd, United States Telecom Association v. FCC*, 825 F.3d 674 (D.C. Cir. 2016), *pet. for rehearing en banc denied*, 855 F.3d 381 (2017).

[34] Hooton, *supra* n. 1 at p. 13.

[35] OECD, Infrastructure Investment (available at: https://data.oecd.org/transport/infrastructure-investment.htm) ("Inland infrastructure includes road, rail, inland waterways, maritime ports and airports and takes account of all sources of financing.").

[36] Given the significant differences in the sizes of inland investment across OECD members, the data should have been transformed in some manner (natural log, or interaction terms for size) to produce sensible estimates.

[37] G.S. Ford, *Bait-and-Switch—Or Why the FCC's "Virtuous Circle" Theory is Nonsense*, BLOOMBERGBNA (May 18, 2015) (available at: http://phoenix-center.org/oped/BloombergBNAVirtuousCircle18May2015.pdf).