

In Response to the FCC...

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On April 3, 2024, the Federal Communications Commission (“FCC”) announced that it intends to vote on an item at its April Open Meeting which will reinstate, in large part, the FCC’s controversial 2015 *Open Internet Rules*.¹ Such action would reverse the Trump Administration’s 2018 *Restoring Internet Freedom Order* (“RIFO”)²—which in turn reversed the 2015 *Rules*³—and again regulate Broadband Internet Access Services (“BIAS”) as a common carrier “telecommunications service” under Title II of the Communications Act of 1934. Such action fulfills a long-standing promise of the Biden Administration.⁴

Keeping with Commission past practices, the Commission released a draft of its order in anticipation of the April Open Meeting (hereinafter “*Draft Order*”), in which the agency outlines the basis of and rules for the reinstatement of Title II regulation for broadband services. Once again, the Commission appeals to the “virtuous cycle” hypothesis of investment, whereby the onerous, open-ended Title II regulation will (somehow) increase investment in telecommunications infrastructure. The *Draft Order* devotes an entire section to investment effects of Title II regulation, and concludes that “to determine whether Title II reclassification caused the change in investment, we would need to determine what investment would have been if Title II reclassification were not adopted.”⁵ Recognizing the need for counterfactual analysis is an important insight, and it is hoped this discipline will be a part of agency analysis going forward.

The *Draft Order* also states that few studies (three, in fact) have provided such a “rigorous,” counterfactual analysis of the investment effects of Title II regulation, one of which was my 2018 paper that was published in the peer-reviewed journal APPLIED ECONOMICS entitled *Regulation and Investment in the U.S. Telecommunications Industry*, which was the foundation for the *RIFO Order*’s conclusion that Title II regulation poses a meaningful risk to telecommunications investment.⁶ Notwithstanding the *Draft Order*’s acknowledgement of the rigor of my work (two of the three “rigorous” studies), the *Draft Order* dedicates several paragraphs and asserts “several issues” with my paper that lead the Commission “to give it no probative value in this proceeding,”⁷ presumably because the evidence therein conflicts with intent to apply Title II regulation once more.

[U]sing the revised BEA data and selecting a valid control group plausibly meeting the model’s assumptions, the investment effects are found to be large, negative, and statistically different from zero.

In addition to the *Draft Order*’s critique of my APPLIED ECONOMICS paper, on April 11, 2024, after the *Draft Order* was released, Dr. Guilia McHenry of the Office of Economics and Analytics (“*OEA Letter*”) submitted a letter into the docket which presents a brief discussion of

the Commission's efforts to replicate the findings in my paper using a revision of the Bureau of Economic Analysis' ("BEA") Fixed Assets tables used in my 2018 paper data.⁸ While the Commission's analysis using these revised data does indicate the "best guess" is that Title II reduced investment by about -6.2%, this estimate is much smaller than that reported in my 2018 paper (about -20%).

In this PERSPECTIVE, I review the Commission's effort to replicate my work and the *Draft Order's* discussion of investment effects (among other things). My analysis of the *OEA Letter* may be summarized as follows: First, OEA's replication analysis erred because it used a control group which was selected based on the original BEA data. Replication requires carrying out the "entire research process." My analysis shows that the controls used by OEA do not abide by the model's assumptions. Second, using the revised BEA data and selecting a valid control group plausibly meeting the model's assumptions, the investment effects are found to be large, negative, and statistically different from zero. Third, using the revised BEA data and following the *Draft Order's* recommended Synthetic Counterfactuals method, I also find large, negative and statistically significant impacts on investment. I likewise point out several internal inconsistencies in the *Draft Order's* discussion of investment, including its criticisms of my recent paper *Investment in the Virtuous Circle: Theory and Empirics*, which uses USTelecom's investment series and the revised BEA data and finds negative effects on investment.⁹

The OEA Letter

The *OEA Letter* presents a brief (barely two-pages) summary of the Commission's efforts to replicate the findings of my APPLIED ECONOMICS paper, which used a Difference-in-Differences ("DID") model to estimate the effects of the regulation on investment. The *OEA Letter* focuses on BEA's scheduled revisions to its data, including the Fixed Assets tables upon which my work relied. Almost all the BEA data are subject

to scheduled revisions. Sometimes the changes are substantial, and in other cases immaterial. It is natural to ask, as the BEA has before, "Why didn't you get it right the first time?" To which the BEA responds, "It's not that the earlier estimate was wrong."¹⁰ The BEA data releases and revisions aim to balance accuracy and timeliness, and all such data are estimates subject to error.

[U]sing the revised BEA data and following the Draft Order's recommended Synthetic Counterfactuals method, I also find large, negative and statistically significant impacts on investment.

Over the years, the Commission has used the BEA data for a variety of purposes, and those data were also subsequently revised. For instance, the BEA data were used to set a cap on the Rural Health Care Program.¹¹ Using the current and revised BEA data, the cap was set too low by \$6 million, a small percentage change but nonetheless a large number.

While the *Draft Order* describes my work as one of two papers (ignoring my December 2023 *Investment in the Virtuous Circle* paper due to the Commission's apparent inability to compute a root mean square error which was used to construct a control group) that provides a "rigorous analysis of the effects on investment of net neutrality regulation or Title II reclassification with forbearance,"¹² the *OEA Letter* claims that the results of that paper do not survive the BEA's revisions. In describing the Commission's analysis, the *OEA Letter* states, "staff replicated Dr. Ford's analysis exactly. Therefore, the difference in estimates is due solely to the data revision."¹³ This statement contains fallacies.

To see why, note that there is a difference between *reproduction* and *replication* in statistics. As explained by Nikolopoulou (2023):

- (1) A research study is *reproducible* when the existing data is reanalyzed using the same research methods and yields the same results. This shows that the analysis was conducted fairly and correctly; and
- (2) A research study is *replicable* (or repeatable) when the entire research process is conducted again, using the same methods but new data, and still yields the same results. This shows that the results of the original study are reliable.¹⁴

The *OEA Letter* does not include the results from a *reproduction* of my work. If the Commission downloaded and analyzed the BEA data from the APPLIED ECONOMICS paper (the 2016 BEA data) and conducted the same analysis therein, then it should obtain the same results as reported in my paper. I did so, and the results are the same.¹⁵ There is no claim from OEA that my work was done incorrectly.

Likewise, the Commission's effort falls short of replication. While it used the revised BEA data (a new dataset), the Commission failed to perform the "entire research process" again. This is OEA's critical error: the "entire research process" includes finding a suitable set of controls for the new data (which may or may not be the same as with different data). The revisions to the BEA data result in material changes to telecommunications investment and most of the controls. Such changes demand an assessment of the continued validity of the control group in the new data. This the Commission did not do, and had it done so it would have found that with the revised data the old controls no longer meet the assumptions of the econometric model. As such, the *OEA Letter* contains neither reproduction nor replication, and is thus scientifically invalid and non-probative.

Empirical Framework

When applying DID analysis, it is necessary to choose a control group to serve as a counterfactual. It is essential that the selected control group for the industries of interest plausibly satisfy the parallel paths (or common trends) assumption, where the investment of the control group serves as a reliable counterfactual for the treated group during the treatment period.¹⁶ This role of the counterfactual is all that is required—the data making up the counterfactual can be obtained almost anywhere so long as they are up to that task. Even combinations of unlike industries may serve as a valid control (such as aggregations of sub-industries into larger industry groups) if their inclusion satisfies the parallel paths assumption.¹⁷ Also, it is the conditional mean of the control group that is of interest, not simply the raw individual series.¹⁸ Visual inspection of the individual series, however, offers insights into whether the conditional mean may meet the requirements of a DID Model, both in terms of overall trends and slope changes near the treatment date.

Thus, the *Draft Order's* criticisms of my control group reflect a misunderstanding of the requirements of DID models.¹⁹ In fact, the *Draft Order* rebuts itself on this criticism by stating the method of Synthetic Counterfactuals is a better approach, since a synthetic counterfactual is a mathematical fabrication (a weighted combination of multiple series) that does not appear in nature (but still a popular approach with good theoretical properties under certain conditions).²⁰

Over the years, several measures of investment have been used in discussing the effects of Title II regulation, including the BEA data, the USTelecom data, the Bureau of Labor Statistics data, various collections from the financial filings of publicly-traded broadband providers, and in some cases data that was simply made up or did not measure investment at all.²¹ Say, for conciseness, there are three sources of data on

investment levels (sources 1, 2, and 3). Let Y_i be the measure of investment from each source i , all of which are presumably correlated but not identical.

Since the dependent variable Y_i differs among data sources, so generally will the control groups, which may be labeled Z_i . Because there are three data sources for investment, there are presumably three investment/control pairs ($Y_1:Z_1$) ($Y_2:Z_2$) and ($Y_3:Z_3$). It may be that the Z_i are similar across data sources, but they need not be so.

OEA Staff conducts neither a reproduction nor replication of my analysis, as replication requires the application of the “methods” and not just haphazardly running a regression suitable for one dataset on another dataset. The Commission does not offer a new and suitable control group for the revised data.

For convenience in discussion, say my APPLIED ECONOMICS paper uses ($Y_1:Z_1$). My December 2023 paper using USTelecom and BEA data uses the pair ($Y_2:Z_2$). Both studies find sizable, negative effects on investment from Title II regulation. Let the revised BEA data contain investment data Y_3 , which is comparable to but not exactly equal to Y_1 . (The revision includes a sizable upward shift in telecommunications investment beginning around 2007, and material changes in the investment levels of the controls from that study.²²) A proper analysis requires, therefore, the pair ($Y_3:Z_3$), and $Z_1 \neq Z_3$. Unfortunately, OEA staff used the pair ($Y_3:Z_1$), apparently without checking whether this pair met the requirements for DID analysis. It does not.²³ Thus, OEA Staff conducts neither a reproduction nor replication of my analysis, as

replication requires the application of the “methods” and not just haphazardly running a regression suitable for one dataset on another dataset. The Commission does not offer a new and suitable control group for the revised data. I do so below.

Differences in Differences Model

As in my earlier papers, the econometric model used to estimate the effects of Title II regulation on investment is the traditional two-way fixed effects regression,

$$Y_{it} = \delta T_{it} \cdot P_{it} + \lambda_t + \mu_i + \varepsilon_{it} , \quad (1)$$

where Y_{it} is investment by industry i at time t , T is a treatment dummy variable, P is a post-treatment indicator, λ_t is a time fixed effect, μ_i is an industry fixed effect, and ε_{it} is the econometric disturbance term. The model is estimated by least squares and Driscoll-Kraay standard errors are computed.²⁴

The revised BEA data are used to measure investment. Telecommunications investment is measured by the Broadcasting and Telecommunications industry group. As in the prior work, the data span years 1980-2016, though the revised data extend through year 2022, and I also present results through that year. For consistency, I choose five controls that plausibly satisfy the parallel paths assumption.

A Review of Estimates

Table 1 summarizes the results of three prior estimates of the investment effects of Title II regulation. Columns A and B are from my APPLIED ECONOMICS paper and my recent *Investment in the Virtuous Circle* paper. Column C is the result from the *OEA Letter*. The APPLIED ECONOMICS paper uses the prior version of the BEA data, while the more recent paper uses the USTelecom investment series along with the revised BEA data. The *OEA Letter* relies on the revised BEA data.

Table 1. Prior Empirical Results

	AE (2018) (Y ₁ :Z ₁)	Ford (2024) (Y ₂ :Z ₂)	OEA Results (Y ₃ :Z ₁)
	[A]	[B]	[C]
δ	-0.221** (-3.59)	-0.135** (-19.69)	-0.065 (-0.96)
Observations	216	378	216

Driskoll-Kraay t-statistics in parenthesis.
Stat Sig. *** 1% ** 5% * 10%

From Column A, the coefficient -0.221 translates to a 19.8% reduction in investment, and from Column B the coefficient -0.135 indicates a 12.6% decline in investment, both relative to the counterfactual.²⁵ The null hypothesis of no effect is rejected at the 5% level or better for both. The δ coefficient in Column C, which is from the *OEA Letter* and is based on control group Z₁, is -0.065, which indicates a 6.2% reduction in investment. Thus, the Commission’s own analysis suggests that the “best guess” of the investment effect of Title II regulation is negative. While its δ coefficient is not statistically different from zero at traditional levels, about 70% of the 95% confidence interval [-0.215, 0.091] is negative.²⁶

[T]he Commission’s own analysis suggests that the “best guess” of the investment effect of Title II regulation is negative.

Revisiting the OEA Letter’s Analysis

Table 2 summarizes my reproduction of the *OEA Letter’s* analysis, using the same data and control group (Z₁). Two estimates are presented. First, I estimate the model with all years, since clustered errors address autocorrelation in the residuals, and second, I exclude year 2010 as does the *OEA Letter*. The coefficient in Column [E] is identical to the *OEA Letter*, and in Column [D] is nearly so (-0.067). Both coefficients are statistically different from zero at the 10% level, a difference due to the use of the Driscoll-Kraay standard

errors.²⁷ With few clusters and one treated cluster, the standard errors may be too small, however. Nonetheless, both coefficients are statistically different from zero at better than the 5% level when evaluated using the Wild Bootstrap.²⁸

Table 2. Reproduction of OEA Letter

	Results (Y ₃ :Z ₁)	Results (Y ₃ :Z ₁)	Results (Y ₃ :Z ₁)
	[D]	[E]	[F]
δ	-0.067** (-2.14)	-0.065* (-2.03)	...
δ’	0.081** (2.41)
Observations	222	216	186

Driskoll-Kraay t-statistics in parenthesis.
Stat Sig. *** 1% ** 5% * 10%

Table 2 also reveals the problem with OEA’s approach of using revised BEA data but failing to check whether the controls continued to be valid. While the pre-treatment growth rates are (statistically) the same between the treated and control groups, as shown in Column F the coefficient on a pseudo-treatment (years 2007-2010) is positive and large (δ’ = +0.081) and statistically significant at the 5% level (t = 2.75). Thus, Z₁ is not a suitable control group for the revised data as there is evidence of a violation of the parallel paths assumption. The *OEA Letter’s* replication effort is, therefore, unreliable.

Table 3. Corrections to OEA Letter

	Results (Y ₃ :Z ₃)	Results (Y ₃ :Z ₃)
	1980-2016	1980-2022
	[G]	[H]
δ	-0.187*** (-3.40)	-0.276*** (-5.47)
Observations	222	258

Driskoll-Kraay t-statistics in parenthesis.
Stat Sig. *** 1% ** 5% * 10%

Table 3 summarizes the results when a control group (Z₃) is chosen for the revised data. Choosing a quality control group requires a careful investigation, but a plausible group of five controls is found.²⁹ In Column G, covering

years 1980-2016, the effect of Title II on investment is decidedly negative (-0.187), statistically significant (at the 2% level using the bootstrap), and close to the estimate from the APPLIED ECONOMICS paper. The pre-trend growth rates between telecommunications and the controls are equal, and a pseudo-treatment is statistically insignificant.

Extending the sample through 2022 (Column H), the last year for which data are available, the δ coefficient is -0.276 (significant at the 1% level, including for the bootstrap). Thus, the investment consequences of Title II are worsening over time.

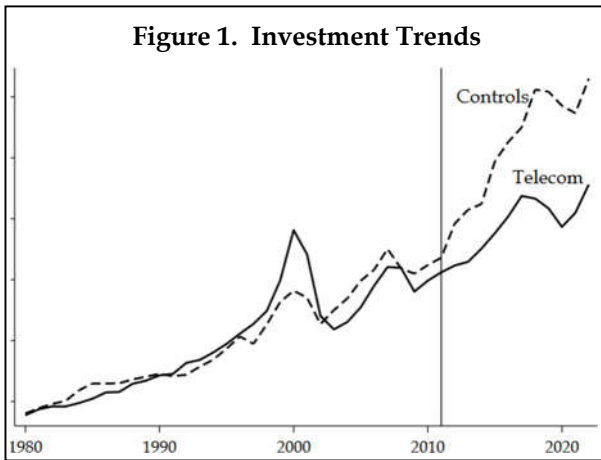


Figure 1 illustrates the investment trends of telecommunications investment relative to the controls. For illustration purposes, the data are centered. Except for the investment bubble around 2001, the controls do an excellent job of tracking telecommunications investments. After 2010, telecommunication investment is well below the counterfactual. The δ coefficient from Equation (1) measures the average effect over the two periods, though the departure of telecommunications investment from the counterfactual is relatively small in the first year or so after 2010, but then rises over time.

Synthetic Counterfactual

The estimated effects from the models presented above depend on the control groups used; these

must be carefully selected and may be criticized for both valid and/or spurious reasons. It may therefore be sensible to consider an alternative, nearly hands-off approach. The *Draft Order* suggests the method of Synthetic Counterfactuals might be preferred in this setting, stating “[t]he proper method to choose the synthetic control group to avoid these problems is to choose a weighted combination of the potential controls where the synthetic control weights are chosen to minimize the pre-treatment differences between the treatment group and the synthetic control group....”³⁰

The method of Synthetic Counterfactuals is a relatively new, and increasingly popular, method to estimate treatment effects with a single treated unit.³¹ It also avoids the problems, to some extent, of selecting a control group, though thought must be put into selecting the control pool, among other considerations.³² Hypothesis testing is non-traditional and based on placebo effects, and thus may have lower statistical power than traditional tests.

Following the *Draft Order’s* guidance, I apply the method of Synthetic Counterfactuals to the revised BEA data and allow the procedure to construct a synthetic counterfactual from the aggregated industry groups, using the Synthetic Difference-in-Differences (“SDID”) method of Arkhangelsky, et al., (2021).³³ Results are summarized in Table 4.³⁴

	1980-2016	1980-2022
	[H]	[I]
δ	-0.140* (-1.81)	-0.236** (-2.41)

p-values based on large-sample approximations.
Stat Sig. *** 1% ** 5% * 10%

The results are comparable to the traditional two-way fixed effects regression. Through 2016, the DID coefficient is -0.140, and through 2022 the coefficient is -0.236. Both coefficients are statistically significant at the 10% level or better,

though the smaller differences in the earlier years of the treatment period reduce the coefficient (a mean effect) for the sample ending in 2016. In 2011, for instance, the difference is -7%, but increases to -24% by 2014. In any case, the effect sizes are large.

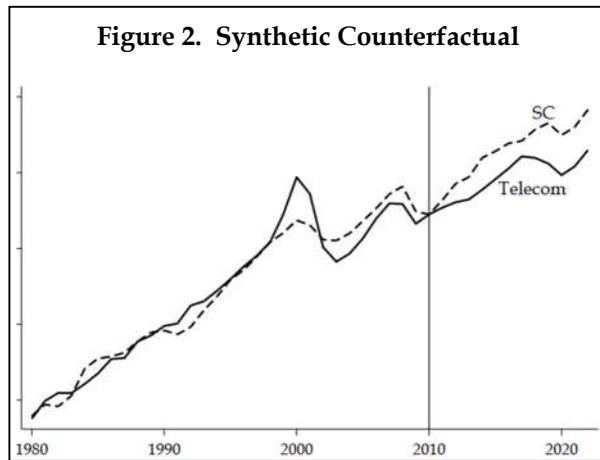


Figure 2 illustrates telecommunications investment and the synthetic counterfactual over time. The fit is quite good in the pre-treatment period, and the synthetic counterfactual (SC) is well above telecommunications investment after 2010. Note that the synthetic counterfactual is not much different than the mean of the controls illustrated in Figure 1, with smaller differences in the year treatment years.

Summary

The implications of the analysis above may be summarized as follows. First, OEA's so-called "replication" erred because it used a control group which was selected based on the original BEA data. To "replicate" the study, OEA should have carried out the "entire research process." My analysis shows (Table 2) that the controls used by OEA do not abide by the model's assumptions.

Second, using the revised BEA data and selecting a valid control group plausibly meeting the model's assumptions, I show in Table 3 that there are still large, negative effects on investment which are statistically significant.

Third, using the revised BEA data and following the *Draft Order's* recommended Synthetic Counterfactual method, I also find large, negative and statistically significant impacts on investment.

The Draft Order

The *Draft Order* devotes three paragraphs to my APPLIED ECONOMICS paper. First, it questions the validity of the control group.³⁵ As discussed above, this discussion reflects a misunderstanding of the requirements of a control group in a DID Model. If the controls, whatever they may be, provide a reasonable estimate of telecommunications investment after 2010, then they are valid. With thirty years of pre-treatment data, the reasonableness of a control can be evaluated.

Second, the *Draft Order* suggests that the BEA's Broadband and Telecommunications investment series is too inclusive.³⁶ While this industry group includes various sorts of firms, the bulk of the investment is made by telecommunications firms which provide broadband service. If other firms in this industry group were unaffected by the regulation, then the inclusion of their spending will generate random noise, which may affect the precision of the estimate but not its magnitude. To the extent the Commission feels these data are too encompassing, my recent paper *Investment in the Virtuous Circle: Theory and Empirics* uses investment data reported by USTelecom to measure telecommunications investments, and this data may be viewed as a more direct measure of broadband investment.³⁷ There is no reason, however, that the effects of Title II regulation should be limited to broadband providers alone, as other telecommunications investments are presumably complementary to investments in the network core; this is, after all, the Commission's virtuous cycle argument.

Third, the *Draft Order* mentions the revision to the BEA data, and suggests the revision nullifies the results from the APPLIED ECONOMICS paper.³⁸

This criticism is discussed at length above, and demonstrated to be incorrect.

[T]he Commission's stance is that Title II regulation will increase investment, though not a single empirical study exists to support this speculation.

The *Draft Order* also levies two criticisms of my December 2023 *Investment in the Virtuous Circle: Theory and Empirics* paper.³⁹ First, the *Draft Order* suggests that mixing the USTelecom and BEA data may be improper. Yet, datasets are mixed all the time in empirical research: datasets may cover different time periods, come from different countries, and so forth. The same criticism would apply with equal force when using any data from the Organization for Economic Cooperation and Development (“OECD”), which collects data from different nations. Yet, the Commission has no problem using the OECD data in its own analysis to serve as a control group for the United States.⁴⁰ Also, the Commission does not criticize the second of two “rigorous” analyses of investment effects, this one using OECD data, on data incompatibility reasons.⁴¹ And again, the comment demonstrates a misunderstanding of the properties of a good control group.

The Commission’s second criticism is that “staff was unable to replicate this paper due to the author’s not describing the twenty industries that were used in the control group.”⁴² While an inadvertent omission, if the Commission was serious about replicating the analysis, then it could have made a request to me for that information. It did not. Moreover, the method used to select that control group is a standard statistical procedure—something a genuine replication analysis would require in any case. Both criticisms are weak and reflect a lack of genuine intent to replicate the analysis.

More generally, the Commission’s *Draft Order* also includes several internally inconsistent statements regarding the effects of Title II on investment. Many, but not all, of these inconsistencies are revealed by considering two of the overarching statements regarding such effects.

First, there is the Commission’s hypothesis of the “virtuous cycle of Internet innovation and investment,”⁴³ which while speculative serves as the foundation of the *Draft Order’s* (as well as the *2015 Open Internet Order’s*) stance that such regulation will have positive effects on investment and innovation the broadband marketplace. In the *2015 Open Internet Order*, the Commission claims that this virtuous cycle “drives innovation and investment on the Internet” and encourages “improvements to network infrastructure.”⁴⁴ Thus, the Commission’s stance is that Title II regulation will increase investment, though not a single empirical study exists to support this speculation.

Second, the *Draft Order* states that “to determine whether Title II reclassification caused the change in investment, we would need to determine what investment would have been if Title II reclassification were not adopted.”⁴⁵ Quantifying the investment effects requires a counterfactual analysis, where actual investment under the regulation is compared to investment absent the regulation. As shown above and confirmed by the *Draft Order* and the *OEA Letter’s results*, all counterfactual analyses of the effects of Title II regulation find a negative effect on investment, so there is no evidence to support the virtuous cycle speculation or the claim of an absence of such effects.

The Commission also contradicts its own framework in the *Draft Order*. For instance, the *Draft Order* states that “the impact of reclassification on BIAS investment is uncertain, and it is unclear that there would be any impact.”⁴⁶ Yet, by the virtuous cycle, the effect

should be positive. In saying that it is “unclear there would be any impact,” the Commission rebuts its own virtuous cycle hypothesis.

It is hypocritical for the Commission to permit non-rigorous evidence in support of its policy positions while demanding rigorous evidence that conflicts with those priorities.

Moreover, the *Draft Order* states that the lack of an effect “comports with the [] available empirical evidence.”⁴⁷ The *Draft Order* contradicts this statement by referring to only three papers that “perform any type of rigorous analysis of the effects on investment of net neutrality regulation or Title II reclassification with forbearance,” all three of which find a negative effect on investment.⁴⁸

Along the same lines, the *Draft Order* criticizes the *2018 Restoring Internet Freedom Order* for concluding that Title II reduces investment incentives by arguing that the Commission “failed to consider the evidence to the contrary, including the *2015 Open Internet Order’s* evidence that investment in mobile voice and DSL thrived during the period in which they were regulated as Title II services.”⁴⁹ This statement conflicts with the *Draft Order’s* conclusions regarding how investment effects may be quantified. The contradictory “evidence” mentioned in the *Draft Order*—which the Commission specifically rejected in the *RIFO*—looked simply at investment trends without reference to a counterfactual and is thus non-rigorous and non-probative by the *Draft Order’s* own claims.⁵⁰ It is hypocritical for the Commission to permit non-rigorous evidence in support of its policy positions while demanding rigorous evidence that conflicts with those priorities.

In fact, the *Draft Order’s* statements regarding “rigorous analysis” implies that the *2018 RIFO* was correct to eschew the reliance on mere

investment trends, and instead rely on a conceptually proper, “rigorous” analysis. Indeed, the *RIFO’s* conclusions on investment relied on one of two studies the *Draft Order* viewed as a “robust analysis” of investment effects (including my APPLIED ECONOMICS paper). While the current Commission leadership views the study as “inconclusive” and guilty of “methodological errors,” the former Commission leadership viewed the evidence as useful and valid.⁵¹

The *Draft Order* also mentions, though not a critique of my work, that “network infrastructure is a long-term irreversible investment that often requires years of planning, preparation, and approvals before construction can begin.”⁵² While perhaps true, no one has argued that the broadband providers are slashing investment by half in the short term. The estimated effects span several years, including six years in my APPLIED ECONOMICS paper and twelve years in the analysis provided above. So, even if a “proper evaluation of the investment effects of Title II reclassification [] would require a longer time period in order to properly evaluate any potential effects on investment,” there are studies that do measure investment effects over a long period of time.

In Figures 1 and 2, for instance, the detrimental effects of Title II regulation are increasing over time. Moreover, investment in the industry can turn on a dime. When the recession hit in 2008, telecommunications investment fell 12%, a large and immediate reduction in capital spending. Since 1980, the range of annual changes in capital spending is $\pm 24\%$, a huge range for single year changes, and $\pm 13\%$ excluding the investment bubble in 1999-2003. Capital spending can quickly change by large amounts, but no one expects or claims the industry will abandon its networks altogether. Investment effects occur at the margin, as funds for lower return opportunities are redirected to greener grass.

The *Draft Order* also is critical of empirical studies on broadband investment for failing to “adjust for macroeconomic factors such as inflation.”⁵³ This criticism is not directed at my work. Any study that includes fixed effects for time, as all my research does, accounts for such factors.

Moving on from the empirical evidence, the *Draft Order* states that “[c]ommenters disagree as to whether reclassification of BIAS as a Title II service will discourage investment in broadband infrastructure or the Internet generally.”⁵⁴ On the one hand, AT&T—a firm that invested nearly \$18 billion in telecommunications infrastructure in 2023—and WISPA (rural wireless broadband providers that invest millions in broadband networks) state that Title II will reduce investment and innovation.⁵⁵ On the other hand, the *Draft Order* states that “[o]ther commenters argue that Title II reclassification would not reduce investment or innovation.” Who are these “other” commenters? Two political interest groups—Free Press and the National Hispanic Media Coalition—neither of which has invested a dime in broadband infrastructure nor offered the Commission a “rigorous analysis” (by the *Draft Order*’s standards) of investment effects.⁵⁶

As for economic theory, the *Draft Order* states that the “[e]conomics literature shows that net neutrality provisions may increase investment and innovation, and may have welfare-enhancing effects.”⁵⁷ The same literature—the same papers in fact—shows that net neutrality provisions may reduce investment, innovation, and welfare. The received theory is ambiguous, and cannot be otherwise; the direction of investment effects is an empirical question. Besides, none of these theoretical papers model the specificities and threats of Title II regulation, which is not the same thing as net neutrality. In fact, it appears no regulation is required for net neutrality, as we have it now in the absence of regulation. Title II regulation, therefore, is purely prophylactic, offering speculative benefits but immediate and real costs.

Perhaps recognizing the empirical cards are stacked against its claim that Title II does not reduce investment—even the OEA’s analysis finds a negative effect on investment—the Commission pivots away from its statutory obligation to increase deployment of broadband networks, instead focusing on investments from other sectors over which it has no regulatory authority, no Congressional directive, and no supporting empirical evidence. The *Draft Order* states, “we view changes in broadband investment as one of the ramifications of regulation, along with regulation’s effect on the prices and quality of broadband access and edge services, and on edge provider investment and innovation.”⁵⁸ The statement seems to imply that the Commission might be satisfied with worse broadband service and coverage in exchange for improvements in the tracking algorithms on TikTok. That is an odd stance for a telecommunications regulator tasked by Section 706 of the Telecommunications Act of 1996 to ensure broadband access for all Americans.⁵⁹ And, this more expansive view of investment effects by roping in more industries into the analysis, directly conflicts with the Commission’s criticism that the BEA data’s measure of investment is too broad. To claim that investment should be evaluated broadly while saying the BEA data are too broad—which are limited to telecommunications firms—is incongruous.

Finally, the *Draft Order*’s devotes two paragraphs to the Phoenix Center’s argument that the Commission’s no blocking and no paid prioritization rules appear to constitute a regulatory taking in violation of the Fifth Amendment to the Constitution.⁶⁰ The Commission’s critiques lack merit.

The *Draft Order* recognizes explicitly that a broadband network “is a two-sided platform with broadband customers on one side of the market and edge providers on the other.”⁶¹ The Commission states that it “has long recognized that regulating rates is not its preferred

approach,”⁶² and thus argues that because “we leave BIAS providers free to set market rates for the broadband Internet access services they offer end-users, we see no evidence that our regulatory approach ‘threaten[s] an [ISP’s] financial integrity’ and is confiscatory.”⁶³

But this statement obfuscates the truth about the Commission’s draft rules and the Phoenix Center’s argument by nakedly ignoring the D.C. Circuit’s ruling in *Verizon v. FCC*, which specifically held that the no blocking and no paid prioritization rules were in fact, zero-price regulation.⁶⁴ As Judge Silberman elaborated in his concurrence:

[W]hile there is a possibility that a “fast lane” Internet service might be offered on a non-common carriage basis, the service that most users receive under this rule [terminating traffic to end users] would still have to be offered as common carriage, at a *regulated price of zero*.⁶⁵

And it was for this exact reason (i.e., mandating that ISPs provide service at a regulated price of zero) that the D.C. Circuit remanded the Commission’s *2010 Open Internet Rules* back to the FCC for violating Section 153(51) of the Communications Act.⁶⁶

Thus, the issue is not about regulation of *retail* rates to the public; the issue is about the heart of the Commission’s rules—the regulation of the other side of the market by the no blocking and no paid prioritization rules.

Under the *Draft Order’s* rules, broadband providers are not “free to set market rates” for the second side of this two-sided market. Those rates are regulated at a price of zero, and paid prioritization is prohibited (though the zero-price rule demands no paid prioritization, since the seller-consumer could simply refuse to pay without consequence).

Moreover, the Commission’s claim that the “‘edge service’ is secondary, and in support of, the promise made to the end user,” is economic and legal nonsense.⁶⁷ First, in a two-sided

market, there is no “secondary service”; there are two, coequal services being offered. In two-sided markets, the platform may charge a non-zero price (which could be negative) to either side or both sides of the platform (i.e., buyers and/or sellers). And, broadband providers have made “no promise” to consumers regarding the second side of the market, and the Commission provides no evidence that the terms and conditions of providers’ offering include such a promise. Besides, if such a promise were made and legally binding, then there would be no need for the *Draft Order’s* rules.

The key question, therefore, is how does the Commission get a regulated rate of zero? It has neither conducted a cost analysis nor identified a ratemaking methodology (TELRIC, LIRC, etc.). By forcing broadband providers to carry traffic for free without adhering to basic ratemaking principles, the Commission’s no blocking rule appears to fit the definition of a “confiscatory” (i.e., below cost) rate in violation of the “just and reasonable” standard of Section 201.⁶⁸ Making matters worse, the Commission’s intent to forbear from Section 203 deprives broadband providers of their due process rights to challenge this apparently confiscatory rate. By failing to follow these basic ratemaking safeguards, the proposed rules are the essence of arbitrary and capricious decision making. Net Neutrality by Title II is explicitly price regulation of one side of a two-sided market; there is no escaping that basic fact, except for the failure to bring this basic disregard for due process and proper ratemaking to the attention of the reviewing courts.⁶⁹

Undeterred, the *Draft Order* also claims that “the end result” makes the Commission’s legal and economic gymnastics legitimate, as the Commission claims that its rules allow broadband providers to maintain financial integrity. Stated another way, the Commission believes that the ends justify the means. This too is legally incorrect.

First, an arbitrary and capricious decision is not legitimate simply because the resulting rate may ultimately prove to be just and reasonable. The Commission must follow basic ratemaking procedures and show its “why’s and wherefore’s”;⁷⁰ it cannot presume a rate out of thin air.

The Draft Order seeks to regulate price to zero on one side of that two-side market, and do so without specifying a ratemaking methodology or conducting a cost study. Absent a rate methodology and a study confirming financial integrity (ensuring competitive retail revenues are fully compensatory), the Commission cannot claim the no blocking rule is just and reasonable (the standard under Section 201).

Second, the Commission’s “end result” language cites to *Illinois Bell Tel. Co. v. FCC*, 988 F.2d 1254, 1260 (D.C. Cir. 1993) for support, a case which involved a dispute over the determination of a rate of return (i.e., explicit rate regulation). In appealing to a rate regulation case as authority, the Commission essentially admits that it is engaged in rate regulation. Also, in this same decision, the court observed that “[b]y long standing usage in the field of rate regulation, the ‘lowest reasonable rate’ is one which is not confiscatory in the constitutional sense. [] An agency blend of ratemaking methods so challenged is arbitrary and capricious if the expected results are so.”⁷¹ Here, we have two prices, one for each side of a two-side market. The *Draft Order* seeks to regulate price to zero on one side of that two-side market, and do so without specifying a ratemaking methodology or conducting a cost study. Absent a rate

methodology and a study confirming financial integrity (ensuring competitive retail revenues are fully compensatory), the Commission cannot claim the no blocking rule is just and reasonable (the standard under Section 201). Since the Commission did not go through the due process of setting a rate, its decision is, thus, arbitrary and capricious. Moreover, denying broadband providers their due process rights to challenge this arbitrary rate by forbearing from Section 203 raises serious Constitutional concerns.

While the Commission’s Draft Order brushes the investment effects under the rug by claiming the regulatory scheme will increase investment (or at least not reduce it), no reasonable economist believes that an overarching, open-ended regulatory framework like Title II regulation would encourage private investment.

Conclusion

All the rigorous empirical evidence finds that Title II regulation is a dark cloud hanging over the industry, reducing private incentives to invest in telecommunications networks. Temporary reprieves, such as the *2018 RIFO*, always an election away, are no consolation to investors. While the Commission’s *Draft Order* brushes the investment effects under the rug by claiming the regulatory scheme will increase investment (or at least not reduce it), no reasonable economist believes that an overarching, open-ended regulatory framework like Title II regulation would encourage private investment. While the investment effects of Title II are surely non-positive, the size of their negative effects is an empirical question, and different data and different methods will provide a range of estimates. To date, all the rigorous

evidence suggests that Title II reduces investment, and such effects may be large. While interested parties may squabble over method, there is no doubt that the preponderance of the evidence is against Title II regulation on investment grounds, and no meaningful rebuttal to these studies, or conflicting results, are available.

As detailed here, the Commission's effort to discredit my earlier work on investment through replication falls flat. The federal government frequently revises its data, and thousands of

analysts use unrevised data, even the FCC, to conduct empirical evidence and make calculations. Correcting for errors in the *OEA Letter*, which reports a negative investment effect of Title II, the revised BEA data are shown here to provide comparable results to my earlier work. The results reported in the *OEA Letter* reflect a failure to update the analysis to the revised data. Properly analyzed, including estimation using the method of Synthetic Counterfactuals as recommended in the *Draft Order*, the revised BEA data reveals large and negative investment effects.

NOTES:

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¹ *Safeguarding and Securing the Open Internet*, DECLARATORY RULING, ORDER, REPORT AND ORDER, AND ORDER ON RECONSIDERATION, FCC-CIRC2404-01 (2024) (hereinafter “Draft Order”).

² *Restoring Internet Freedom*, DECLARATORY RULING, REPORT AND ORDER, AND ORDER, 33 FCC Rcd. 311 (2018), *aff’d by, in part, vac’d by, in part, rem’d by Mozilla Corp. v. FCC*, 940 F.3d 1 (D.C. Cir. 2019).

³ *Protecting and Promoting the Open Internet*, REPORT AND ORDER ON REMAND, DECLARATORY RULING, AND ORDER, 30 FCC Rcd. 5601 (2015) (hereinafter “2015 Order”), *aff’d U.S. Telecom Ass’n v. FCC*, 825 F.3d 674 (D.C. Cir. 2016), *reh’g en banc denied* 855 F.3d 381 (2017).

⁴ Biden Executive Order on Competition, EXEC. ORDER NO. 14036, 86 FED. REG. 36987 (July 14, 2021); *see also* L.J. Spiwak, *The FCC Returns to the Law and Economics Free Zone*, FEDERALIST SOCIETY BLOG (November 20, 2023) (available at: <https://fedsoc.org/commentary/fedsoc-blog/the-fcc-returns-to-the-law-and-economics-free-zone>).

⁵ *Draft Order*, *supra* n. 1 at ¶ 284.

⁶ G.S. Ford, *Regulation and Investment in the U.S. Telecommunications Industry*, 50 APPLIED ECONOMICS 6073–6084 (2018).

⁷ *Draft Order*, *supra* n. 1 at ¶ 287.

⁸ Letter from Giulia McHenry, Chief, Office of Economics and Analytics, WC Docket No. 23-320 (April 11, 2024) (available at: <https://www.fcc.gov/ecfs/document/10411227464732/1>).

⁹ G.S. Ford, *Investment in the Virtuous Circle: Theory and Empirics*, PHOENIX CENTER POLICY PAPER No. 62 (December 2023) (available at: <https://phoenix-center.org/pcpp/PCPP62Final.pdf>).

¹⁰ *Revising Economic Indicators: Here’s Why the Numbers Can Change*, Bureau of Economic Analysis (July 8, 2013) (available at: <https://www.bea.gov/news/blog/2013-07-08/revising-economic-indicators-heres-why-numbers-can-change>).

¹¹ *In the Matter of Promoting Telehealth in Rural America*, FCC 18-82, REPORT AND ORDER, 33 FCC Rcd. 6574 (rel. June 25, 2018), at ft. 47 (available at: <https://www.fcc.gov/ecfs/document/06252124507219/6>). Data available at: <https://fred.stlouisfed.org/series/GDPCTPI#0>.

¹² *Draft Order*, *supra* n. 1 at ¶ 286.

¹³ *OEA Letter*, *supra* n. 8 at ft. 5.

¹⁴ K. Nikolopoulou, *Reproducibility vs Replicability | Difference & Examples*, SCRIBBR (June 22, 2023) (available at: <https://www.scribbr.com/methodology/reproducibility-repeatability-replicability>).

¹⁵ The *OEA Letter* provides the link for the older BEA data: https://apps.bea.gov/histdata/Releases/FA/2016/AnnualUpdate_August-23-2017/Section3ALL.xls.xls. It seems reasonable, therefore, to suppose the OEA did reproduce the work.

¹⁶ There is no direct test of the parallel paths assumption, but there are several ways to evaluate its plausibility.

¹⁷ For example, “Food and Beverage and Tobacco Products” and “Petroleum and Coal Products” are both members of the “Nondurable Goods Manufacturing” industry group. Simulated industries could, in fact, be created, which is exactly the mechanism of the method of Synthetic Counterfactuals.

¹⁸ *See, e.g.*, J.D. Angrist & J. Pischke, *MASTERING METRICS: THE PATH FROM CAUSE TO EFFECT* (2015), at Ch. 5.

¹⁹ *Draft Order*, *supra* n. 1 at ¶ 288-9 (“it is not clear why this diverse set of industries with very different technology and productivity shocks would be an appropriate control group for telecommunications.”).

²⁰ *Id.* at ¶ 288.

NOTES CONTINUED:

- ²¹ *Investment in the Virtuous Circle*, *supra* n. 9; G.S. Ford, *Net Neutrality and Investment in the U.S.: A Review of Evidence from the 2018 Restoring Internet Freedom Order*, 17 REVIEW OF NETWORK ECONOMICS 175-205 (2018); G. S. Ford, *Does Title II Reduce Infrastructure Investment? Repairing Hooton's Analysis*, PHOENIX CENTER POLICY PERSPECTIVE No. 19-06 (October 15, 2019) (available at: <https://www.phoenix-center.org/perspectives/Perspective19-06Final.pdf>); G.S. Ford, *A Review of the Internet Association's Empirical Study on Network Neutrality and Investment*, PHOENIX CENTER POLICY PERSPECTIVE No. 17-09 (July 24, 2017) (available at: <https://www.phoenix-center.org/perspectives/Perspective17-09Final.pdf>) (“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).”); G.S. Ford, *A Further Review of the Internet Association's Empirical Study on Network Neutrality and Investment*, PHOENIX CENTER POLICY PERSPECTIVE No. 17-10 (August 14, 2017) (available at: <https://www.phoenix-center.org/perspectives/Perspective17-10Final.pdf>) (“Dr. Hooton's analysis of USTelecom's data on U.S. broadband investment for years 1996 through 2015 employs data that have been corrupted in some way. Dr. Hooton's results are not consistent with the actual USTelecom data, a fact easily demonstrated”); C.A. Hooton, *Testing the Economics of the Net Neutrality Debate*, 44 TELECOMMUNICATIONS POLICY 101869 (2020); G.S. Ford, *Testing the Economics of the Net Neutrality Debate: A Comment*, 45 TELECOMMUNICATIONS POLICY 102137 (2021); Editors, *Expression of concern: Testing the Economics of the Net Neutrality Debate*, 44 TELECOMMUNICATIONS POLICY 102005 (2020).
- ²² One possibility for the changes is that the later BEA data includes more industry groups (79 in the 2016 data, 96 in the latest data), which may have diverted portions of investment for the relevant industries into different categories.
- ²³ It appears the Commission recognized this problem prior to the release of the *OEA Letter*. The *Draft Order* observes, presumably based on the revised BEA data, that “[v]isual inspection comparing the pre-2010 (pretreatment) investment trends of the synthetic control industries with the trends in telecommunications and broadcasting investment confirm that the controls are inappropriately chosen.” *Draft Order*, *supra* n. 1 at ¶ 288.
- ²⁴ J.C. Driscoll and A.C. Kraay, *Consistent Covariance Matrix Estimation with Spatially Dependent Panel Data*, 80 REVIEW OF ECONOMICS AND STATISTICS 549-560 (1998).
- ²⁵ The percentage change in the dependent variable is $\exp(\delta) - 1$.
- ²⁶ *Draft Order*, *supra* n. 1 at ¶ 274.
- ²⁷ The typical clustered standard errors provide a t-statistic of -1.04, which is close the *OEA Letter's* t-statistic of -0.96, which may be explained by differences in the estimation software or command.
- ²⁸ A.C. Cameron, J.B. Gelbach, and D.L. Miller, *Bootstrap-based Improvements for Inference with Clustered Errors*, 90 THE REVIEW OF ECONOMICS AND STATISTICS 414-27 (2008); A. Abadie, A. Diamond, and J. Hainmueller, *Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program*, 105 JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION 493-505 (2010).
- ²⁹ The control industries include: Nondurable Goods Manufacturing; Other Manufacturing; Transit and Ground Passenger Transportation; Utilities; and Wholesale Nondurable Goods. The Transportation and Warehousing industry aggregate is excluded since the Commission was skeptical of it. When choosing a control group, the analysis considers only the pre-treatment data. Here, I use visual inspection and some descriptive statistics to select a suitable control group.
- ³⁰ *Draft Order*, *supra* n. 1 at ¶ 288. The *Draft Order* also states that my APPLIED ECONOMICS paper “constructs what is known as a “synthetic control group.” *Id.* at ¶288. It does not. The method of Synthetic Counterfactual creates a single counterfactual from many members of a control pool, my study uses all the controls in estimation. A proper statement would be that the paper selects industries for the control group that plausibly satisfy the parallel paths assumption.
- ³¹ See, e.g., S. Cunningham, CAUSAL INFERENCE: THE MIXTAPE (2021).
- ³² A. Abadie, *Using Synthetic Controls: Feasibility, Data Requirements, and Methodological Aspects*, 59 JOURNAL OF ECONOMIC LITERATURE 391-425 (2021).
- ³³ D. Arkhangelsky, S. Athey, D.A. Hirshberg, G.W. Imbens, and S. Wager, *Synthetic Difference-in-Differences*, 111 AMERICAN ECONOMIC REVIEW 4088-4118 (2021) (available at: <https://www.aeaweb.org/articles?id=10.1257/aer.20190159>).

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³⁴ The “Information” industry aggregate is excluded. Only three of the aggregate sectors receive zero weight. For conciseness, the weights are provided by line code: 2 (0.009); 5 (0); 9 (0.077); 13 (0.013); 14 (0.145); 40 (0.063); 43 (0.048); 48 (0.187); 62 (0.108); 73 (0); 76 (0.026); 80 (0.099); 81 (0); 84 (0.001); 85 (0); 90 (0.126); 93 (0.044); and 96 (0.052).

³⁵ *Draft Order*, *supra* n. 1 at ¶ 288.

³⁶ *Id.* at ¶ 289.

³⁷ *Investment in the Virtuous Circle*, *supra* n. 9.

³⁸ *Draft Order*, *supra* n. 1 at ¶ 290.

³⁹ *Id.* at ft. 1170.

⁴⁰ See, e.g., *In the Matter of Communications Marketplace Report*, 2020 COMMUNICATIONS MARKETPLACE REPORT, FCC 20-188, 36 FCC Rcd. 2945 (rel. December 31, 2020) at ¶ 295 (“We selected 35 Organisation for Economic Cooperation and Development (OECD) countries to meet the statutory directive of developing a geographically diverse set of countries for comparison with the United States concerning international broadband services capability.”).

⁴¹ *Draft Order*, *supra* n. 1 at ¶¶ 291-6.

⁴² *Id.* at ft. 1170. The line codes for the control group from this paper are: 3, 10, 20, 21, 22, 23, 26, 30, 35, 45, 50, 68, 74, 79, 82, 87, 88, 89, 90, and 96.

⁴³ *Id.* at ¶ 384; G.S. Ford, *Revisiting the “Virtuous Circle” Two Years Later*, BLOOMBERG BNA (July 10, 2017) (available at: <https://www.phoenix-center.org/oped/BloombergBNAVirtuousCircleRevisited10July2017.pdf>).

⁴⁴ *2015 Order*, *supra* n. 3 at ¶ 2, 20.

⁴⁵ *Draft Order*, *supra* n. 1 at ¶ 284.

⁴⁶ *Id.* at ¶ 274.

⁴⁷ *Id.*

⁴⁸ *Id.* at ¶¶ 286-296, recognizes only three studies that it concludes are “rigorous,” and all three find a negative effect on investment.

⁴⁹ *Id.* at ¶ 274

⁵⁰ For a review of the economic evidence the Commission both relied upon and rejected in the *RIFO*, see G.S. Ford, *Net Neutrality and Investment in the US: A Review of Evidence from the 2018 Restoring Internet Freedom Order*, *supra* n. ___.

⁵¹ See *RIFO*, *supra* n. 2 at ¶¶ 95-98.

⁵² *Draft Order*, *supra* n. 1 at ¶ 282

⁵³ *Id.* at ¶ 283

⁵⁴ *Id.* at ¶ 275.

⁵⁵ *Id.*

⁵⁶ *Id.* See, e.g., 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: <https://www.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: <https://www.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).

⁵⁷ *Draft Order*, *supra* n. 1 at ¶ 280.

⁵⁸ *Id.* at ¶ 278.

NOTES CONTINUED:

⁵⁹ 47 U.S.C. § 1302.

⁶⁰ *Draft Order, supra*. 1 at ¶¶ 671-72. See G.S. Ford and L.J. Spiwak, *Tariffing Internet Termination: Pricing Implications of Classifying Broadband as a Title II Telecommunications Service*, 67 FEDERAL COMMUNICATIONS LAW JOURNAL 1 (2015); L.J. Spiwak, *USTelecom and its Aftermath*, 71 FEDERAL COMMUNICATIONS LAW JOURNAL 39 (2019).

⁶¹ *Draft Order, supra* n. 1 at ¶ 279

⁶² *Id.* at ¶ 279.

⁶³ *Id.* at ¶ 671.

⁶⁴ *Verizon v. FCC*, 740 F.3d 623, 658 (D.C. Cir. 2014).

⁶⁵ 740 F.3d at 668.

⁶⁶ 47 U.S.C. § 153(51) (“A telecommunications carrier shall be treated as a common carrier under this [Act] only to the extent that it is engaged in providing telecommunications services.”).

⁶⁷ *Draft Order, supra* n. 1 ¶ 672.

⁶⁸ For a full review of the law governing the “just and reasonable” standard, see *USTelecom and Aftermath, supra* n. 60.

⁶⁹ *USTelecom and Aftermath, id.*

⁷⁰ See, e.g., *American Municipal Power-Ohio, Inc., v. FERC*, 863 F.2d 70, 73 (D.C. Cir. 1988) (an “agency must make clear the ‘basic data and the whys and wherefores’ of its conclusions.”).

⁷¹ *Illinois Bell, id.* (citations omitted).