

ATTACHMENT

Declaration of Robert W. Hahn

**Before the
Federal Communications Commission
Washington, DC 20554**

In the Matter of)	
)	
Comments Sought on Broadband Study)	NBP Public Notice # 13
Conducted by the Berkman Center for)	
Internet and Society)	GN Dkt. Nos. 09-47, 09-51, 09-137
)	

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INTRODUCTION

1. I have been asked by AT&T to analyze the broadband study by the Berkman Center for Internet and Society (“Berkman study”).¹ In my opinion, there are three major, empirical “findings” in the Berkman study that serve as the underpinning of the study’s policy recommendations:

- The United States ranks in the middle with respect to broadband penetration and performance based on an international benchmarking exercise;
- The quality-adjusted price offered by U.S. broadband carriers does not compare favorably with the offerings in many other countries; and
- Unbundling appears to increase a country’s broadband penetration, all other things being equal.

In what follows, I analyze the methodological foundations of these findings. I demonstrate that these results do not withstand careful empirical scrutiny; and even if they did, they do not support the authors’ policy recommendations. Moreover, I argue that the penetration measures

1. Berkman Center for Internet and Society, Next generation connectivity: A review of broadband Internet transitions and policy from around the world, Oct. 2009 [hereinafter *Berkman Study*].

used in the Berkman study do not provide a good measure of consumer welfare, and should not be used as a basis for determining public policy.

2. My report is organized as follows: In Part I, I analyze the authors' claims that the United States ranks in the middle of the OECD countries on broadband penetration (defined as broadband connections per 100 inhabitants).² Notwithstanding my serious reservations about using this measure to assess country ranking or quality, the United States has made significant progress in recent years, enjoying impressive increases in broadband penetration from 47 percent in 2007 to 63 percent by 2009 according to the Pew Internet Project.³ In addition, U.S. broadband operators have supplied broadband access in a near-ubiquitous manner: 92 percent of U.S. homes have access to cable modem service,⁴ 82 percent of U.S. homes have access to DSL,⁵ and 92 percent of the population has access to a wireless 3G network in their primary place of residence.⁶ To illustrate my concerns with the simplistic broadband measure that is at the heart of the Berkman study, I introduce a multiple regression analysis that takes account of different factors that could affect broadband penetration. My analysis shows that broadband penetration in the United States modestly *outperforms* what would be *expected* given its demographic and economic characteristics.

3. In Part II, I analyze the authors' claim that prices and quality-adjusted prices for broadband service are too high in the United States. The authors ignore critical data to the

2. The Organisation for Economic Co-Operation and Development (OECD) is an international organization that consists of thirty major economic democracies.

3. Pew Internet, Home Broadband Adoption 2009, at 3, *available at* <http://pewinternet.org/Reports/2009/10-Home-Broadband-Adoption-2009.aspx> [hereinafter *Pew 2009 Report*].

4. SNL Kagan (2008), *available at* <http://www.ncta.com/Statistics.aspx> (accessed on Nov. 12, 2009)

5. Federal Communications Commission, In the Matter of Inquiry Concerning the Deployment of Advanced Telecommunications Capability to All Americans in a Reasonable and Timely Fashion, and Possible Steps to Accelerate Such Deployment Pursuant to Section 706 of the Telecommunications Act of 1996, Fifth Report (June 12, 2008), Appendix B, Table 14.

6. Comments of CTIA-The Wireless Association, WC Dkt. No. 05-337, filed April 17, 2008, at Attachment 1.

contrary. For example, based on my own research of U.S. broadband prices, the price of entry-level DSL in the United States *decreased* (in nominal terms) by 81 percent from 2001 to 2008.⁷ Indeed, the International Telecommunications Union finds that the United States has the *most* affordable broadband Internet service among the 150 countries that it surveys.⁸ Finally, on the issue of broadband speeds, another alleged deficiency of the United States broadband market, the vast majority of U.S. broadband consumers (85 percent) reported being satisfied with their download speeds—the *highest* satisfaction level of the countries surveyed.⁹ Verizon is offering download speeds of at least 15 Mbps in its territories upgraded with fiber-to-the-home, with 50 Mbps offered in some regions.¹⁰ Comcast offers broadband access at download speeds of up to 24 Mbps, depending on the service selected, and up to 50 Mbps with the introduction of DOCSIS 3.0 technology.¹¹ And AT&T U-verse customers have been given increased download speeds as high as 18 Mbps.¹² Many U.S. providers offer triple-play bundles that include high-speed Internet beginning at \$100 per month,¹³ with an “imputed” price of broadband at roughly \$30 per month.¹⁴ These data are not reported in the Berkman study. It is difficult to reconcile this impressive record of declining prices, nearly ubiquitous deployment, and high customer

7. See Table 1, *infra*.

8. International Telecommunications Union, *Measuring the Information Society: The ICT Development Index, 2009*, at 65-66, *available at* http://www.itu.int/ITU-D/ict/publications/idi/2009/material/IDI2009_w5.pdf [hereinafter *ITU Study*].

9. Ofcom, *The International Communications Market, Dec. 2007*, at 5, *available at* <http://www.ofcom.org.uk/research/cm/icmr07/icmr07.pdf> [hereinafter *Ofcom 2007*].

10. See Verizon FioS Internet, *Plans*, *available at* <http://www22.verizon.com/residential/fiosinternet/Plans/Plans.htm> (accessed on Oct. 26, 2009).

11. See Comcast Corp. SEC Form 10-K, for the year ending Dec. 31, 2008, at 3.

12. AT&T’s “Max Plus” U Verse plan, *available at* <http://www.att.com/u-verse/explore/internet-landing.jsp?wtSlotClick=1-0023EO-0-1&WT.svl=calltoaction>.

13. The triple-play bundle typically offered in the United States includes a substantial quantity of video programming that its distributor must procure at significant cost, which may make U.S. pricing not comparable to that for IP video services offered in other countries. See *Annual Assessment of the Status of Competition in the Market for the Delivery of Video Programming*, Jan. 16, 2009, at 37.

14. The imputed price of the broadband service is the price of the bundle less the standalone prices of cable television and voice service.

satisfaction with the study's conclusions that the United States is a mid-tier country when it comes to broadband.

4. In Part III, I analyze the authors' claim that unbundling is empirically correlated with greater broadband penetration. I find that the regression technique they employ generates biased estimates and is unreliable. Correcting this problem reveals that unbundling is not correlated with broadband penetration in any statistically significant way.

QUALIFICATIONS

5. My name is Robert W. Hahn. I am a Senior Visiting Fellow at the Smith School, University of Oxford and a Senior Fellow at the Georgetown Center for Business and Public Policy.

6. From 1999 to 2008, I served as the director of the AEI-Brookings Joint Center, a leader in policy research in law and economics, regulation, and antitrust. Previously, I worked for the President's Council of Economic Advisers, where I helped design the innovative market-based approach for reducing acid rain. I also have served on the faculties of Harvard University and Carnegie Mellon University.

7. I am a frequent contributor to leading scholarly journals including the *American Economic Review*, *Science*, and the *Yale Law Journal*, as well as to general-interest periodicals including the *New York Times*. I am the author of several books, among them *Reviving Regulatory Reform: A Global Perspective*. I currently serve on the editorial boards of the *Review of Environmental Economics and Policy*, *Policy and Internet, Regulation*, and the *Journal of Benefit-Cost Analysis*.

8. I received a B.A. in Mathematical Economics and an M.A. in Economics, both from Brown University, and a Ph.D. in Economics from the California Institute of Technology.

I. RANKINGS ON BROADBAND PENETRATION

9. The Berkman study concludes that the United States is a mid-tier country when it comes to broadband connections.¹⁵ In addition to presenting the rankings based on OECD statistics (Figure 3.4), the study presents penetration rankings from “GlobalComms database” (Figure 3.5).¹⁶ The authors conclude that the two databases produce nearly identical rankings for the United States (Figure 3.6): with OECD data, the United States ranks 15th out of 30, with the GlobalComms data, the United States ranks 16th.¹⁷

10. I find that the Berkman study’s reliance on these two benchmarking comparisons of broadband penetration to be tenuous. First, the Berkman study either ignores or summarily dismisses a significant number of studies that find the United States’ performance to be substantially better.¹⁸ Second, the accuracy and relevance of the OECD’s broadband measures is itself quite questionable.¹⁹ Despite the significant economic literature on this point, many of the

15. *Berkman Study, supra*, at Table 3.6.

16. The authors describe GlobalComms as follows: “GlobalComms is a regularly updated database of international broadband statistics, maintained by the widely-cited and long-time industry analysis group Telegeography, a division of PriMetrica, Inc. The firm states that the data comes from primary sources wherever possible (e.g., the operators), and secondarily from national regulatory agencies, international statistics organizations, and other sources.”) *Id.* at 129.

17. *Id.* at 33.

18. *See, e.g.*, Leonard Waverman & Kalyan Dasgupta, *Connectivity Scorecard 2009*, available at <http://www.connectivityscorecard.org/images/uploads/media/TheConnectivityReport2009.pdf> (finding that the United States has the highest broadband connectivity rate in the world); INSEAD & World Economic Forum, *The Global Information Technology Report 2008-2009*, available at <http://www.weforum.org/en/initiatives/gcp/Global%20Information%20Technology%20Report/index.htm> (finding that the U.S. Internet infrastructure is one of the world’s best).

19. For a review of the criticism of the OECD’s broadband rankings, see Market Clarity, *Broadband Wars—The OECD’s International Broadband Arms Race* (2007) [hereinafter *Broadband Wars*]; Scott Wallsten, *Understanding International Broadband Comparisons*, TECHNOLOGY POLICY INSTITUTE 19 (2008); George S. Ford, *Normalizing Broadband Connections*, 09-01 PHOENIX CENTER PERSPECTIVES 1-2 (2009) [hereinafter *Normalizing Broadband Connections*]; Scott Wallsten, *Understanding International Broadband Comparisons*, TECHNOLOGY POLICY INSTITUTE 1 (2009 update) [hereinafter *Understanding International Broadband Comparisons 2009*]; Scott Wallsten, *Broadband and Unbundling Regulations in OECD Countries*, AEI-BROOKINGS JOINT CENTER FOR REGULATORY STUDIES Working Paper 06-16 (2006) [hereinafter *Broadband and Unbundling Regulations*]; James B. Speta, *Policy Levers and Demand Drivers in Korean Broadband Penetration*, 4 JOURNAL OF KOREAN LAW 1-18 (2004) [hereinafter *Policy Levers and Demand Drivers*]; George S. Ford, *The Broadband Performance Index: A Policy-Relevant Method of Comparing Broadband Adoption Among Countries*, PHOENIX CENTER POLICY PAPER

criticisms of the OECD's broadband rankings are not addressed by the Berkman study. This research suggests that OECD statistics do not reliably distinguish business from residential connections, and they fail to count most wireless Internet connections, where the United States is among the leaders. Survey data could correct many of the infirmities of broadband counts.

11. A key weakness of the Berkman study's analysis of broadband rankings is that it fails to take account adequately of factors that could affect the supply and demand for broadband. Instead of controlling for key variables, the authors embrace a benchmarking approach—that is, the authors simply compare the variable of interest (what an econometrician calls the “dependent variable”) in the United States with data on the dependent variable from other countries. For example, the authors compare broadband penetration in Korea (measured in broadband connections per 100 inhabitants) with U.S. broadband penetration, without taking account of all of the key distinctions between the two countries that might explain those differences. In defense of this approach, the authors of the Berkman study provide an unorthodox algorithm for omitting certain variables that can help explain variations in broadband penetration across countries, and are likely important factors affecting demand and supply. There is no reason, for example, to expect on an *a priori* basis that broadband consumption in the United States—or consumption of any good—should be identical to the levels consumed in other countries. This critique of benchmarking is not specific to the Berkman study; in general,

SERIES Paper Number 29 (2007); T. Randolph Beard, George S. Ford & Lawrence J. Spiwak, *The Broadband Adoption Index: Improving Measurements and Comparisons of Broadband Deployment and Adoption*, PHOENIX CENTER POLICY PAPER SERIES Paper Number 36 (2009); George S. Ford, Thomas M. Koutsky & Lawrence J. Spiwak, *The Broadband Efficiency Index: What Really Drives Broadband Adoption Across the OECD?*, PHOENIX CENTER POLICY PAPER SERIES Paper Number 33 1 (2008); Thomas Hazlett, *Rivalrous Telecommunications Networks With and Without Mandatory Sharing*, AEI-BROOKINGS JOINT CENTER FOR REGULATORY STUDIES Working Paper (2005) [hereinafter *Rivalrous Telecom Networks*].

economists do not embrace benchmarking as an analytical tool because it fails to account for factors that might contribute to differences in the variable of interest.

12. Finally, the authors do not provide a good justification for why raw broadband penetration should be the primary measure of economic welfare across different countries. I also explain why incorrectly premised policies aimed at increasing broadband penetration of legacy networks could harm consumers in the long-run by reducing incentives for investment and innovation in more advanced networks.

A. The underlying data on broadband penetration are highly suspect

13. Serious problems exist with current broadband penetration measures generally, even if one recognizes broadband penetration as the correct variable of interest. A vast body of research has demonstrated that OECD data commonly used to “rank” countries have several significant limitations that make cross-country comparisons dubious.

1. OECD data are not gathered consistently from year to year or from country to country

14. OECD data, on which many studies rely in whole or in part, have been widely criticized by economists. Market Clarity, a leading telecommunications industry research organization, released a study in May 2007 detailing many troubling facts about OECD data.²⁰ First, OECD data are insufficiently sourced, and they fail to discuss likely sources of error, degrees of error, or the sampling methodologies used to collect the information.²¹ Second, the OECD is not consistent in the way it counts broadband connections from country to country, or from year to year.²² Wireless connections are included in some country totals but not others.

20. *Broadband Wars, supra.*

21. *Id* at 5-6.

22. Scott Wallsten, *Understanding International Broadband Comparisons*, TECHNOLOGY POLICY INSTITUTE 19 (2008) (“Collection methods or methods of counting broadband appear to have changed differently in each country

Although the OECD intends only to count connections with download speeds greater than or equal to 256 Kbps, in some cases slower connections speeds are counted.²³ OECD data also fail to account for business connections in some countries.²⁴

15. The unreliability of OECD data is readily apparent when it is compared to other data sources, such as governmental statistics or data provided by telecommunications regulators. Market Clarity found that the OECD's data released in June 2006 was in full agreement with national data sources in only two out of 30 countries.²⁵ Researchers have found that OECD metrics often fail to account for broadband lines in three important categories: wireless connections, connections at colleges and universities, and connections at places of business.²⁶ Although these data inconsistencies are troubling by themselves, they are particularly problematic when the data are being used to calculate inter-country rankings, because any data distortions across countries could affect the rankings substantially and render them unusable.

2. OECD broadband penetration rankings based on per capita metrics are misleading

16. The Berkman study points to 2008 OECD rankings, which place the United States 15th among OECD nations in "penetration per 100 inhabitants."²⁷ This per capita metric, however, is a deeply flawed basis for making country-to-country comparisons. Differences in household sizes across countries skew broadband per capita metrics because the number of

over time. Such changes are not surprising; as broadband was first becoming popular many regulators and providers may had not yet determined how to best count connections.") [hereinafter *Understanding International Broadband Comparisons 2008*].

23. *Broadband Wars*, *supra*, at 5-6.

24. *Id.*

25. *Id.* at 5.

26. Scott Wallsten, *Everything You Hear about Broadband in the U.S. is Wrong*, Progress on Point, Periodic Commentaries on the Policy Debate, The Progress and Freedom Foundation 5 (2007).

27. *Berkman Study*, *supra*, at 10.

broadband connections, however they are counted, are normalized by population.²⁸ The United States is predisposed to ranking low in per capita penetration due to the relatively large size of U.S. households compared to non-U.S. households.²⁹ Per capita estimates fail to account for the simple reality that broadband connections are purchased to serve a given location, such as a home or business establishment that often caters to multiple individuals.³⁰

17. Although using measures of penetration per household may seem like the obvious solution, this measure is also unreliable.³¹ Further, arriving at penetration per household—namely, by dividing the OECD’s number of connections in a country by the total number of households—is likely incorrect because the OECD combines residential and business lines inconsistently across countries.³² For example, the OECD’s tally of total U.S. lines has been estimated to potentially omit up to 70 million wireline business broadband connections.³³ To be fair, the OECD’s methods likely misstate broadband penetration in many countries, not just the United States.

18. Many studies, including the Berkman report, contrast the deployment of broadband in the United States with broadband rollout in countries like Korea.³⁴ Korea ranks sixth in the world in penetration per capita and first in penetration per household.³⁵ But Korea is

28. *Normalizing Broadband Connections*, *supra*, at 1-2.

29. *Understanding International Broadband Comparisons 2009*, *supra*, at 1.

30. *Normalizing Broadband Connections* at 1.

31. The OECD’s data on household penetration have a number of flaws. Indeed, the Berkman study acknowledges some of these deficiencies. *See Berkman Study*, *supra*, at 32 (“The Japanese [household penetration] numbers are potentially polluted by the fact that they include 3G subscriptions, which are particularly high in Japan, and therefore make it potentially inappropriate to interpret the Japanese household penetration numbers as in fact comparable to those of other countries.”). Moreover, the OECD makes prior period adjustments to its statistics without full explanation, which further clouds the usefulness of these data. Indeed, prior period changes have appeared in the OECD’s household penetration statistics since the Berkman study was issued in October 2009.

32. *Understanding International Broadband Comparisons 2009* at 2.

33. *Id.* at 3.

34. *Berkman Study*, *supra*, at 11.

35. *Id.* at 46.

much more densely populated than the United States, reducing the cost of broadband buildout there.³⁶ Indeed, Korea's population density is roughly 16 times greater than that of the United States, and more than half of Koreans live in large apartment buildings.³⁷ Empirical analyses have demonstrated that buildout occurs faster in areas where population density is higher.³⁸ In addition, broadband in Korea has been the beneficiary of massive government-subsidized investments.³⁹ Accordingly, comparing OECD statistics on penetration per capita in two countries is simply not the apples-to-apples exercise implied by the rankings.

3. Surveys can overcome many of the problems caused by counts

19. Broadband data collection has, to date, followed the tradition in telecommunications of counting lines and connections.⁴⁰ Yet the fast spread of broadband and the varied ways in which people and businesses use it makes such counts increasingly irrelevant. For example, counts of lines now routinely miss most business and university connections, simply because it is not possible for providers to count each device connected to the large data pipes that serve those institutions. Each counted connection to a household, meanwhile, may be used by a number of household members, making the line count only loosely correlated with the number of people using broadband connections. Line counts are thus likely to underestimate both the number of connections and the number of people who use broadband. At the same time, counts of wireless broadband users are primarily counts of the number of broadband-enabled wireless devices. Not all people who have broadband-enabled wireless devices use those

36. *Broadband and Unbundling Regulations, supra.*

37. *Policy Levers and Demand Drivers, supra*, at 1-18.

38. *Broadband and Unbundling Regulations, supra*, at 7.

39. *Berkman Study, supra*, at 87

40. See, e.g., Robert W. Hahn & Scott Wallsten, *An Economic Perspective on a U.S. National Broadband Plan, POLICY AND INTERNET* (forthcoming 2009), available at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1416426 [hereinafter *National Broadband Plan*].

broadband services.⁴¹ For this reason, the Berkman study's measurements of wireless broadband penetration are likely not to provide an accurate portrayal of actual mobile broadband usage.⁴²

20. A potential solution to these problems is to use surveys rather than counts.⁴³ Surveys can be done relatively inexpensively and quickly, and can be designed to answer specific policy questions. Surveys should be done with input from the Commission, and be designed to answer relevant policy questions. Already some of the most useful information about broadband comes not from counts, but from surveys conducted by the Pew Internet and American Life Project, along with the few questions the U.S. Census asked about broadband in a 2007 household survey.

B. Simple comparisons of broadband lines per 100 inhabitants omit critical variables that explain variation in broadband penetration across countries

21. Setting aside my concerns about their penetration data described above, the authors' benchmarking exercise omits critical variables that likely explain variations in broadband penetration across countries. In particular, because the United States has more inhabitants per household than other countries, and because the United States is more rural than other countries, a simple benchmarking exercise will generate unreliable inferences about U.S. broadband performance.⁴⁴ The Berkman study seeks to rebut criticisms relating to omitted

41. For this reason, measuring 3G penetration by SIM card counts is not reliable. Because most U.S. mobile subscribers are post-paid, however, count data are slightly more reliable in the United States than in countries with mostly pre-paid customers.

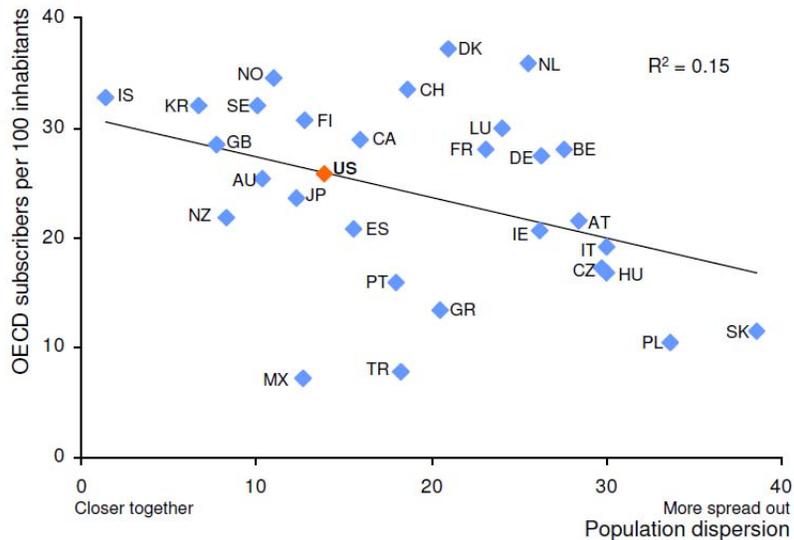
42. The OECD's reported figures for wireless 3G subscriptions (that the Berkman study relies on) also happen to be subject to immense errors. See *OECD's Communications Outlook 2009*, Table 4.12 (reporting just 586,141 wireless 3G subscriptions in the United States in 2007). Comscore reports over 35 million of these devices in the United States as of June 2007. See Comscore Press Release, *Comscore Reports that the U.S. Catches Up with Western Europe in Adoption of 3G Mobile Devices*, available at http://www.comscore.com/Press_Events/Press_Releases/2008/09/US_Adoption_of_3G_Mobile_Devices. See also Business Monitor International, *United States Telecommunications Report Q3 2009*, at 16 (reporting even higher figures of 41.7 million U.S. 3G subscribers in 2007).

43. Of course, surveys themselves would need to be interpreted with care in an international context, and they are potentially more complex to execute.

44. See *National Broadband Plan*, *supra*.

variables by using unorthodox empirical methods. In particular, the study fits a line between the dependent variable (broadband penetration) and several of the variables that have been cited by researchers who are critical of the OECD's rankings (for example, percent of households with broadband), and then analyzes where the United States sits relative to that trendline. According to the authors, if the United States sits reasonably close to the trendline, then its "shortfall" in broadband penetration cannot be explained by the omitted variable in question. Stated differently, in the authors' minds, if there is a linear relationship between the dependent variable and the alleged omitted variable in the analysis, and if the omitted variable in the United States can be reasonably predicted with knowledge of the number of broadband subscribers per 100 inhabitants, then omission of that variable cannot explain the alleged shortfall in U.S. broadband connections. These univariate regressions are performed for broadband penetration and households (Figure 3.4), urbanicity (Figure 3.7), and population dispersion (Figure 3.8). Figure 3.8 is replicated here.

FIGURE 1: REPRODUCTION OF FIGURE 3.8
“BROADBAND PENETRATION AND POPULATION DISPERSION”



According to the authors, “What this analysis does allow us to do, however, is again identify countries that outperform the (very limited) degree to which [population dispersion] measure predicts their penetration. As with urbanicity, the United States’ ranking is largely unaffected”⁴⁵ In other words, the authors conclude that controlling for these variables would *not* alter their conclusion that broadband performance in the United States is below expectations. As I demonstrate below, the authors’ suggested technique for reaching such a conclusion is incorrect as a matter of statistical inference.

22. Econometricians have developed specific techniques to determine whether an omitted variable may cause a researcher to make biased inferences when conducting empirical analyses. To take advantage of these scientific techniques, however, one must first reframe the problem presented by the study’s authors. The study’s cross-country benchmarking comparisons imply a simple prediction model for U.S. broadband penetration—namely the predicted value is

45. *Berkman Study, supra*, at 36.

equal to the average broadband penetration realized in other countries. An equivalent test of whether the United States is underperforming its peers is to regress broadband penetration in a country on a constant term (which picks up the average effect) and an indicator variable (also known as a “dummy” variable) that equals one for the United States (and zero otherwise). Stated differently, a regression of $P = a + b D + e$, where P is broadband penetration, D is an indicator variable for the United States (that is $D = 1$ for the United States and zero otherwise), and e is an error term. If the coefficient on the indicator variable (b) is negative and significant, then one can infer that the United States is indeed a laggard *so long as there are no important omitted variables that are correlated with the U.S. indicator variable (D)*.⁴⁶

23. The economic literature has identified several candidates for omitted variables here, including (1) the number of households, (2) the percentage of households living in urban areas (“urbanicity”), and (3) income per capita. If any one of those variables (or other omitted variables) is correlated with the U.S. indicator variable, then the estimate of b will be biased. Because the U.S. indicator variable can only take on the value of zero or one, the correlation between this indicator variable and a given omitted variable will be zero if and only if the value for that omitted variable in the United States is *not* equal to the average value across the sample of countries. For example, if U.S. urbanicity were not equal to the average of urbanicity across the sample, then urbanicity would be correlated with the U.S. indicator variable. Accordingly, to determine whether the omission of a variable from the regression permits the researcher to obtain an unbiased estimate of b , one must calculate the correlation coefficient between the U.S. indicator variable and the omitted variable. Compare this well recognized criterion for

46. RAMU RAMANATHAN, INTRODUCTORY ECONOMETRICS WITH APPLICATIONS 186 (Dryden Press 2nd ed. 1992).

identifying the effect of omitted variable bias to the authors' novel standard, which involves showing that the *dependent variable is* correlated with the omitted variables.

24. It is straightforward to show that the standard econometric criterion for omitted variable bias—that is, whether relevant variables are omitted and those variables are correlated with the U.S. indicator variable—is satisfied here. In particular, the United States does not exhibit the average values across the 30 OECD countries for the number of households or urbanicity. For example, according to United Nations urbanicity data from 2005, which the authors of the Berkman study claim to use, the percent of households in urban areas in the United States was 81 percent, whereas the average urbanicity across the 30 OECD countries was 75 percent. According to OECD data, the poverty rate in the United States was 17 percent in the “mid-2000s,” whereas the average poverty rate across the 30 OECD countries over the comparable period was 11 percent. Accordingly, exclusion of a variable that captures urbanicity or poverty in a regression of broadband penetration ensures that the coefficient on the U.S. indicator variable (b) is biased. As it turns out (see regression results in Table 1 below), because these UN data suggest that the United States is *more* urban than the average OECD country, and because urbanicity is *positively* correlated with broadband penetration), the omission of urbanicity by the authors biases the coefficient on the U.S. indicator variable (b) *upward*.⁴⁷ But because the United States has a *higher* poverty rate than the average rate for OECD countries, and because poverty is *negatively* correlated with broadband penetration, the omission of urbanicity from the regression biases estimates of b *downward*. The omitted variable bias from

47. There are many potential alternative definitions for urbanicity. See, e.g., John de Ridder, *Catching-up in Broadband: What Will it Take?*, Organisation for Economic Co-Operation and Development, July 25, 2007, at 11-12 [hereinafter *Catching-up in Broadband*].

the poverty variable is stronger, which implies that on net, the omission of both variables causes the authors to understate the coefficient on the U.S. indicator variable.

25. Indeed, the authors appear to recognize the importance of these omitted variables; in a subsequent part of their report, they include the percent of households and poverty as explanatory variables in a regression of broadband penetration across the OECD countries.⁴⁸ As it turns out, their “percent in urban areas” variable is positively and statistically significantly related to broadband penetration at the five percent level.⁴⁹ And their “poverty” variable is negative and statistically significantly related to broadband penetration at the five percent level.⁵⁰ The fact that the United States may lie close to a trendline that relates broadband penetration and urbanicity (or poverty) is meaningless; so long as the United States does not take on the *average* value of these variables across the OECD countries, omission of that variable in any comparison of broadband penetration ensures that inferences about the United States will be biased.

26. Finally, to test whether urbanicity or other control variables should be considered when comparing broadband penetration in the United States to other countries, the authors should have regressed broadband penetration on a vector of explanatory variables plus an indicator variable for the United States. I do that analysis here. In particular, I add a single variable, a U.S. indicator variable, to nearly the same regression run by the authors in Annex 3.8, in which the authors regressed broadband penetration on a constant term, the poverty rate, income per capita, urbanicity, and education.⁵¹ According to the authors’ hypothesis that the United States is underachieving in broadband penetration, the U.S. indicator variable should be

48. *Berkman Study, supra*, Annex 3.8.

49. *Id.*

50. *Id.*

51. The authors ran an additional specification of this regression that included a variable for DSL prices. Because we lack that data, we did not attempt to reproduce that specification.

negative and statistically significant. Stated differently, if the authors’ hypothesis is correct, one allegedly cannot account for the “gap” in U.S. broadband penetration by invoking factors such as poverty and urbanicity. Table 1 summarizes my results alongside those from the Berkman study (Annex 3.8).

TABLE 1: MULTIPLE REGRESSION ANALYSIS
DEPENDENT VARIABLE: BROADBAND PENETRATION

<i>Variable</i>	<i>Specification without U.S. indicator</i>			<i>Specification with U.S. indicator</i>		
	<i>Coefficient</i>	<i>Std. Error</i>	<i>t-stat</i>	<i>Coefficient</i>	<i>Std. Error</i>	<i>t-stat</i>
Poverty_Rate	-0.40	0.33	-1.24	-0.41	0.38	-1.08
Berkman	-0.54	0.21	-2.57			
GDP_Per_Cap	0.24	0.15	1.56	0.23	0.16	1.51
Berkman	0.65	0.15	4.33			
Urbanicity	0.24	0.12	2.08	0.24	0.12	2.04
Berkman	0.20	0.08	2.50			
Education	0.55	1.08	0.51	0.53	1.16	0.46
Berkman	0.19	0.70	0.27			
United States				0.39	7.49	0.05
Berkman						
Constant	-12.48	13.92	-0.90	-12.13	15.66	-0.77
Berkman	4.58	NA	NA			

Notes: 30 observations. Broadband penetration in 2005 from the OECD. For each explanatory variable, the Berkman estimates are presented below my estimates. NA is not available.

The first three columns in Table 1 represent my attempt to replicate the results from the Berkman study. To facilitate comparisons, I included the estimated coefficient, standard error, and t-statistic from the Berkman study immediately below my estimate. The estimated coefficients are of the same sign and are close in magnitude. The second three columns produce my results with the inclusion of the U.S. indicator variable.

27. One problem with trying to replicate the Berkman study estimates is that its specific data sources are not always clear. The Berkman study only offered vague references to the OECD and the UN as the source for their explanatory variables. Moreover, it did not present

the summary statistics for those variables, which is customary in most econometric papers. Despite these impediments, I was able to largely replicate their findings. One difference in my models is the construction of the income variable; the Berkman study used median income and I used income per capita. Another difference between the two models is that because I include a U.S. indicator variable, this affects the estimated coefficients of the other explanatory variables. As Table 1 shows, the urbanicity variable continues to be statistically significant at the five percent level. Although poverty has the expected (negative) sign, it is no longer significant at the five percent level in my attempt to replicate their model. Most important, the coefficient on the U.S. indicator variable is positive but not statistically significant. The implication is that (1) controlling for all other factors, the United States may modestly outperform its peers in terms of broadband penetration, but (2) the relationship is not statistically significant.

28. My results should not be surprising in light of the Berkman study's *univariate* regression results (Figures 3.4, 3.7, 3.8), which showed the United States sitting on the trendline in regressions of broadband penetration on households, urban concentration, and population dispersion, respectively. In effect, what the authors admit (through their unorthodox technique) is that the United States is precisely where one would expect them to be when controlling for *one* variable at a time. My *multivariate* regression confirms this finding in a more rigorous statistical way. In sum, the United States is *not* a laggard when it comes to broadband penetration, when one takes account of the combined effect of key differences across countries.

29. My results are corroborated by other econometric studies that take multiple country-specific characteristics into account in evaluating broadband penetration. For example, using an econometric analysis, Dr. Wallsten concluded that controlling for household size and changing methods of counting broadband connections explains nearly all of the changes in rank

for particular countries in OECD data from year to year.⁵² Other studies have suggested that factors such as population density, income, income inequality, education, and population age may have explanatory power with regard to broadband penetration and adoption. OECD and ITU metrics, and measures like them, ignore these characteristics.⁵³ Although the Berkman study purports to show that consideration of these alternative factors would not affect the results for the United States, a statistical test using proper multiple regression analysis reveals just the opposite.

C. It is difficult to account for differences in preferences

30. Simple benchmarking exercises cannot account for differences in preferences across countries, which likely explain variations in broadband consumption. Indeed, even cross-sectional regression analyses, which I performed above (and which improve upon the Berkman study's benchmarking approach), cannot account for differences in preferences. There is no *a priori* reason to believe that the consumption levels of any good—be it broadband Internet access, cheese steaks, or hair products—will be identical across all countries. Citizens of country A might prefer to visit theatres to watch movies, while citizens of country B might prefer to rent DVDs; the fact that Country A consumes fewer DVDs is not a failing (or a market failure) or a cause for government interference. For example, some dial-up Internet customers in the United States have formed strong preferences for dial-up access. According to the most recent Pew Internet survey, nearly 20 percent of U.S. dial-up customers indicated that *nothing would convince them to switch to broadband*.⁵⁴ (These respondents were given alternative choices such as a reduction in broadband prices, yet chose “nothing would get them to change”). Americans also have stronger preferences for watching multichannel television than their European peers—

52. *Understanding International Broadband Comparisons 2008, supra*, at 21.

53. George S. Ford, *The Broadband Performance Index: A Policy-Relevant Method of Comparing Broadband Adoption Among Countries*, PHOENIX CENTER POLICY PAPER SERIES Paper Number 29 (2007).

54. *Pew 2009 Report, supra*, at 7.

an activity that may substitute somewhat for broadband access.⁵⁵ Although there are sophisticated methods that econometricians use to account for these factors (for example, fixed effects models using panel data or instrumental-variable techniques), the Berkman study makes no attempt to account for tastes. Their failure to do so is troublesome because it is important to understand key factors affecting supply and demand before making policy recommendations.

D. Broadband penetration is not a complete measure of consumer welfare

31. Even if one could properly account for unobservable variables such as tastes in the context of a properly specified regression model, any resulting shortfall in a country's broadband penetration would still not justify unbundling or "open access" regulation. (I use the terms "unbundling" and "open access" hereinafter to describe the general policy advocated by the study's authors.) Raw broadband penetration statistics are not a complete measure of consumer welfare across different countries. Consumer welfare is a function of both immediate metrics such as price and penetration of current legacy broadband networks, and longer-run variables such as investment and innovation in more advanced networks. Although it is possible as a matter of economic theory that "open access" could decrease prices and increase penetration of current broadband services (I review the empirical support of this hypothesis in Part III below), "open access" could also reduce the incentives for broadband providers to invest in more advanced networks,⁵⁶ which could reduce overall consumer welfare in the long run.

55. OECD's *Communications Outlook 2009*, Table 6.2 (reporting that daily hours of television viewing in the United States is more than *double* that in any other OECD country).

56. See Scott Wallsten & Hausladen, *Net Neutrality, Unbundling, and Their Effects On International Investment In Next-Generation Networks*, REVIEW OF NETWORK ECONOMICS (2009) (showing that the relationship between unbundling and next-generation network penetration is negative and statistically significant for both incumbents and entrants) [hereinafter *Wallsten & Hausladen*]; Grajek & Roller, *Regulation and Investment in Network Industries: Evidence from European Telecoms* EMST Working Paper 09-004 (showing that access regulation negatively affects both total industry and individual carrier investment, and that regulators respond endogenously to incumbent infrastructure investments by providing easier access to entrants) [hereinafter *Grajek & Roller*]; Robert W. Crandall, Allan T. Ingraham & Hal J. Singer, *Do Unbundling Policies Discourage CLEC*

II. RANKINGS ON PRICES AND SPEED

32. The Berkman study produces a series of quality-adjusted price comparisons across countries, again based on OECD and GlobalComms data. Taking their pricing data at face value, the United States appears to fare moderately well in comparisons of “low” speeds (Figure 4.3), “medium” speeds (Figure 4.4), and “high” speeds (Figure 4.5).⁵⁷ All 30 countries in the OECD survey offer “low” and “medium” speeds; 27 of 30 offer “high” speeds; and only 12 countries offer “very high” speeds (greater than 35 Mbps). Based on the price comparison of the very highest speed (and likely least-subscribed-to services⁵⁸), the authors claim to have discovered a “clear pattern”:

First, we conducted a study of pricing at the company level of 59 companies that offer high speed access. Our pricing study (Figure 4.2) shows that prices and speeds at the highest tiers of service follow a clear pattern. The highest prices for the lowest speeds are overwhelmingly offered by firms in the United States and Canada, all of which inhabit markets structured around “inter-modal” competition—that is, competition between one incumbent owning a telephone system, and one incumbent owning a cable system.⁵⁹

Taking their pricing data at face value, three U.S. broadband carriers that offer download speeds in excess of 35 Mbps—Verizon, Charter, and Qwest—appear to charge more than most carriers with comparable offerings in their database. In particular, Figure 4.2 suggests that the prices for very fast offerings in the United States may be *\$100 more per month* than comparable offerings elsewhere (\$140 per month versus \$40 per month). Because these carriers compete in “markets

Facilities-Based Investment?, 4 BERKELEY ELECTRONIC JOURNAL: TOPICS IN ECONOMIC ANALYSIS AND POLICY (2004) (showing that mandatory unbundling encourages a CLEC to delay facilities-based investment by altering its relative net present value of investment between time periods) [hereinafter *Crandall, Ingraham & Singer*].

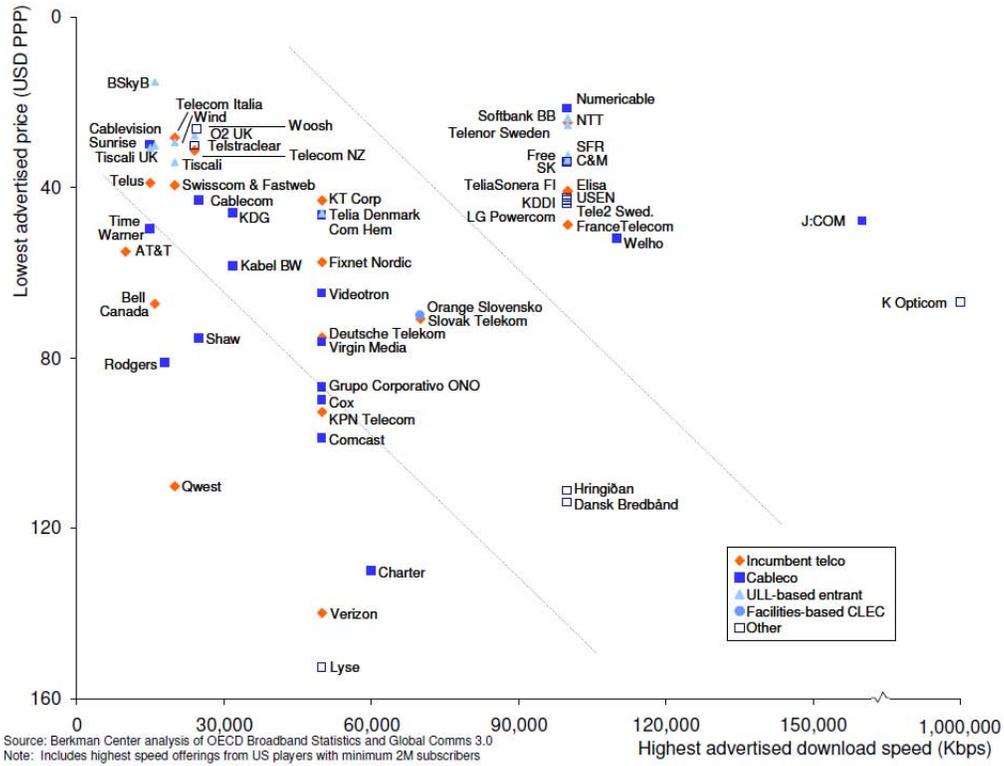
57. The authors define their speed tiers as follows: “low (256 Kbps to 2 Mbps), medium (2.5 Mbps to 10 Mbps), high (10 Mbps to 35 Mbps) and very high (greater than 35 Mbps).” *Berkman Study, supra*, at 126.

58. The only services that likely can support those speeds are DOCSIS 3.0 or fiber to the home, neither of which have yet been adopted widely by consumers. Moreover, advertised speeds generally exceed delivered speeds. See OECD *Communications Outlook 2009* at 108-112. Indeed, the larger the claimed speed, the greater percentage exaggeration over the actual download speed. This may be observed simply by comparing Akamai’s actually measured download speeds in different countries with OECD statistics for advertised speeds. See Akamai’s *State of the Internet Report 4Q08*, available at <http://www.akamai.com/stateoftheinternet>.

59. *Berkman Study, supra*, at 12.

structured around ‘inter-modal’ competition,” the authors suggest incorrectly that inter-modal competition is the cause of higher quality-adjusted prices. To assist in my discussion, I reproduce Figure 4.2 from the Berkman study here, which plots the fastest-speed offerings by carrier in the space of speed (x-axis) and price (y-axis).

FIGURE 2: REPRODUCTION OF FIGURE 4.2 “BEST PRICE FOR HIGHEST SPEED OFFERING”



I explain below that this analytical approach is as flawed as the prior benchmarking exercise regarding penetration rates. In particular, the authors fail to mention whether significant numbers of customers actually take these highest speed offerings, or if they do, if they pay the advertised price for standalone broadband service.⁶⁰ Furthermore, focusing on only one level of broadband service tells you nothing about the other aspects of service that consumers may use. For example,

60. Although the OECD publishes prices in both Purchasing Power Parity and actual currency-exchange-rate terms, the Berkman study employs only the former in its price comparisons; doing so may result in significant discounts to the prices of European broadband services.

Verizon offers roughly the same price-speed pair (when purchased as part of a bundle with video service) as Swiscomm's *fastest* offering (25 Mbps at \$40 per month, shown in the center of the figure above). But Verizon also offers a faster service at a higher price (50 Mbps at \$140 per month, standalone). By construction of their figure, the Berkman study suppresses Verizon's 25 Mbps offering, which implies incorrectly that Verizon's price for high-speed service is high compared to the offerings of its peers. If Verizon wanted to score well on this metric, it would withdraw its 50 Mbps offering from the marketplace, which is not consistent with the goal of maximizing consumer welfare. Accordingly, these comparisons of the fastest offerings may not be economically meaningful. (A more sensible price comparison would be separate scatter plots of all prices in the marketplace for 5 Mbps, 10 Mbps, 25 Mbps, and 50 Mbps.)

33. The authors' representations of the "best price for the highest speed service" offered by these carriers is often misleading. To portray the United States as an outlier, the authors rely on just three data points—an inadequate sample for reaching sweeping policy conclusions. I demonstrate below that these data points are unreliable. Given the boldness of the authors' causal inference between inter-modal competition and higher quality-adjusted prices, it is surprising that the authors did not probe the integrity of their data more carefully. I consider each observation in turn.

34. The authors focus on an irrelevant high-speed offering by Charter, which was quickly pulled from the market. On January 29, 2009, Charter announced a 60 Mbps service, the fastest in the United States, at \$140 per month.⁶¹ This offering appears as an "outlier" in Figure 4.2, as it sits in the southwestern portion of the graph. Two months later, however, the firm filed

61. Various news reports discussed the Charter announcement, noting the 60 mbps speed and \$140 monthly price. See, e.g., Nate Anderson, *Charter gets bragging rights with new 60Mbps broadband tier*, ARS TECHNICA, Jan. 29, 2009, available at <http://arstechnica.com/telecom/news/2009/01/charter-gets-bragging-rights-with-new-60mbps-broadband-tier.ars>.

for Chapter 11 bankruptcy protection,⁶² and as of October 2009, Charter did not appear to offer that speed any longer.⁶³ Currently, Charter offers a variety of packages with download speeds up to 20 Mbps. The firm charges \$55 per month for this service if it is included as part of a bundle with telephone service; it charges \$65 per month for the service when purchased on a standalone basis. Even using this higher price of \$65 for 20 Mbps, Charter's high-speed broadband service falls within the range of price and speed offered by providers in other countries in the analysis; it is hardly the "outlier" shown in the Berkman study's dramatic graph.

35. The second observation in the U.S. sample is from Qwest. In this case, it appears that the authors overstate the price of the fastest Qwest offering. The Berkman study shows the firm charging about \$110 for 20 Mbps service. Yet a simple online search reveals a Qwest offering of 20 Mbps broadband service for \$55 per month, or just \$45 per month if added to a qualifying telephone service.⁶⁴ Using the higher rate for standalone broadband service at the fastest speed offered, the Berkman study overstates the Qwest price by over 100 percent. At best, the higher price used in the Berkman study was a short-lived offer (perhaps an introductory offer) that was changed considerably since the OECD recorded that data.

36. The third and final "outlier" for the U.S. sample reported in the Berkman study is based on Verizon's best price for its highest-speed offering. Unfortunately, this so-called best price represents a limited picture of the firm's actual pricing and plans. As of October 2009, Verizon's fastest service was 50 Mbps, which the firm offered at \$145 per month if purchased on

62. See, e.g., *Charter Communications Files for Reorganization*, NEW YORK TIMES, Mar. 27, 2009, available at <http://www.nytimes.com/2009/03/28/technology/companies/28cable.html>.

63. Charter Broadband Price Plan, available at <http://www.charter.com/Visitors/Products.aspx?MenuItem=20>.

64. Qwest High-Speed Internet with WindowsLive, available at <http://www.qwest.com/residential/internet/broadbandlanding>.

a standalone basis in certain markets.⁶⁵ The Berkman study's analysis graphically shows that this 50 Mbps service offered by Verizon is substantially more expensive than similar speeds offered by other providers. The authors neglected to report that Verizon offers 50 Mbps residential service at \$90 per month in New York, Virginia, and Washington, D.C. with a one-year agreement and Verizon's telephone service.⁶⁶ Had the authors plotted this price-speed pair instead—which represents Verizon's *lowest* price for its *fastest* service—Verizon would not have appeared in the lower-left-hand corner of the figure.

37. More fundamentally, most broadband customers in the United States subscribe to broadband as part of a larger package that includes cable television and telephone service. Yet Verizon's 50 Mbps service is not part of its most discounted packages, and thus that offering is almost certainly marketed to a relatively few demanding customers.⁶⁷ For example, Verizon's 25 Mbps plan costs \$70 per month when purchased on a standalone basis.⁶⁸ However, Verizon offers that same 25 Mbps service as part of a bundle for \$120 per month, which includes unlimited telephone service (\$50 per month if purchased separately) and 320 channels of video programming (\$58 per month if purchased separately).⁶⁹ Accordingly, the "imputed price" for 25 Mbps broadband service in this package is \$12 per month (equal to \$120 less \$50 less \$58), a speed/price combination that falls within the range of many other services providers shown in the Berkman study. Even if one were to allocate the price of the bundle equally across the three

65. FiOS Internet Plans, *available at* <http://www22.verizon.com/Residential/FiOSInternet/Plans/Plans.htm> [hereinafter *FiOS Plans*].

66. Verizon Press Release, *available at* <http://newscenter.verizon.com/press-releases/verizon/2008/verizon-extends.html>.

67. *FiOS Plans, supra*.

68. *Id.*

69. Verizon FiOS Bundles, *available at* <http://www22.verizon.com/residential/bundles/fiosbundlesab>.

services, the resulting price of \$40 per month would place Verizon within the mid-range of offerings in Figure 4.2.

38. The fact that the Berkman study does not appear to have dealt with bundled offerings is troubling because bundling is important in many countries. This omission provides a significantly distorted picture of the effects on consumers in the United States for a simple reason—most U.S. broadband subscribers consume this service as part of a bundle that includes telephony or video or both.⁷⁰

39. In addition to ignoring the critical role of bundled pricing, the authors failed to consider the trajectory of broadband prices in the United States, which tell a different story about market performance. To fill this information gap, I examined a time series for a particular broadband offering in the United States from 2001 to 2008. I focused on the advertised prices of Verizon for 1.5 Mbps service, which would be considered by the Berkman study as a “low-speed” plan (below 2.0 Mbps).⁷¹

TABLE 2: PRICE OF LOWEST-SPEED DSL OFFERED BY VERIZON, 2001-2008

	Price	Mbps (down)	Price/Mbps (down)	Price Relative to May 2001 (Mbps)
May 2001	\$79.95	1.5	\$53.30	100%
April 2003	\$59.95	1.5	\$39.97	75%
May 2003	\$34.95	1.5	\$23.30	44%
June 2004	\$34.95	1.5	\$23.30	44%
March 2005	\$29.95	1.5	\$19.97	37%
July 2008	\$29.99	3	\$10.00	19%

70. Verizon Communications, SEC Form 10-Q, for period ending Sept. 30, 2009, at 30 (“Mass Markets revenue during the three months ended September 30, 2009 decreased \$44 million, or 0.9%, compared to the similar period in 2008. The decrease was primarily driven by the continued decline of local exchange revenues, principally as a result of switched access line losses, partially offset by the expansion of consumer and business FiOS services (Voice, Internet and TV), *which are typically sold in bundles.*”) (emphasis added). Comcast Corp., SEC Form 10-K, for the year ending Dec. 31, 2008, at 27 (“Average monthly revenue per customer for our digital phone service has declined, to approximately \$39 in 2008 from approximately \$42 in 2007 and approximately \$45 in 2006, *due to customers receiving service as part of a promotional offer or in a bundled service offering.*”) (emphasis added).

71. Note that only “low speed” offers (by current standards) have any significant price history.

As Table 2 shows, the price of 1.5 Mbps declined by 63 percent between 2001 and 2005. This impressive decline in broadband prices is hard to reconcile with the authors' suggestion that inter-modal competition cannot be counted on to discipline broadband prices. By 2008, the lowest-speed offering by Verizon was 3 Mbps (the 1.5 Mbps offering was no longer available). After dividing the price by the Mbps, the quality-adjusted price for Verizon's lowest-speed offering has declined by over 80 percent from 2001 through 2008. To be clear, this analysis is not the only way to measure quality-adjusted prices. But it does suggest that broadband prices in the United States are declining rapidly.

40. Finally, third-party sources confirm my pricing analysis above that U.S. broadband customers benefit greatly from facilities-based competition in terms of speed and prices. According to the Pew Internet Project, average U.S. broadband prices fell by four percent between December 2005 and April 2008, even as speeds increased.⁷² According to the OECD, the low end of U.S. broadband prices in 2007 ranked *fifth* in a 30-country survey of prices—that is, only four of 30 countries offered broadband services at a price less than the price offered in the United States.⁷³ The OECD also found that the United States ranked sixth overall in terms of fastest connections.⁷⁴ And according to Ofcom, 85 percent of U.S. consumers reported being satisfied with their download speeds—the *highest* satisfaction level of the countries surveyed.⁷⁵ These data are hard to reconcile with the Berkman study's suggestion that the United States is a mid-tier country when it comes to broadband.

72. *Pew 2009 Report, supra*, at 25.

73. OECD, *Broadband Growth and Policies in OECD Countries* (2008), Figure 1.14 [hereinafter *Broadband Growth and Policies*]. The ITU, which publishes both currency exchange rate and PPP comparisons of broadband, finds (among 150 countries) that the United States is the *most* affordable on a currency-exchange-rate basis, and fourth most affordable on a PPP basis (after Maldives, Serbia and Macao). *ITU Study, supra*.

74. *Broadband Growth and Policies, supra*, at 1.16.

75. *Ofcom 2007, supra*, at 5.

III. REGRESSION OF PENETRATION ON UNBUNDLING

41. In this section, I present an econometric critique of the authors' regression of broadband penetration on unbundling and other demographic and economic characteristics. As was the case in its prior empirical exercises, the techniques employed in the Berkman study do not conform to the standards adhered to by practicing economists. I do not address some fundamental problems with the authors' data, including the measurement of broadband penetration,⁷⁶ the definition of unbundling (which comes in many forms), or the use of a single variable to capture the price of DSL in a country for a given year (which similarly comes in many forms and varies within a year). Instead, I focus my attention here on problems with the empirical technique. In particular, I demonstrate that (1) the unbundling variable is likely capturing a different effect (the maturity of the broadband market), (2) the replacement of the original values for the unbundling variable with alternative values is not properly motivated, (3) the exclusion of certain observations is not properly motivated, (4) the integrity of the estimated coefficients on the unbundling variable is undermined by multicollinearity and omitted variable bias, and (5) the authors fail to address a large literature showing that unbundling does not increase broadband penetration and reduces investment by rival platform providers.

42. Before explaining those criticisms in detail, I briefly describe here what the authors have done. Using a regression analysis, the Berkman study seeks to demonstrate empirically that unbundling increases broadband penetration. In particular, the authors regress the total broadband penetration in a country ("QTOT") on, among other variables, (1) the (natural log of the) price of DSL ("LNPDSL"), (2) the percent of the population that resides in

76. We have already addressed the problems inherent to the OECD's penetration data above. Moreover, the OECD's urbanization data are known to be unreliable in countries with small populations where there is a concentration on a single, major city.

urban areas, (3) the percent of non-DSL lines of total broadband lines, and (4) the total number of years since a country imposed unbundling (“GUYRS”).⁷⁷ Their OECD-based dataset consists of 54 observations, 30 from 2005 and 24 from 2002. Following de Ridder, the authors ignore six data points from 2002 that “were deemed to be unreliable.”⁷⁸ The original values for GUYRS are from de Ridder (2007).⁷⁹ Using these data, the Berkman study claims to find that unbundling is significantly and positively related to broadband penetration in most of their specifications.⁸⁰

43. Next, the authors *replace* the original de Ridder values for GUYRS based on the following rationale: “After collection and review, a replacement set for the original GUYRS variable has been proposed. A change to this variable results in many more countries defined to have GUYRS = 0 than before.”⁸¹ Table 3 below reproduces the authors’ Table 4.8, which shows the original values for GUYRS from de Ridder and the replacement values for several observations.

TABLE 3: ALTERNATIVE VALUES FOR GUYRS (THE UNBUNDLING VARIABLE)

<i>Country</i>	<i>Original Value</i>	<i>Alternative Value</i>	<i>Difference</i>
Australia	6	1	5
Belgium	5	0	5
Canada	9	5	4
Germany	8	0	8
Greece	5	0	5
Ireland	5	0	5
South Korea	4	9	-5
Luxembourg	5	0	5
Netherlands	9	5	4
United Kingdom	5	0	5
United States	10	0	10

77. *Berkman Study, supra*, at 139-41.

78. *Id.* at 144.

79. *Catching-up in Broadband, supra*.

80. *Berkman Study, supra*, at 144.

81. *Id.* at 145.

After making these adjustments to 17 of the 30 observations in the OECD sample, the authors find that their regressions yield consistently significant results. In particular, they claim that “with this alternate specification, GUYRS now has t-statistics greater than 3 in all regressions where it is included.”⁸² They conclude that “Now GUYRS seems to have larger effects.”⁸³

44. Finally, the authors assign replacement values to the GUYRS variable once more; in this iteration, rather than being a *continuous* variable expressing the number of years since a country embraced unbundling, GUYRS is an *indicator* variable, taking on the value of one if (by the authors’ idiosyncratic definition) the country adopted unbundling during the relevant time period (2002-05) and zero otherwise. Using these replacement values, the authors purport to demonstrate that the indicator unbundling variable is significantly and positively related to broadband penetration in each of their specifications.⁸⁴

A. The unbundling variable (“GUYRS”) is picking up the effect of another phenomenon

45. Because of its construction, GUYRS is likely picking up the effect of variables unrelated to unbundling. I demonstrate below that GUYRS is simply a proxy for the length of time that broadband Internet has been available in the market, which itself is correlated with broadband penetration. Recall that the original definition of GUYRS is the total number of years since a country adopted unbundling.⁸⁵ For example, GUYRS in the United States is originally assigned the value 10 by de Ridder,⁸⁶ presumably because the Federal Communications Commission had implemented unbundling rules ten years before de Ridder did his analysis. Using these original values for GUYRS, the authors purport to find a statistically significant

82. *Id.* at 146.

83. *Id.*

84. *Id.* at 148.

85. *Id.*

86. *Id.* at 150.

relationship between GUYRS and broadband penetration, and they offer the following intuition for their results:

Since GUYRS has significant t-statistics even in models when the price is included, we now seek an explanatory mechanism for this residual effect. Because we have limited ourselves to DSL prices, this residual effect might come from reduced prices in other broadband options, or the generation of competition might lead to increased access to regions that were previously under-supplied with broadband. Since the regression with both price of DSL and Internet Penetration in the model has a non-significant GUYRS effect, this combination may be the best way to interpret how a legal [sic] unbundling results in increased broadband usage.⁸⁷

Stated differently, because their unbundling variable is not statistically significant when the authors control for total Internet penetration including dial-up connections (“SIP”) and the price of DSL (“LNPD”), the authors argue that “this combination”—that is, presumed lower prices for *cable modem* service plus increased deployment in previously underserved areas by DSL providers—must be the mechanism by which unbundling increases broadband penetration. The last sentence in the block quote is a non-sequitor; the authors’ conjecture that cable modem prices are somehow lowered by unbundling is *not* bolstered by a finding that the inclusion of the number of dial-up connections (a move from specification 5 to specification 4 in Table 4.4) removes most of the explanatory power of GUYRS. And there is no theoretical basis to believe that DSL providers would increase DSL deployment in response to the imposition of unbundling; if anything, the truncation of returns for an incumbent carrier caused by unbundling would induce it to *reduce* its deployment of current-generation infrastructure or to invest in the infrastructure of a non-regulated service or both.

46. Setting aside these logical errors, an alternative, and much more compelling, explanation of their results is that GUYRS, as it was originally constructed, is simply a proxy for the length of time that broadband Internet has been available in the market, which itself is likely

87. *Id.* at 144.

to be significant determinant of broadband penetration. Research by Boyle, Howell, and Zhang, which directly critiqued Mr. de Ridder and was cited by the Berkman study, has found that unbundling loses its explanatory power after controlling for the maturity of the market.⁸⁸ Surprisingly, the Berkman study chose to ignore completely this important criticism, as it does not present a single regression that controls for the time at which broadband was initially offered. Moreover, in the Berkman study, the significance of GUYRS disappears when the authors control for SIP and AGE, the percent of the population between 35 and 44.⁸⁹ When SIP⁹⁰ and AGE are included (see specification 4 in Table 4.4), GUYRS is no longer statistically significant; its t-value falls from 4.36 to 1.15. (A t-value of 2.0 or greater is typically required to achieve statistical significance at the 5 percent level.) Given these tenuous results, the authors appear to resort to ad hoc adjustments to the values of GUYRS, a point to which I turn next.

B. The replacement values for GUYRS appear to be highly subjective

47. The Berkman study altered the values of GUYRS for 17 of 30 observations, which had the effect of increasing the significance of their estimated effect of unbundling on broadband penetration. These ad hoc adjustments are inconsistent with the accepted practices employed by econometricians. The reason should be clear: If an econometrician can choose which variables to include in a regression and can assign *values* to those variables, then it is relatively easy to arrive at a particular result (such as a significant, positive coefficient on GUYRS). Table 4 provides the authors' brief justifications for changing the values for GUYRS.

88. Glenn Boyle, Bronwyn Howell and Wei Zhang, *Catching up in Broadband: Does Local Loop Unbundling Really Lead to Material Increases in OECD Broadband Uptake?* New Zealand Institute for the Study of Competition and Regulation Working Paper (July 2008).

89. *Berkman Study, supra*, at 143 (Table 4.2).

90. It is also worth noting that SIP, by capturing the number of total Internet lines including dial-up, is a country-specific demand effect that likely accounts for the particular preference for broadband in a country—which itself may be the result of the pricing structure for dial-up use (flat-rate in the U.S.). As explained earlier, a large proportion of U.S. dial-up subscribers would not switch to broadband under any circumstances.

TABLE 4: ALTERNATIVE VALUES FOR GUYRS

<i>Country</i>	<i>Original Value</i>	<i>Alternative Value</i>	<i>Justification</i>
Australia	6	1	Reflects the resistance by Telstra, and the high prices until the competition notice issued in March 2004 by ACCC, which reportedly led to lower prices.
Belgium	5	0	Reflects continuing complaints, to this day, about absence of LLU; all entrants who are not facilities based, about 22% of the market, are resellers. This suggests no real competition on network management, which may explain the data cap anomaly.
Canada	9	5	Reflects initial sunset and high LLU rates, followed by the 2001 decision to extend LLU indefinitely and reduction in service charges.
Germany	8	0	Reflects DT resistance and BnetzA lack of capacity
Greece	5	0	Reflects the fact that only in May of 2007 was LLU regulation changed, and a new framework put in place that seems to have resulted in a shift of some of the wholesale/carrier pre-selection model operators shifting to more unbundling
Ireland	5	0	Eirecom [sic] litigated extensively to delay the regulator, and succeeded in delaying implementation of unbundling throughout the relevant period.
South Korea	4	9	Reflects the fact that Thrunet entered over access to incumbent facilities—cable owned by Kepeco.
Luxembourg	5	0	Justified by claim that incumbent effectively only offers an unregulated wholesale product to competitors, with no effective enforcement to the contrary
Netherlands	9	5	Reflects exclusion of competitors from naked DSL until 2001.
United Kingdom	5	0	Reflects unobserved BT resistance that led to massive jump in unbundled loop usage after functional separation implemented in 2005
United States	10	0	Reflects 2001-02 FCC decisions to shift to intermodal competition

Note: Reproduced from Table 4.8 (“Alternative values for GUYRS based on actual adoption patterns”).

To be fair, unbundling is a difficult variable to measure. But the ability to assign values based on subjective standards such as eircom (the incumbent network operator in Ireland) litigated too much, or that Canadian LLU rates were too high, or that unbundled loops in the United Kingdom did not have a large uptake until after Openreach was established, or that Deutsche Telekom resisted too much, affords the researcher too much discretion. The notion that two researchers looking at the same data could assign the value of ten (de Ridder)⁹¹ and zero (Berkman) to the United States suggests that the exercise is arbitrary. Indeed, this latter data adjustment is extremely hard to reconcile with the fact that loop unbundling at TELRIC has been mandatory in

91. *Catching-up in Broadband, supra*, at 12 (citing the use of Table 2.10 in the 2005 OECD broadband report to construct his unbundling variable).

the United States since 1996, and it has not been changed by any subsequent FCC or court decision. Moreover, during de Ridder's and Berkman's study period (2002-2005), the United States had a very significant number of unbundled loops.⁹² The authors appear to have decided whether a country was sufficiently exposed to unbundling based on how they evaluated the country's broadband performance. In what follows, I briefly review the rationales offered by the study's authors for some of the significant changes in the value of GUYRS.

1. Canada

48. An inspection of the regulatory history in Canada suggests the authors' reclassification from 9 to 5 is not justified. In May 1997, the Canadian Radio-Television and Telecommunications Commission ("CRTC") enacted unbundling rules, including price controls and local loop unbundling.⁹³ Although the CRTC originally intended to rescind unbundling regulations on ILECs after a five-year "sunset" period, the CRTC extended the policy indefinitely.⁹⁴ Competitors began offering services via unbundled local loops in 1998.⁹⁵ Between 1999 and 2002, the share of unbundled loops in Canada increased approximately 23 percent, while the share of resold lines decreased 22 percent.⁹⁶ Despite this record of unbundling, the Berkman Study reclassifies Canada's GUYRS value at 5 (from an original value of 9).⁹⁷ The above data, however, clearly corroborate the original GUYRS value of 9, corresponding to the 1997 CRTC unbundling regulations.

92. See OECD's *Communications Outlook 2009*, Table 2.9.

93. Jerry A. Hausman & J. Gregory Sidak, *Did Mandatory Unbundling Achieve its Purpose? Empirical Evidence from Five Countries*. 1(1) JOURNAL OF COMPETITION LAW AND ECONOMICS 226 (2005) [hereinafter *Hausman & Sidak*].

94. *Id.* at 227.

95. *Id.*

96. *Id.* at 233.

97. *Berkman Study, supra*, at Table 4.8.

2. Germany

49. The authors' reclassification of the unbundling variable in Germany is not supported by the facts. The German Regulator of Telecommunications and Post ("RegTP") took the first steps toward mandating local loop unbundling in Germany through the Telecommunications Act of 1996. The main provision of the Act was to require that a "dominant" operator allow new market entrants to interconnect to its network.⁹⁸ In January 1998, new entrants began leasing local loop infrastructure from incumbent giant Deutsche Telekom, offering both voice and data services.⁹⁹ Competition spread quickly in the market, as RegTP reported that by mid-1999, 51 providers had signed contracts with Deutsche Telekom for local loop access. Between 2000 and 2002, DSL subscribership through Deutsche Telekom and unbundled access providers increased by more than 450 percent.¹⁰⁰ These facts are hard to reconcile with the Berkman Study's reclassification of Germany's GUYRS metric as a zero (implying no history of unbundling) from an original value of 8.¹⁰¹

3. South Korea

50. The logic for reclassifying South Korea is also strained. The Berkman Study seeks to justify its reclassification of South Korea's GUYRS value from 4 to 9 on the grounds that Thrunet leased lines from government-owned incumbent KT beginning in 1998.¹⁰² This leasing program, however, was not the result of local loop unbundling, which was not enacted

98. *Id.* at 235.

99. Ashish Kelkar, *Economics of unbundling the local loop through provision of DSL*, Massachusetts Institute of Technology Working Paper 6, available at <http://in3.dem.ist.utl.pt/downloads/cur2000/papers/S27P06.PDF>.

100. *Hausman & Sidak, supra*, at 237.

101. *Berkman Study, supra*, at Table 4.8.

102. *Id.*

until 2002.¹⁰³ The South Korean government involved itself directly in promoting competition by offering heavy subsidies for capital investments. The government provided the impetus for competition on pricing, infrastructure development, and quality of service by allowing KT to lease lines, without imposing unbundling.¹⁰⁴ As such, the Berkman study's reclassification is unsupported. Broadband rollout by new entrants in South Korea was also spurred by the fact that in the multi-unit dwellings common throughout the country, landlords, not incumbents like KT or Dacom Powercomm, own the local loop infrastructure.¹⁰⁵

C. The authors seek to exclude certain observations that reduce their model's goodness-of-fit

51. Econometricians generally do not throw out observations. Even when an observation appears to be an “outlier,” the observation is generally retained and, if necessary, an appropriate test is used to determine the effect of the purported outlier on the estimation results. Yet the study's authors insist that the proper way to deal with Switzerland—a country that rejected unbundling yet has a high level of broadband penetration according to the OECD—is to remove it entirely. The authors offer several rationales, each of which is presented here:

- “We feel confident that the only country whose removal generated a large change in the point estimate was Switzerland, and removing Switzerland would have produced a much larger coefficient effect, not a smaller one.”¹⁰⁶
- “Switzerland has many covariates similar to high GUYRS countries, specifically a low price for broadband, though it did not adopt unbundling.”¹⁰⁷
- “The inclusion of Switzerland in the dataset tends to reduce the t-statistic for GUYRS, since it is a country that did not adopt unbundling, though it has significant broadband usage.

103. Robert D. Atkinson, Daniel K. Correa & Julie A. Hedlund, *Explaining International Broadband Leadership*, The Information Technology and Innovation Foundation F3 (2008) available at <http://www.itif.org/files/ExplainingBBLeadership.pdf>.

104. *Id.*

105. *Id.*

106. *Berkman Study, supra*, at 140.

107. *Id.* at 141.

- Eliminating Switzerland may result in a doubling of the t-statistic for some of these regressions.”¹⁰⁸
- “However, the country of Switzerland seems to have a significant dampening effect on regressions #1, #2, #5, and #6. Switzerland represents the right most outlier in those histograms. Eliminating Switzerland results in much more significant models than when it is included.”¹⁰⁹

None of these rationales has merit. The fact that Switzerland defies the authors’ belief about the relationship between unbundling and broadband penetration is no reason to exclude that observation from the regression. At a minimum, Switzerland’s high penetration demonstrates that unbundling is not a *necessary* condition for high levels of penetration. (It bears noting that the authors did not change the original value of GUYRS for Switzerland, presumably because they could not identify a plausible basis.) The authors’ inclination to exclude this data is consistent with a broader policy objective of supporting unbundling.

52. Other observations in the authors’ limited dataset suggest that unbundling is not a *sufficient* condition for high broadband penetration. Consider Germany, a country that embraced unbundling (original GUYRS = 8 in 2005) but had below-average broadband penetration in 2005 ($QTOT_{\text{Germany}} = 12.97$, Average $QTOT = 14.14$). Not satisfied with the role played by Germany, the authors replace the original value of GUYRS (8) with a new value (0), and they offer the following rationale: “Reflects DT resistance and BnetzA lack of capacity.”¹¹⁰ When Germany reenters the dataset with a low GUYRS (0) and a below-average penetration rate (12.97), their model appears to perform better. But there are no lessons to be learned here, other than the fact that the selective reassignment of values can make a regression say anything.

53. Finally, if an individual country such as Switzerland should be excluded because its inclusion *reduces* the goodness of fit, then why not exclude other observations such as

108. *Id.* at 144.

109. *Id.* at 145.

110. *Id.* Table 4.8.

Finland, which *improve* the goodness of it? Indeed, the authors admit that “In Regression #5 (LNPDSL + AGE + UURB+GUYRS+DUMMY), eliminating Finland is enough to take the GUYRS coefficient out of statistical significance.”¹¹¹ This admission reveals the tenuous nature of the authors’ results: One data point is removed and the empirical basis for unbundling unravels. (It bears noting that the authors did not choose to change the original value of GUYRS for Finland, presumably because this country conformed to the authors’ *a priori* belief that unbundling causes penetration to rise.) In general, data should not be included or excluded in a regression merely because they support or reject the researcher’s *a priori* beliefs on how the world works. Economists are highly skeptical of results-oriented methods.

D. Because two of the authors’ explanatory variables, the price of DSL and GUYRS, are positively correlated, the coefficient on GUYRS is difficult to interpret

54. The authors seek to estimate the marginal effect of unbundling on broadband penetration.¹¹² Accordingly, the primary objective of the authors’ regression is to obtain a precise estimate of the coefficient on GUYRS. (In contrast, the authors do not appear to use their model to predict broadband penetration in a country given its characteristics.) When the objective of a regression analysis is to estimate marginal effects, as is the case here, great care must be given to identifying these effects with precision. Unfortunately, this precision can be elusive if there is “multicollinearity”—that is, a statistical phenomenon in which two or more explanatory variables in a regression are correlated.¹¹³ In this situation, the coefficient estimates of any individual variable may change substantially in response to small changes in the model or the data. While multicollinearity may not result in biased coefficients, it produces large standard errors in the coefficients on the related explanatory variables. Moreover, if there are any other

111. *Id.* at 145.

112. *Id.* at Tables 4.2-4.7.

113. *Ramanathan, supra*, at 233.

problems that could introduce bias, multicollinearity can exacerbate the effects of this bias. (See section on omitted variable bias, below.) Applied here, adding more years of data or adding new explanatory variables to the regression could cause the sign of the coefficient on GUYRS to become negative, suggesting that unbundling decreases broadband penetration.

55. It bears noting that this critique relating to multicollinearity would not apply here if the purpose of the authors' regression were different. In particular, if the objective of the regression were to provide the best prediction for broadband penetration in the United States (or in any other country), then concerns relating to multicollinearity would not matter. But the authors are not interested in predicting broadband penetration in the United States (or any other country). Instead, their focus is on estimating the (average) incremental effect of unbundling on broadband penetration across a sample of countries.

56. The problem relating to multicollinearity arises here because (1) the price of DSL (LNPDSL) and unbundling (GUYRS)—two explanatory variables in the authors' regression model—are positively correlated, and (2) the sample size in the regressions is particularly small. As a matter of economic theory, unbundling may increase output by lowering broadband prices, including DSL prices. Indeed, in a regression of GUYRS on LNPDSL and other variables, the authors find that the coefficient on LNPDSL is negative and statistically significant (t-value = 2.82).¹¹⁴ Given this strong correlation, the covariance between the regression coefficients of LNPDSL and GUYRS will be high, thus making it difficult to interpret individual coefficients. Despite finding that GUYRS and LNPDSL are highly correlated, the authors take no steps to address the problems associated with multicollinearity. The preferred solution among econometricians is to obtain additional data, which can produce more precise parameter

114. *Berkman Study, supra*, at 141.

estimates with lower standard errors. Given the small sample size here (54 observations), however, the coefficient on GUYRS cannot be trusted when making out-of-sample predictions unless the explanatory variables follow the same pattern of multicollinearity as the data on which the regression model was fitted.

E. Although poverty and income were shown to be important explanatory variables in an earlier regression of broadband penetration, the authors omit those variables when modeling the effect of unbundling on broadband penetration

57. In addition to ignoring the problem of multicollinearity, the authors omit critical variables in their unbundling regressions, which has the effect of biasing their estimate of the marginal effect of unbundling. Recall that the authors performed an additional econometric exercise in a separate part of their report (Annex 3.8), which examined the relationship between broadband penetration and the poverty rate, income per capita, urbanicity, and education.¹¹⁵ The authors find in that section that *each* of these variables is significantly related to broadband penetration.¹¹⁶ Yet when they proceed to estimate the incremental effect of unbundling on broadband penetration (Annex 4.13), the authors neglect to include poverty and income as explanatory variables.

58. As I explained above (Part I.B.), the estimated coefficient on the key explanatory variable will be biased—and therefore unreliable—unless the omitted variable is *uncorrelated with every included variable*.¹¹⁷ Applied here, the coefficient on GUYRS is biased so long as GUYRS (or any other included explanatory variable) is correlated with the poverty level, income per capita, or any other potential omitted variable that is important. As it turns out, GUYRS, as it was defined in Boyle-Howell-Zhang and initially adopted in the Berkman study, is correlated

115. *Id.* at Annex 3.8.

116. *Id.*

117. *Ramanathan, supra*, 186.

with income per capita (correlation coefficient = 0.474) and with poverty (correlation coefficient = -0.129). Other omitted variables, including whether the network is state-owned (as is the case of Turkey and Luxembourg), are also statistically significantly related to broadband penetration. Because GUYRS is also correlated with a state-ownership indicator (correlation coefficient = -0.226), the estimated coefficient on GUYRS is further biased. In particular, given these correlations with GUYRS,¹¹⁸ and given the correlation between these omitted variables and broadband penetration, omitting these variables biases the study's estimated coefficient on GUYRS *upwards*—that is, omitting these variables (conveniently) strengthens the authors' conjecture that “a legal unbundling results in increased broadband usage.”¹¹⁹ Until these and other critical omitted variables—that is, variables that are correlated with both the dependent variable and an included explanatory variable—are properly accounted for in the regression, the authors' estimated coefficient on GUYRS cannot be relied upon to make inferences about the role of unbundling on broadband penetration.

F. Prior research shows that unbundling policies are not correlated with increases in broadband penetration

59. Additional research demonstrates that unbundling policies have at best, a weak correlation, and at worst, a significantly negative correlation with increases in broadband penetration. The Berkman study fails to account for these findings. For example, using a panel dataset of OECD countries, Wallsten measured the incremental effects of local loop unbundling. He concluded that, when controlling for country and year effects, “unbundling has no robustly significant impact on broadband penetration. More extensive ‘subloop unbundling,’ however, is

118. The correlation between poverty, income per capita, and state-ownership with broadband penetration is -0.42, 0.57, and -0.19, respectively. Omission of a variable that is negatively (positively) related with the explanatory variable and negatively (positively) related with the dependent variable implies an upward bias in the coefficient of the explanatory variable.

119. *Berkman Study, supra*, at 144.

negatively correlated with penetration.”¹²⁰ Mandatory unbundling cannot be counted on to increase broadband penetration; country-specific effects have a substantial impact on the efficacy of mandatory unbundling. Other major findings on the output effects of unbundling include:

- Aron and Burnstein (2003) estimate the effect of intermodal competition on broadband penetration, relative to the effect of simple broadband availability, while controlling for various demand and cost drivers.¹²¹ The regression results reveal that the relationship between unbundled access prices and penetration is not statistically significant.¹²²
- Bauer, Kim, and Wildman (2003) examine the effect of various policy variables on broadband penetration, including unbundling, cable-telco cross ownership, and government funding for broadband.¹²³ The analysis fails to detect any statistically significant relationship between membership in a given policy cluster and broadband penetration.¹²⁴
- Denni and Gruber (2005) analyze biannual state-level panel data from 1999 to 2004 in an effort to determine the extent to which intra- versus inter-platform competition affects broadband penetration, using a logistic model of technology diffusion.¹²⁵ The authors find that unbundling actually inhibits broadband penetration. Specifically, the results indicate that the share of central offices upgraded for equal access has a negative and statistically significant effect on the rate of broadband diffusion.¹²⁶
- Distaso, Lupi, and Manenti (2005) estimate a model of oligopolistic competition between differentiated products to analyze the relative importance of intra-platform competition and inter-platform competition in driving broadband adoption.¹²⁷ The authors find that, although inter-platform competition is a substantial driver of broadband adoption, competition within the market for DSL services—the type of intra-platform competition that unbundling of broadband is supposedly designed to stimulate—does not play a significant role.¹²⁸
- Cava-Ferreruela and Alabau-Munoz (2006) analyze data from a panel of 30 OECD countries from 2000 to 2002 to explore the determinants of wireline broadband coverage.¹²⁹ The analysis

120. *Broadband and Unbundling Regulations, supra.*

121. Debra Aron & David Burnstein, *Broadband Adoption in the United States: An Empirical Analysis*, Working Paper, LECG Ltd. (March 2003).

122. *Id.* at Table 3.

123. Johannes Bauer, Jung Kim, & Steven Wildman, *Broadband Uptake in OECD Countries: Policy Lessons and Unexplained Patterns*, Paper prepared for the European Regional Conference of the International Telecommunications Society (August 2003), at 14.

124. *Id.* at 17-18.

125. Mario Denni & Harald Gruber, *The Diffusion of Broadband Telecommunications: The Role Of Competition*, Paper presented at the International Communications Society Conference (2005).

126. *Id.* at 13.

127. Walter Distaso, Paolo Lupi, & Fabio Manenti, *Platform Competition And Broadband Uptake: Theory And Empirical Evidence From The European Union*, Paper presented at the joint PURC - University of Florida and LBS 2005 telecommunications conference (April 2005).

128. *Id.* at 16. As discussed in more detail below, to the extent that opportunistic regulators choose to lower access prices in response to increased levels of broadband investments by incumbents, this assumption is invalid, and results in a spurious negative correlation between penetration and unbundling prices.

129. Inmaculada Cava-Ferreruela & Antonio Alabau-Munoz, *Broadband Policy Assessment: A Cross-National Empirical Analysis*, TELECOMMUNICATIONS POLICY 30 (2006).

indicates that neither the existence of unbundling regulations nor the number of unbundled loops are found to be significantly correlated with DSL coverage.¹³⁰

- Waverman Meschi, Reillier, and Dasgupta (2007) employ a panel of 12 European countries from 2002-2006 to estimate the effect of unbundling on broadband penetration.¹³¹ The authors report that the number of years since adoption of unbundling has no statistically significant effect on penetration, and that the coefficients on the unbundled price variables are negative and statistically significant.
- Boyle, Howell, and Zhang (2008) responded to Mr. de Ridder's analysis of 2002 and 2005 OECD data. The authors found two key problems with Mr. de Ridder's research. First, upon estimating robust standard errors, which are needed to correct for heteroskedasticity, the statistical significance of GUYRS disappears. Second, GUYRS is merely serving as a proxy for the diffusion of the broadband technology. Once this is controlled for by including the age of the broadband offering, GUYRS becomes insignificant. Therefore, unbundling is not found to increase broadband penetration. Again, it is worth noting that although the Berkman study summarized Boyle-Howell-Zhang, it did not mention this particular criticism of de Ridder, nor did it attempt to estimate regressions that control for broadband diffusion.

Once again, it is hard to reconcile the findings of the Berkman study with this large body of literature.

G. Prior research shows that unbundling policies discourage investment by entrants, which implies that dynamic efficiency losses must be weighed against static output effects

60. Finally, several economists have attempted to measure the incremental effect of unbundling on investment by rival platform providers on current-generation networks or by incumbent providers on next-generation networks. Yet the Berkman study makes no attempt to measure these important "dynamic effects." Nor does the study try to weigh the alleged positive static effects of increased output with the potential negative dynamic effects. I briefly review the relevant literature here.

- Grajek and Roller (2009) show that access regulation negatively affects both total industry and individual carrier investment, and that regulators respond endogenously to incumbent infrastructure investments by providing easier access to entrants.¹³²

130 *Id.* at 455.

131 Leonard Waverman, Meloria Meschi, Benoit Reillier, & Kalyan Dasgupta, Access Regulation and Infrastructure Investment in the Telecommunications Sector: An Empirical Investigation, Working Paper, LECG Ltd. (Sept. 2007).

132. *Grajek & Roller, supra.*

- Crandall, Ingraham and Singer (2004) find that unbundling encourages a CLEC to delay facilities-based investment by altering its relative net present value of investment between time periods.¹³³
- Hazlett (2005) finds that cable companies invested more quickly in unregulated broadband networks than did telephone companies, who were required to give access to broadband facilities to competitors.¹³⁴
- Wallsten and Hausladen (2009) investigate the effects of unbundling on the penetration of next-generation broadband technology.¹³⁵ Estimating separate equations for incumbents and entrants, the authors find, in both cases, that the relationship between unbundling and fiber per capita is negative and statistically significant.¹³⁶

Once again, the Berkman study is largely silent with respect to these findings relating to dynamic efficiency effects. It is impossible to make informed policy prescriptions without considering *both* the potential benefits (increased immediate output) and costs (decreased investment) of unbundling or “open access” regulations. Indeed, dynamic efficiency is typically much more important to consumers than static efficiency; it leads to missing and delayed markets where producer and consumer surplus are missing in their entirety, while static inefficiency may lead only to a relatively small deadweight losses in consumer and producer surplus.¹³⁷

133. *Crandall, Ingraham & Singer, supra.*

134. *Rivalrous Telecom Networks, supra.*

135. *Wallsten & Hausladen, supra.*

136. As a practical matter, the architecture of the next-generation networks that have deployed in the United States would make unbundling either extremely costly or flatly infeasible. See Robert Crandall, Jeffrey Eisenach, & Robert Litan, *Vertical Separation of Telecommunications Networks: Evidence from Five Countries*, FEDERAL COMMUNICATIONS LAW JOURNAL (forthcoming 2009).

137. See, e.g. Austin Goolsbee, *The Value of Broadband and the Deadweight Loss of Taxing New Technology*, 5(1) CONTRIBUTIONS TO ECONOMIC ANALYSIS AND POLICY, 1-29, at 4 (2006) (finding that the dynamic efficiency losses from a hypothetical tax applied to broadband Internet in the U.S. exceed the allocative efficiency losses by a factor of two or three); Robert Solow, *Technical Change and the Aggregate Production Function*, 9(3) REVIEW OF ECONOMICS AND STATISTICS, 312-320 (1957) (concluding that approximately 87 percent of the source of economic growth in the United States in the first half of the 20th century could be explained by technical change, rather than by increases in capital and labour); Paul Romer, *New Goods, Old Theory, and the Welfare Costs of Trade Restrictions*, 43(1) JOURNAL OF DEVELOPMENT ECONOMICS, 5-38. (1994) (finding that the dynamic efficiency losses from an import tariff could be as much as 20 times allocative efficiency losses).

CONCLUSION

61. The Berkman study advocates a benchmarking methodology to analyze broadband penetration across countries, which fails to consider critical variables that explain inter-country variations. In defense of this methodology, the authors adopt an unorthodox approach—namely, estimating separate univariate regressions of broadband penetration on variables that are recognized to drive broadband adoption. Although these separate univariate regressions indicate that the United States lies along the predicted line—suggesting that the United States is *not* a laggard in broadband adoption—the authors conclude otherwise. This conclusion is further refuted by a multivariate regression analysis that shows that, when *simultaneously* controlling for these important variables, broadband penetration in the United States is slightly higher than its predicted value. Accordingly, the United States is not a laggard in broadband penetration.

62. Another concern with the Berkman study is its reliance on flawed or questionable data. In particular, numerous scholars have discussed the many problems that exist in the OECD broadband data. Given that these data are not collected in a consistent manner across countries, it is particularly troubling that the Berkman study relies on them so heavily to develop country rankings. These rankings could change dramatically if corrections were made for different data collection methods across countries.

63. The data integrity concerns in the Berkman study are not limited to the OECD broadband penetration data, however. In its analysis of bandwidth-adjusted price, the Berkman study relies on offerings in the United States that either (1) do not exist, (2) are not comparable to offerings in other countries, or (3) are almost never selected by the consumer. Therefore, the

Berkman study's figures that purport to show non-competitive offerings by U.S. service providers are based on faulty comparisons, and therefore misleading.

64. The Berkman study's analytical deficiencies are then compounded when it attempts to estimate the impact of unbundling by altering the key independent variable in the analysis. In particular, the authors altered 17 of 30 values of their key right-hand-side variable, GUYRS. A cursory review of just a handful of these data alterations reveals that the authors' justification for their changes was unwarranted. Moreover, the selective exclusion of countries such as Switzerland—characterized by high broadband penetration and *no* unbundling—in key regressions is not consistent with accepted econometric practices.

65. Even if these flaws in the Berkman study were insufficient to call its analyses and conclusions into question, the authors also selectively omitted any discussion of a wide body of existing literature on unbundling. In particular, a large body of research shows that unbundling either does not improve broadband penetration or actually impairs it. Moreover, even to the extent that one believes that unbundling of legacy networks improves short-term broadband adoption, this short-term benefit ignores the possible long-term loss in dynamic efficiency. Published research has shown that unbundling discourages investment in new networks. Any possible short-term gain in unbundling—to the extent it may exist—would likely be offset by a long-term loss to consumers.

I declare under penalty of perjury that the foregoing is true and accurate.

/s/ Robert W. Hahn

November 16, 2009