
In Review

Another Antitrust Consensus?

REVIEWED BY RICHARD L. GORDON

Antitrust Law and Economics

Edited by Keith N. Hylton

298 pages; Edward Elgar, 2010

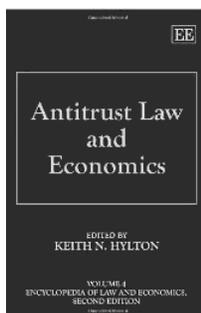
A decade ago, Edward Elgar Publishing released its *Encyclopedia of Law and Economics* in five thick volumes. A revision is now in process and involves more numerous, more specialized, and at least in this case, shorter volumes. In particular, the volumes cover standard areas of the law school curriculum.

This review looks at the antitrust volume, edited by Boston University law professor Keith Hylton. The volume is an unusual, and positive, contribution to the antitrust literature. Separate articles, each by different authors, provide a survey of basic topics in antitrust. Thus, we have a shorter, multi-author alternative to the long-extant antitrust textbooks.

For example, the chapters cover most of the same topics covered in Herbert Hovenkamp's 2005 one-volume *Federal Antitrust Policy: The Law of Competition and Its Practice* (one of several different antitrust texts of which Hovenkamp is an author). That book has 841 pages, as compared to Hylton's 298. (On the other hand, Hovenkamp costs \$85 new on Amazon.com, while Hylton costs \$172.) Clearly, the Elgar volume seeks to provide terse treatments from varied sources. Thus, those readers unwilling to wade through a massive text and those looking for alternative views will benefit most from this offering.

The volume's contributors differ

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greatly in outlook, ranging from noted skeptics about much of antitrust to Jonathan Baker, an advocate of continued strong enforcement. With the exception of Baker — a believer in University of Chicago overreach — and Michael Meurer, the tone is post-post-Chicago: contributors generally reject efforts to undermine University of Chicago-generated criticisms on much of antitrust.

A look inside | Daniel Crane's opening chapter tersely navigates the morass that is antitrust enforcement. Initiative is divided among two federal agencies, the 50 states, and aggrieved private parties. Civil and criminal charges are possible.

Crane raises a theme that pervades the book. In a classic 1984 *Texas Law Review* article on antitrust, Frank Easterbrook presented the proposition that incorrect guilty verdicts were more problematic than incorrect acquittals; market forces can counteract the inefficiencies that were upheld, but erroneous court successes are irreversible. Crane also endorses the principle that economic efficiency is the proper goal of antitrust. In turning to the specifics, he does well at balancing views on the many problems of divided authority. Predictably, the most extensively treated issue is the nature and control of private initiatives.

Six of the remaining chapters cover different concepts of supposedly undesirable

antitrust practices. Overall, the writers conclude that the last three decades have produced a desirable skepticism in the courts about the dangers those practices pose. The difficulty of determining how, if at all, those tactics differ is left tacit.

In his chapter, William Page treats deciding whether industry practices illegally facilitate monopolization. As Page somewhat opaquely indicates, the problem is distinguishing efforts to collude on price from beneficial behavior. He correctly considers the prevailing state of law unsatisfactory because it does not clearly articulate how to evaluate practices. Improper practices involve agreeing to and complying with adoption of techniques such as forbidding discounts. Page feels that the law should more clearly require that communications among firms produce the practices.

Jeffrey Harrison covers the related topic of boycotts. His stress is on rules of organizations such as sports leagues and trade associations. However, examples are made of dealings between a single seller and its buyers and a buyer with its supplier. Again, the problem arises of correctly assessing the effects.

Roger Blair and Celeste Carruthers present the first of two chapters dealing with defining and identifying monopoly. The bulk of the discussion treats the relevant economics. This includes the economics of perfect competition, pure monopoly, and a dominant firm with a fringe of competitors, as well as the classic 1934 index of monopoly power devised by Abba Lerner. They conclude with a sketch of the considerations that courts use in antitrust cases, such as market definition, market shares, and ease of entry. The biggest problems with the chapter are its terseness and relegation of mention of the key literature to the notes and references.

Hylton extends the analysis with a review of the history of how courts treated the issue. He reports various appraisals of these practices, presents and appraises for-

mulas that embody proposed approaches, and then presents an integrated review of these principles. He argues that the Supreme Court in the 1911 *Standard Oil* decision introduced the principle that illegality arose only if the acquisition of monopoly was intentional, but the court's 1945 *Alcoa* decision lessened that requirement, though the court would restore it in later cases. He distinguishes several criteria for determining monopolization that subdivide between those stressing effects and those focusing on whether the intent was to limit competition. He ends up saying the choice among them depends on the type of case involved.

Predation | Bruce Kobayashi ably and thoroughly reviews the curious literature on predation and its legal impact. He appropriately divides his literature review into two parts.

The first part treats the enormous theoretical and empirical literature inspired by John McGee's 1958 article in the *Journal of Law and Economics* rethinking the theory and facts of *Standard Oil*. McGee argued economic theory suggested predation was not a viable technique and *Standard Oil* did not engage in the tactic. An enormous literature ensued arguing that circumstances arose in theory in which predation worked and further examining the empirical evidence. Kobayashi correctly observes that the new theories on predation have two severe drawbacks: First, while predation as formerly defined was always inefficient, successful use of these new concepts of aggressive competition might or might not be inefficient. Secondly, the information needed to evaluate these practices exceeds what is practically available in antitrust litigation. The empirical work is a mixed bag, with some papers extending McGee's conclusions to other cases, other papers refuting McGee, and still others finding no predation.

The second part of Kobayashi's chapter concerns a 1975 *Harvard Law Review* article by Phillip Areeda and Donald Turner (the latter of whom served as the assistant attorney general for antitrust). Areeda and Turner started with the basic point that the economic criterion of pre-

dition is selling below marginal cost, to which they argue that, given the difficulty of measuring marginal costs, average variable costs should be used instead. Many other observers, mostly academic economists, tried with questionable success to improve on this suggestion by Areeda and Turner. Kobayashi notes that in two critical cases on predation — one involving a charge against Japanese television manufacturers by their American rivals and the other involving discounts given by a producer of private-brand cigarettes — the Supreme Court adopted the case for skepticism about predation. Kobayashi shows that this skepticism should be extended to other practices, such as aggressive bidding for resources. (Disappointingly, he fails to discuss the true defect of Areeda and Turner, which is that when their unrealistic assumption that a firm produces only one product is dropped, the average cost of any product is a nonsense concept.)

Thomas Cotter grapples with the essential-facilities concept. He notes the wide disagreement about whether this idea has content. He observes that the cases are few and ambiguous and cover various problems in purchasing an essential commodity. Cotter nicely reports the ambiguities of the legal precedents and the literature expressing skepticism about the reality of the problem and the ability of courts to intervene sensibly. His literature citations are thin, unfortunately; in particular, the writing on the morass that is local-network access in telecommunication is neglected.

Alden Abbott and Joshua Wright nicely show why in neither theory nor practice is tying and exclusive dealing likely to be anti-competitive. In contrast, Shubha Ghosh advocates an approach to restraints that suppliers place on retailers that recognizes both the quality assurance benefits and the possibility of undermining price competition.

Return of antitrust? | Jonathan Baker's contribution is effectively a last-ditch post-Chicago effort to rekindle classic antitrust ideas. Its essence is that increasing market shares among already-large

firms makes cooperative behavior among firms more likely. This is virtually tautological and Baker's way of making the case is unsatisfactory. He trots out a standard game theory model of long-term competition that indicates the likelihood of cooperation rather than defection increases with higher market shares. Baker dutifully notes that other influences matter, but does not deign to cite, let alone to discuss, classic criticisms of market-share arguments by such writers as Harold Demsetz, John McGee, and William Landes and Richard Posner.

Baker's choice to condemn a decision not to challenge the Whirlpool-Maytag merger is one I attacked in a review of a contribution Baker coauthored to an earlier anthology. Here he worsens his argument by claiming that Whirlpool and Maytag focus on a unique market, low-end top-loading washers, while foreign competitors concentrate on high-end front-loaders. After double-checking that my Maytag washing machine is front-loading, I visited the Internet sites of two leading chain stores that sell top-loading washing machines and found that General Electric remains in the market, at least four foreign-owned companies participate, and the price ranges overlap.

Michael Meurer's effort to cover intellectual property is the book's outlier: where all of the other papers are useful discussions of real-world antitrust issues, Meurer's treats what seems to be a purely hypothetical problem. He chooses to develop an abstract model of optimum regulation under the assumption that the inventor, the assumed single rival, and the government all know the odds of success at invention. Reference to practice is sketchy. While this might be a good article for one of the many journals devoted to abstract economics, its practical relevance is not discussed and seems nonexistent.

Final thoughts | The book largely supports the view that misuse of antitrust has prevailed but can be overcome. The treatment is readable, but readers will differ on whether a short anthology is an improvement on the extensive literature covering the area. R

Burying Good Ideas

REVIEWED BY DAVID R. HENDERSON

Zombie Economics: How Dead Ideas Still Walk among Us

By John Quiggin

238 pages; Princeton University, 2010

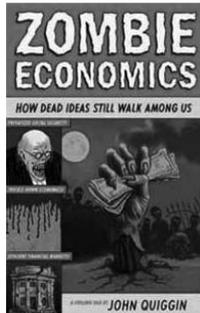
In his book *Zombie Economics*, subtitled “How Dead Ideas Still Walk Among Us,” Australian economist John Quiggin tries to bury five major economic ideas that many mainstream economists have accepted for the last two decades or more. Two of the ideas are in macroeconomics, the part of economics that studies Gross Domestic Product, inflation, and unemployment; one idea is in finance; and two ideas are in microeconomic policy.

Although Quiggin is obviously a smart economist who has thought a lot about these issues and often has good criticisms and insights, three of the five ideas hold up well despite his efforts to put them to rest. On one of the ideas, he delivers some heavy blows, and it is not clear whether the idea should be revived. But in only one of the five cases does Quiggin make a persuasive case for burying an idea.

DSGE | Quiggin is on his strongest ground in rejecting much of modern macroeconomics. One of the ideas that he effectively buries is dynamic stochastic general equilibrium (DSGE), the idea that the macroeconomy can be modeled in a manner that adequately takes account of shocks, technological change, preferences, and institutions. DSGE became all the rage in the 1990s and 2000s.

In my opinion, although possibly not in Quiggin’s, the originators of DSGE started with good intentions. They wanted to build

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a macroeconomic framework on top of microeconomic principles. But they did so by making extremely unrealistic assumptions.

One of the most extreme assumptions, notes Quiggin, is that of the “representative agent.” In DSGE analysis, the millions of flesh-and-blood humans with different tastes and abilities are collapsed into one person, and the thousands of goods and services are collapsed into one good. Writes Quiggin: “The fact that people differ massively in their tastes, wealth, and good or bad economic fortune is assumed away on the basis that all these differences should cancel out in the aggregate.” Quiggin quotes economist Charles Goodhart’s lament that DSGE “excludes everything I’m interested in.”

To say that Quiggin has good reason to reject DSGE is not to say that his preferred alternative is strong. Quiggin accepts a Keynesian view of macro, but he gives only scattered evidence for that view and some of his evidence is misleading. Comparing the economies of Australia and New Zealand, for example, Quiggin points out that the New Zealand government in the 1980s cut the size of its government sector, whereas the Australian government did not. By 2000, he writes, “when New Zealand finally abandoned radical reform, income per person was one-third less than Australia’s.” But that is no basis for comparing policies at all. To make a fair comparison, one would need to know New Zealand’s income per person versus Australia’s in 1980, *before* the reforms. Quiggin does not present this number. It turns out that New Zealand’s income per capita in 1980 was 77 percent of Australia’s and fell to 70 percent in 2000. This is a decline, and so it mildly supports Quiggin’s claim — but because he fails to give this

baseline, many readers will probably come away with the mistaken thought that per capita income in New Zealand took a huge drop relative to Australia’s.

Also, disappointingly, given Quiggin’s belief in basic Keynesianism, he never mentions what 1960s Keynesians saw as the problem leading to high unemployment: wage rigidity. The ultimate in a rigid wage is the minimum wage. Actually, that is an understatement because the U.S. minimum wage is much *higher* than it was two years ago, and reducing it by 20 percent would probably cut the unemployment rate by a few tenths of a percentage point. Keynesians like Paul Samuelson, James Tobin, and Franco Modigliani — all Nobel Prize winners — were harsh critics of the minimum wage, but Quiggin says nothing about it.

Great Moderation | He scores some points against the idea of the “Great Moderation.” This idea became common among macroeconomists in the late 1990s. The idea is that in the late 1980s, the United States — and, possibly, other advanced economies — entered an era in which inflation would be low and recessions would be mild, short, and infrequent, making the growth of real GDP less volatile. Of course, the current U.S. recession, which lasted quite a long time and from which recovery is slow, is the big counterexample to this idea of a great moderation.

Quiggin presents evidence against the Great Moderation. While he agrees that the standard deviation of output growth, a measure of volatility, fell between the 1960s and the 1990s, it did not fall relative to the average growth rate because growth was higher in the 1960s.

The big question, though, is why the United States experienced such a deep recent recession and is having such a slow recovery. My own candidates for the cause of both are: the Troubled Asset Relief Program (TARP), which put government in the position of choosing winners and losers and made property rights less certain; the extension of unemployment benefits in many states to 99 weeks, which reduced the incentive to find work; the misnamed “stimulus bill,” which diverted many

resources to wasteful uses; the Obama administration's shutting down offshore oil drilling and then allowing it only to those companies that satisfy the administration's new stringent requirements; and passage of Obamacare, which is creating uncertainty for employers. At no point in his book does Quiggin seriously consider any of these as potential causes of the deep recession and the slow recovery.

Most people who used the term "Great Moderation" thought that monetary policy had been tamed. Maybe it was tamed, but the other factors I mentioned above are the main causes of our current troubles. The bottom line is that it is too soon to say whether the Great Moderation, assuming it ever existed, is over.

Efficient markets | Quiggin is unpersuasive on his three other candidates for "zombie" ideas: the Efficient Market Hypothesis (EMH), supply-side economics (which he calls "trickle-down economics"), and privatization.

The Efficient Market Hypothesis (EMH) has various versions. The "semi-strong" version is the idea that the prices of financial assets reflect all publicly available information. If this version is right, then an investor can do better than the market only by luck or by having information that is not publicly available. Quiggin rejects this version, arguing that if it were correct, there would never be asset price bubbles — that is, prices of assets that are well above the prices that would reflect the earnings of those assets. Because Quiggin believes that such bubbles exist, this, for him, is enough evidence to reject the EMH.

But if you never know there is a bubble until after it has burst, is the EMH incorrect, or is it just that we do not have enough information? Many people have claimed, *after* the prices of assets have dropped, that the markets in those assets were bubbles. Hindsight is 20/20. But it is much less common to correctly call a price too high in advance. One economist who did claim that stock prices reflected "irrational exuberance" was Yale University's Robert Shiller. He sounds spot on, right? Not exactly. The reason is that he made this claim in December 1996. That

month, the Dow Jones Industrial Average ranged from a low of 6,268 to a high of 6,560. Today, of course, the Dow averages over 11,000. In other words, it rose by a compounded annual average of 4.2 percent; that does not include earnings from those shares, which would make the returns even higher.

Moreover, even if there are bubbles, what policies follow from that? Quiggin claims that if stock prices do not reflect all publicly available information, the "appropriate response is [for the government] to intervene in finance markets to restrict the unsound lending practices that drive the growth of such imbalances." He and I agree that there were unsound lending practices. But then would it not make more sense to end government's role in sponsoring those practices? Quiggin mentions Fannie Mae and Freddie Mac, but he does not suggest that these previously government-sponsored enterprises, which are now outright government enterprises, stop fostering such practices. Quiggin also seems to see his case against EMH as a justification for returning to Keynesian policies — but he never makes that case.

Supply-side economics | Perhaps Quiggin's biggest disdain is reserved for what he calls "trickle-down economics." He admits that his term is pejorative and is used mainly by critics. What, according to Quiggin, is trickle-down economics? He quickly narrows it down to supply-side economics and one of its most important contributions, the Laffer curve, which shows that at both a 100-percent and a zero-percent tax rate, government revenue collections are zero. The reason is that the higher the marginal tax rate, the stronger is the disincentive to work. At a 100 percent tax rate, there is no incentive to work.

Quiggin seems conflicted about the Laffer insight. On the one hand, he claims that the Laffer curve threw the supply-side school "into disrepute." On the other hand, he claims that the Laffer curve "seemed too obvious to bother spelling out." So how could something that is obviously true throw a school of thought into disrepute?

When he gets to his analysis, Quiggin's objection is not to the Laffer curve per se,

but to what he calls the "Laffer hypothesis," the idea that a cut in marginal tax rates will increase government revenue. Quiggin points out, correctly, that some advocates of tax cuts have claimed this and that the case is hard to make. Although he references economist Lawrence Lindsey's 1987 article in the *Journal of Public Economics*, he does not mention Lindsey's main empirical finding: that the drop in the top marginal income tax rate from 70 percent to 50 percent in the early 1980s raised more revenue from high-income people.

Quiggin claims that flatter tax schedules have contributed to the large degree of income inequality in the United States. But this has to be incorrect. It could be true of after-tax income, but the standard measures of inequality are of before-tax income. To the extent that cuts in the top marginal tax rates of the highest-income people cause any increase in their labor supply — and he must think they do, or else he would not say that the Laffer curve is "obvious" — this added labor supply will cause their before-tax incomes to fall, reducing income inequality.

But let us consider the issue of income inequality and his arguments on their own merits, aside from the discussion of "trickle-down." Quiggin claims that "many high income earners pay a smaller proportion of their income in tax than the population as a whole." He gives no source for this claim and no data to back it up. He could be right, if by "many" he means "a few thousand" out of the millions of high-income earners. But the standard sources on this issue find that the higher-income the person, the higher a percent of income he pays in total federal taxes, not just income taxes. A 2007 Congressional Budget Office study of all federal taxes in 2005, for example, found that the average federal tax rate for households in the lowest quintile of income was 4.3 percent, for households in the top quintile was 20.5 percent, and for households in the top one percent was 31.2 percent. It is true that state taxes tend to be regressive, but the states' portion of overall taxation is low enough that they would not reverse the federal results.

Few people care about income inequality if everyone is doing better over time.

But Quiggin argues that “real incomes for the lower half of the distribution have stagnated.” To his credit, he does point out that his data understate income growth for two reasons. First, household size has decreased, and so the households that appear to have the same income as earlier actually have more income per person. Second, he points out that until the 1990s, which was halfway through the period of apparent stagnation he discusses, the consumer price index used to compare real incomes over time overstated declines in real wages and real incomes. Quiggin does not point out that the people being compared over time are different people. Where this really matters is for immigrants. Newly arrived immigrants are disproportionately in the lowest income category and, by their standards, they are doing well or else they would not have immigrated to the United States. So the data over time can show stagnation even if everyone is doing better.

Quiggin does address the issue of upward mobility. He points out that 42 percent of American men with fathers in the bottom quintile remain there. This seems like a high number to him, although to me it indicates substantial mobility — 58 percent have moved to a higher quintile. It also seems strange, in this country where women are approximately half of the labor force, to focus only on men’s income.

Quiggin shows that this 42 percent is above the number in the Scandinavian countries. But it is unclear what to make of this. Could one reason be that incomes are more compressed in Scandinavia and that, therefore, moving from the bottom quintile to a higher quintile is easier there because there is less distance to cover? Quiggin does not mention that possibility in his book, although, in an EconTalk podcast with economist Russ Roberts, he admitted as much.

Privatization | Finally, Quiggin turns his rhetorical guns on the idea of privatization. He points to enterprises that were originally run by the British government, privatized, and then re-nationalized. He argues that the re-nationalization happened because the privatization had been a failure, but he does not give enough

data to persuade the reader that various privatizations failed.

Quiggin does not oppose privatization per se but argues that it makes sense when the enterprise in government hands makes losses. This was the rationale for Margaret Thatcher’s privatization of council housing, the British term for government-provided housing.

Pick up almost any U.S. economics textbook from the 1960s and early 1970s and you will find the U.S. economy described as a “mixed economy,” a mixture of capitalism and socialism. But sometime in the 1980s, the language switched, and textbook authors started referring to the U.S. economy as a “market economy.” One refreshing aspect of Quiggin’s book is that he restores the term “mixed economy” to

its rightful place.

It is disappointing that there is one zombie idea that Quiggin does not bury and still buys into: the idea that a government with a lot of coercive power over people’s lives can be trusted to use that power for good ends. In saying this, I am not making the point that many public choice economists make: that governments tend to serve special interests. Indeed, Quiggin’s book references his own 1987 critique of public choice, a critique that I find persuasive. I am making a more basic point: I believe that governments with significant power over people — whether or not the governments are controlled by special interests — are dangerous.

Quiggin has much more faith in government’s benevolence than I do. His zombie view is still stalking the land. R

(Mostly) Realism on Global Warming

REVIEWED BY JONATHAN H. ADLER

The Climate Fix: What Scientists and Politicians Won’t Tell You about Global Warming

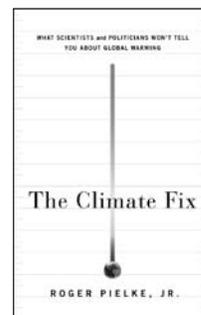
By Roger Pielke Jr.

304 pages; Basic Books, 2010

Last December, delegates from nearly 200 countries jetted to Cancun, Mexico in search of a binding agreement limiting greenhouse gas emissions. As with prior climate meetings, nothing meaningful emerged. Delegates piously pronounced the need to halt global warming, but few were willing to assume the costs. Commitments were made, but only of the non-binding sort. And so, some months hence, diplomats will reconvene at another destination and try again.

This has been the pattern since adoption of the Kyoto Protocol in 1997. Year after year, global leaders pledge to con-

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trol emissions, but their words are not matched with deeds. Even in Europe, where sanctimonious critiques of U.S. climate policy (or the lack thereof) are cheap sport, there has been no real progress toward Kyoto’s emission reduction goals. Indeed, the European Union’s rate of decarbonization post-Kyoto has not been meaningfully greater than in the years before the protocol was adopted.

The persistence of climate talk without action supports what Roger Pielke Jr. calls the “Iron Law of Climate Policy”: “when policies focused on economic growth confront policies focused on emission reduction, it is economic growth that will win out every time.” What this means, Pielke argues in *The Climate Fix*, is that there will be little progress at reducing atmospheric concentrations of greenhouse gases until

it becomes relatively cheap and easy. The United States will not enact costly greenhouse gas emission controls — that is to be sure — but neither will Europe; nor will rapidly industrializing nations such as India and China, let alone the rest of the developing world.

The failure of policymakers and pundits to confront the Iron Law of Climate Policy frustrates the search for effective climate stabilization strategies. *The Climate Fix* is Pielke's effort to reorient the climate policy debate so it may lead in a more positive direction. He believes there is a need to control greenhouse gas emissions, but challenges most other aspects of the conventional climate wisdom. In his view, "accelerating decarbonization of the global economy and improving adaptation to climate change make good sense quite independent of long-term projections of future climate." But climate policy should also proceed with an appreciation of the possible and a concern for human dignity.

Politicized science | For Pielke, too much of the debate focuses on climate science. Partisans spend too much time debating whether computer models provide sufficiently accurate forecasts of future climate change and what specific effects such changes could bring. While there remain differences of opinion about the climate system's sensitivity to greenhouse forcing and the probable consequences of increased atmospheric concentrations of greenhouse gases, "adversaries on either side of [the] debate have agreed about core aspects of the science" for years. Yet scientists and policymakers alike pretend as if resolving the remaining scientific uncertainties will generate a policy consensus. The residual degree of uncertainty about the magnitude and effects of human contributions to climate change is substantial, and is unlikely to go away. Such uncertainty does not justify "inaction" nor does it "compel action." Rather, it should "force us to clarify that which we value and to make choices accordingly." As he argued in his earlier book *The Honest Broker*, "science can alert us to a potential problem and provide some insight about the consequences of different policy choices, but

science cannot decide what choices we ultimately make."

Though sometimes labeled a climate "skeptic" (or, worse, a "denier"), Pielke insists he has "never questioned the climatic importance of human emissions of carbon dioxide" and believes there is a "need to dramatically accelerate the decarbonization of the global economy." But he also believes climate science has become a "fully politicized enterprise, desperately in need of reform if integrity is to be restored and sustained." In his view, scientists share much of the blame for the politicized nature of the climate debate and he strongly objects to the way some of his own research has been used (and misrepresented) for political ends, including by the United Nations Intergovernmental Panel on Climate Change.

While environmental advocates devote their efforts to convincing the public the world is on the brink of a climate catastrophe, policy measures fail to move forward. This effort to use fear to build support for desired climate or other environmental policies is misguided. The fear-based strategy "collapses under its own weight, because science does not compel action, and arguments that cannot be well supported by science will be found out." As Pielke sees things, the problem is not that the public lacks concern about climate change, but that policy proponents have yet to develop policies that are consonant with public opinion.

International agreements set a goal of avoiding "dangerous interference" in the global climate system, but what is this supposed to mean? How much interference is too much? And what degree of climate change, with all its potential consequent effects, is tolerable? Science cannot answer these questions. As Pielke explains, "the degree to which climate is 'dangerous' differs around the world and further depends upon how different communities value security and risk." There is no consensus upon how much interference in the climate system is "dangerous" or on what sacrifices should be made to prevent it. Worse, he suggests, the very notion of "dangerous interference" dresses up a policy question as a scientific matter and compels "science

to serve as the arbiter of what ultimately are political considerations that science cannot resolve." Framing the climate policy challenge in this way pressures science to define what is or is not acceptable and devalues those policy responses, such as adaptation, that might reduce the consequences of climate change, human-caused or otherwise.

Energy needs | The debate over whether governments should aim to stabilize atmospheric concentrations of greenhouse gases at the equivalent of 550 parts per million of carbon dioxide, 450 ppm, or even 350 ppm misses the point. The sobering reality is that "stabilizing concentrations at any level means transforming the global energy system." At the same time, we live in a world in which demand for energy — electricity in particular — is on the rise. India and China are rapidly industrializing, while other nations are only just setting out on the path to economic development. Despite tremendous economic progress, approximately 1.5 billion people still lacked access to electricity in 2008. For many of those people it is difficult to argue that climate change is a more pressing threat than their poverty and deprivation. As Pielke explains, "even if the most dire predictions about global warming come true, some of the poorest people in the world may still be better off tomorrow if they are able to enjoy some of the fruits of development, such as education, health care, and electricity."

In those nations where access to electricity is limited, there is little support for carbon-control measures that could increase the cost of expanding access to electricity. "An approach focused on expanding access to energy while also diversifying supply would almost certainly be better received by developing countries than one that implicitly or explicitly questions their desire for continued economic growth," Pielke writes. Such policies could help accelerate the rate of "decarbonization," but they are unlikely to be enough. All else equal, alleviating poverty in developing nations will result in higher greenhouse gas emissions.

Whereas most environmentalists focus

on limiting the use of carbon-based fuels, Pielke approaches climate policy from the opposite direction. The goal is not to reduce the use of carbon-based energy so much as it is to expand greatly the use of low- or zero-carbon energy sources. “The world needs more energy. Much more,” he explains. Given the urgent need for growth and development, “we can stabilize concentrations of carbon dioxide at a low level only via a massive expansion of the availability of carbon-free energy supply.” He further rejects the ideas that “we use too much energy” or that “fossil fuels are too cheap.” Rather, he wants to increase economic growth and low-carbon energy at the same time:

A focus on expanding the energy supply thus unites two virtues — addressing fundamental human needs while providing inescapable motivation for accelerating decarbonization of the global economy — and eliminates the pathological trade-off between development and emissions reductions implicit (and at times explicit) in conventional climate policies.

Pielke demolishes Pollyannaish claims that the United States and other nations can dramatically reduce greenhouse gas emissions at modest economic costs: “assertions that we have all the technology we need (or soon will) simply do not square with the numbers in any practical sense.” While there was significant decarbonization in the later part of the 20th century, largely due to increased energy efficiency, the world has been recarbonizing in the beginning of the 21st; he notes, “even as the world attention has been focused on climate policy, the global economy became ironically and frustratingly more carbon intensive for every additional dollar of economic activity.”

The world achieved a 1.5 rate of decarbonization from 1980 to 2006, while experiencing 3.5 percent average rate of economic growth. To reduce emissions 50 percent below 1990 levels by 2050 would require nearly three times that rate of decarbonization, even if average economic growth were only 3 percent. This has never been done, and there is no self-evident roadmap to accomplish it.

Pielke crunches the numbers to show how difficult reducing greenhouse gas emissions without restricting energy use really is. Replacing all coal use projected through 2020 with natural gas only nets a 16 percent reduction in emissions below 2005 levels. Achieving similar reductions through the expansion of wind and solar power would require multiplying 2008 levels of production more than 20-fold. Reducing global emissions of carbon dioxide 50 percent below the common 1990 baseline with nuclear energy would require increasing the number of reactors more than 10-fold; the equivalent of opening one new reactor per day for the next few decades. For those who doubt Pielke’s calculations, he provides basic data on energy consumption patterns for the world’s largest greenhouse gas emitters, so the doubters can “do the math” themselves.

Magical innovation | If this dilemma is to be solved, technological advance is key. Absent substantial breakthroughs in low-carbon energy technologies or methods for removing carbon dioxide from emission streams or the ambient air, emissions will continue to rise along with GDP.

While Pielke recognizes that technological innovation in the energy sector is essential, he says relatively little about how the requisite level of innovation could be achieved. “Progress on energy innovation will occur technology by technology, via investments in innovation,” he writes. Fair enough, but where are these investments going to come from? Insofar as government puts up the money for energy technology research, how are we to avoid wasteful boondoggles like the Synthetic Fuels Corporation? Without much support, Pielke claims that “innovation is something that governments at national and international levels have often managed well over periods of decades.” He further endorses a “public works model” to spur energy-climate innovation, yet recognizes the need for “technological agnosticism” in government financing “since we do not presently know where advances might lie.” There is a tension here he does not quite resolve. Transportation infrastructure is funded through a public works model, but the transportation sector

is hardly a fount of innovation.

Pielke believes the federal government should spend somewhere between \$30 and \$80 billion on energy-climate innovation, and suggests a modest carbon tax as the primary funding source. He adds that the purpose of this tax is to raise revenue for innovation, not “to change people’s behavior, to restrict economic activity, or to price fossil fuels at a level higher than alternatives.” The tax he proposes would not be that high — but it still seems a bit unrealistic for an author who spends so much time trying to keep climate policy grounded in reality. Just as nations will not adopt climate policy measures that impose substantial economic burdens, so they are unlikely to impose multi-billion-dollar taxes for the purpose of funding new government programs. If the goal is to use federal spending to spur innovation, it would make sense to consider how to leverage existing investments, such as by leveraging the power of federal procurement to induce innovation (as is done by the military) or by converting some of the billions of dollars the U.S. government already spends on research grants into technology inducement prizes.

While controlling carbon dioxide emissions is important, carbon controls alone will not eliminate potentially harmful human influences on the climate system. There are myriad other ways human activities affect the climate, from emissions of other greenhouse gases to changes in land use. This means there is no quick and easy climate policy, no silver bullet. Focusing on carbon dioxide emissions to the exclusion of other factors may help set policy priorities, but it obscures the larger realities of climate change.

With this in mind, Pielke advocates greater consideration of adaptation strategies and even cautious consideration of some geo-engineering. The choice is not either/or, but all of the above: “Adaptation and mitigation are not trade-offs but complements that address different issues on very different timescales of costs and benefits.” He adopts an unnecessarily broad definition of geo-engineering to encompass everything from direct interventions to counteract the warming effect

of greenhouse gas emissions, to what he calls “carbon remediation” technologies that would remove carbon dioxide directly from the atmosphere. He believes the former is fraught with peril because of the potential for unintended consequences, while the latter is more promising, but labeling them both as “geo-engineering” obscures the difference.

Pielke has high hopes for carbon remediation. “If accumulating carbon dioxide in the atmosphere presents a problem requiring action, then we should just take it out of the atmosphere and store it somewhere safe and permanent,” he reasons. Air capture of carbon dioxide is still too expensive for widespread deployment, but it “compares favorably with the cost estimates” for emission reductions proposed by others. It

is no silver bullet, but is definitely a weapon anyone truly concerned about the climate should want in his arsenal. Yet carbon remediation’s directness may also be its political liability, as it “offers very little benefit to those who see accumulating carbon dioxide as a symptom of a deeper problem or even as a vehicle to address related concerns, such as overconsumption or population growth.”

Conclusion | *The Climate Fix* is most effective when making the case that governments will not sacrifice economic growth for carbon emission reductions, and that atmospheric stabilization of greenhouse gas emissions will not happen without dramatic technological advance. Pielke is more successful in puncturing over-

inflated ideas and identifying critical challenges than setting forth a positive agenda for climate policy. As he himself admits, he is only providing a “rough outline.”

Nonetheless, his clear-headed and non-ideological analysis is welcome in a field dominated by wild-eyed partisans and fear-mongers of various stripes. If one accepts climate change as a real threat, it is essential to acknowledge the lack of clean and easy answers. However urgent global warming may seem, policies to address it cannot be pursued to the exclusion of other concerns, including economic development and access to affordable energy sources. Understanding the depth of the challenge is not only a good place to start, it is essential for there to be any hope of success. **R**

IN REVIEW | WORKING PAPERS

BY PETER VAN DOREN

Housing

- “The Subprime Crisis: How Much Did Lender Regulation Matter?” by Robert B. Avery and Kenneth P. Brevoort. August 2010. SSRN #1726192.
- “Complex Mortgages,” by Gene Amromin, Jennifer Huang, Clemens Sialm, and Edward Zhong. November 2010. SSRN #1714605.
- “Endogenous Gentrification and Housing-Price Dynamics,” by Veronica Guerrieri, Daniel Hartley, and Erik Hurst. July 2010. SSRN #1657176.
- “What Are the Social Benefits of Homeownership? Experimental Evidence for Low-Income Households,” by Gary V. Engelhardt, Michael D. Eriksen, William G. Gale, and Gregory B. Mills. February 2011. SSRN #1752381.
- “The Inefficiency of Refinancing: Why Prepayment Penalties Are Good for Risky Borrowers,” by Christopher J. Mayer, Tomasz Piskorski, and Alexei Tchisty. December 2010. NBER #16586.

Ever since the collapse of the housing bubble and the start of the ensuing recession, economic researchers have been at work trying to determine what caused the bubble, why it collapsed, and how the chaos in real estate was transmitted to the broader financial market and economy. This review looks at some recent papers in that literature.

What is to blame? | Why did the availability of credit for hous-

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ing increase in the 2000s and what effect did that increase have on housing price appreciation and subsequent decline? Many market-oriented analysts argue that the federal Community Reinvestment Act and the affordable housing investment goals for the government-sponsored enterprises Fannie Mae and Freddie Mac recklessly increased the availability of credit for housing. In a recent paper, Robert Avery and Kenneth Brevoort, economists with the Federal Reserve, test whether the regulations that encouraged more lending to lower-income people and poor areas led to worse loan performance. If the regulations induced lending that would not otherwise have taken place, presumably loan defaults would be higher in areas with more CRA-regulated institutions or more people that fulfill GSE lending goals.

Their research design exploits differences in the geographic density of institutions governed by CRA regulations (commercial banks and thrifts) relative to those not governed by them (mortgage companies and credit unions). CRA-regulated institutions get credit for loans made to homebuyers living in low- and moderate-income census tracts within the counties of their branch locations. Low- and moderate-income census tracts are defined as having less than 80 percent of the median income of the broader area in which the bank is located. GSE regulations define low- and moderate-income areas as census tracts with less than 90 percent of median income of the area in which the bank is located.

The authors test whether census tracts with greater density of CRA-regulated lending suffered worse loan performance in the current financial crisis. They also examine loan performance in

census tracts that just qualified for CRA (just below 80 percent of median income) or GSE (just below 90 percent of median income) attention relative to loan performance in census tracts just above 80 or 90 percent of the median income. The assumption in such a research design is that nothing of substantive importance varies with the presence or absence of legal qualification for extra loan attention, and thus the comparison of outcomes between such census tracts is a good approximation of an experiment.

Surprisingly, tracts with greater CRA lender density had lower 2008 loan delinquency rates. However, in regressions estimated only on states that experienced very large growth in real estate loans during 2000–2007 (the so-called “sand states” of Arizona, California, Florida, and Nevada), those tracts with greater CRA lender density had higher 2008 loan delinquency — but the delinquency rate was even higher outside the CRA-relevant assessment area. That is, loans issued to people outside the county in which the bank was located, which do not count toward CRA ratings, performed worse than those that did count toward the CRA rating. Loans generally did not do well in the sand states, but the variation within those states is not consistent with CRA compliance pressure (because CRA-compliant areas had better, not worse, performance).

The authors also estimate regressions for loan quality (debt-to-income ratio and no-income-documentation loans) in 2004–06, the peak of the housing boom. They find that loan quality was higher, not lower, in census tracts with greater CRA lender density.

In the discontinuity tests, the authors find no evidence of any threshold effects. Loan quality and performance increase with census tract income, but this is true of all loans and not just those favored by the regulations. There are no discontinuous worse loan results in census tracts that just qualify as CRA- or GSE-compliant relative to tracts that do not.

Another paper, by Gene Amromin et al., also casts doubt on the role of affordable housing mandates in the recent housing bubble. The paper analyzes data on what the authors term “complex mortgages,” which they define as being interest-only, have negative amortization, or have teaser rates. Two hypotheses exist about the use of such products: The first is that they are a rational capital market response for those consumers whose incomes and house values are expected to rise. Such products are also rational in environments where incomes and home prices are volatile because they allow homeownership with a default option if incomes or prices decline. The second hypothesis is simply that unsophisticated consumers were duped by lenders into using these complex products.

The researchers had access to data on 10 million mortgages from nine out of the 10 largest mortgage servicers in the country over the period 2003–2007. In the frequency distribution of

complex mortgages by metropolitan statistical areas, the top 25 percent of cities that used these mortgages experienced the greatest appreciation and depreciation in the recent housing cycle. Complex loans were used by higher-income people (\$141,000 average income, versus \$88,000 for fixed-rate mortgage holders and \$101,000 for adjustable-rate mortgage consumers) with high credit scores (only 7 percent below 620) in nonrecourse-loan states (where foreclosure results only in the loss of the home and not other assets) in MSAs with previously higher home price appreciation and higher population growth. Complex mortgage users were not only higher-income but also had higher ratios of house value to income and they purchased houses 20 percent more expensive at every income level. Put simply, complex mortgage users had the same credit scores as those with fixed-rate mortgages, but purchased much more expensive houses. For example, complex mortgage use exceeded 40 percent in multiple counties in California, Nevada, Colorado, and Florida, but only 5 percent in Albany, NY.

In short, complex mortgages were used by the affluent to stretch their budgets to afford more expensive homes. Complex mortgage use was higher in areas with high population growth and no price decline in the last 10 years, and less in low-income areas.

Why not more housing? | Why did increased flows of credit into housing in the 2000s result in rising home prices rather than more housing supply with little or no price appreciation? A common explanation is that supply was constrained by natural barriers and/or land-use regulation (see “Zoning’s Steep Price,” Fall 2002). But a new paper by Veronica Guerrieri et al., economists at the Federal Reserve Bank of Cleveland, argues that housing prices can increase in a city even though housing supply for the city in the aggregate is completely elastic.

The mechanism that causes this result is gentrification. That is, an increase in the income of some city residents (or a decrease in the price of credit that increases the ability to bid for housing) increases the price of housing nearest the housing occupied by the most affluent city residents. People bid up the price of this housing so as to experience the positive spillovers created by the affluent (better services, shopping, and other neighborhood amenities). The data show that the greatest price appreciation from a positive income shock occurs in the neighborhoods nearest an existing affluent neighborhood, rather than in the most affluent neighborhood itself.

This price appreciation can occur in the context of an overall-elastic city housing supply. The authors find that even after controlling for housing supply elasticities, the elasticity of housing prices to income changes was 0.96 from 2000 to 2006.

These results do not undermine the argument to reduce or eliminate regulatory barriers to housing supply. But housing sup-

Complex mortgage users were not only higher-income but also had higher ratios of house value to income and they purchased houses 20 percent more expensive.

ply deregulation will not eliminate housing price increases during economic booms.

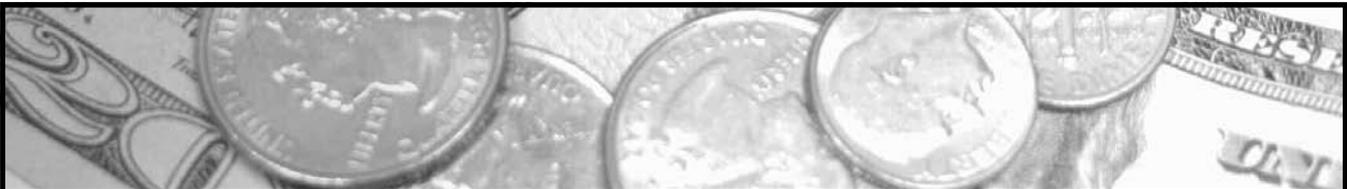
Importance of homeownership? | Subsidies for homeownership are often justified by economists because of the positive externalities that allegedly result: local physical amenities and higher civic involvement. Simple regressions support this hypothesis, but those results are suspect because homeowners differ from renters in ways not observable to the researcher. Thus, homeownership itself may not be causal; it may be simply a surrogate for characteristics of homeowners not specified in the regressions that make them more likely to be civic minded. Researchers have attempted to solve this problem through the use of instrumental variables that are correlated with homeownership but not the unobserved characteristics of homeowners, but scholars are not convinced that these measures do the job.

A recent paper by Gary Engelhardt et al. tackles the issue through random assignment. Low-income renters in Tulsa, OK were recruited and randomly assigned to a treatment or control group. Those in the treatment group were eligible for a savings program in which up to \$750 in annual savings would be matched on a two-for-one basis when used as a down payment for home purchase. The control group was not eligible for the savings subsidy. After four years, homeownership rates were 9.2 percent higher in the treatment group. But neighborhood

volunteering was actually significantly less in the treatment group and other measures of civic involvement showed no difference between the two groups. Those in the treatment group were more likely to spend money on housing maintenance, but only for the interior and not exterior of their homes, and thus there were no positive spillovers. Justifications of homeownership subsidies based on positive externalities would appear to be invalid.

Prepayment | One of the characteristics of nontraditional mortgages that upsets consumer groups is prepayment penalties — an extra charge for paying off the balance of a mortgage before the end of the loan. The Federal Reserve issued regulations during 2008 to restrict prepayment penalties on adjustable-rate loans, and the Dodd-Frank financial reform bill included similar provisions for adjustable- and high-interest-rate loans.

In a new paper, Christopher Mayer from Columbia Business School and two colleagues argue that prepayment penalties prevent adverse selection among the riskiest borrowers. The prepayment penalty binds a pool of borrowers to a lender. Without the prepayment penalty, those who have good ex post outcomes will refinance out of the pool, leaving only the borrowers more likely to default. In the absence of the prepayment penalty, lenders have to charge higher rates that, in turn, make default more likely among those borrowers who experience negative shocks and exclude some borrowers from the market in the first place. R



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