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# Mainstream Discourse on Inequality: A Close Inspection

## Review-Essay of "Economics in the Twenty-First Century" by Robert Chernomas and Ian Hudson

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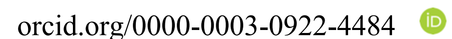
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**Abstract**

This review-essay raises several important issues with the book *Economics in the Twenty-first Century: A Critical Perspective* (2016) by Robert Chernomas and Ian Hudson. I begin by summarizing the book, followed by a critical discussion of its content. The primary focus of this review-essay is on inequality. Using figures from the US Bureau of Labor Statistics, I dispute three popular narratives characteristic of popular discourse on economic inequality. I also discuss problems with Dependency Theory, and the notion of "Comparative Advantage," which is often erroneously attributed to David Ricardo. Finally, I discuss issues with the author's critique of Acemoglu, Johnson and Robinson, and some broader issues with the school of thought known as New Institutional Economics. In general, Chernomas and Hudson present a fair critique of mainstream economics; however, many of their assertions throughout this book deserve critical scrutiny in light of existing empirical data.

**JEL Classification**: B12, B14, J30

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Robert Chernomas and Ian Hudson (2016) criticise mainstream economics in "*Economics in the Twenty-First Century"*, a small book, consisting of only seven chapters. The authors write that mainstream economics can be described as having four key pillars: 1) the modelling of human behaviour on the basis of rational, self-interested maximization; 2) emphasis on the individual as the unit of analysis, in particular individual choice as the subject of inquiry; 3) formal mathematical modelling in which logically consistent conclusions are derived from explicitly defined sets of assumptions; and 4) the use of econometric statistical techniques lacking relevant institutional and historical context for empirical verification of hypotheses. (Chernomas & Hudson, 2016, p. 12) In the book, the authors examine the research of various recipients of the John Bates Clark Medal (JBCM), and argue that mainstream economics' reliance on these four pillars has resulted in a lack of attention to the impact of broader social forces, "macro foundations," on the individual.

The book begins with an introduction, where the authors present their main critique of mainstream economics. They write, *"people do make choices, but a focus on individual choice, rather than the powerful social and economic institutions that shape those choices, pulls a veil over what actually determines economic reality"* (Chernomas & Hudson, 2016, p. 11). Chernomas and Hudson put forward what they describe as Dow-Heilbroner-Milberg critique of mainstream economics, which is based on the four pillars discussed above. They use this critique to examine the research of JBCM recipients. The authors write that mainstream economic analysis systematically neglects any discussion of power. They write: *"power is held and wielded by actors such as corporations, but power is also deeply embedded in the structures, institutions and even ideologies of a society"* (Chernomas & Hudson, 2016, p. 17). Although Chernomas and Hudson present a thoughtful critique of mainstream economics, their critique raises several issues. This review-essay supplements their book by discussing some new research. Special emphasis is given to the third chapter of this book which discusses inequality.

Unfortunately, a big issue with the book is that the authors do a poor job of discussing what constitutes "mainstream" economics vs "non-mainstream" economics. It is heavily implied that they are criticizing the neoclassical paradigm, but this is never explicitly stated, which raises several issues. Certain schools of thought, such as the Austrian school, are critical of "mainstream" neoclassical orthodoxy, but are not necessarily desirable from the perspective of building a more emancipatory social science. Another significant issue with the book is the

authors' presentation of what they call "Classical" Political Economy. Many in the "heterodox" tradition, including Chernomas and Hudson, continue to advocate an odd textual interpretation of the Classics (Adam Smith and David Ricardo), often described as the "surplus approach," or sometimes also described as Physicalism. The approach was developed in the 20th century by followers of Piero Sraffa. Over the years, however, many scholars have pointed out that this interpretation is not very helpful in understanding Classical Political Economy; especially classical value theory. For this reason, I and others would argue that Chernomas and Hudson's interpretation of the classics, departs in very serious respects from the classics themselves. Indeed, there are various other issues with this interpretation. For instance, proponents of the Sraffian "surplus approach" often include Marx as a member of the wider Classical Political Economy school of thought. Arguably, however, this is not accurate because "Critique of Political Economy," the subtitle of Marx's *Capital*, refers to a separate body of thought, rather than a critique of the existing political order. Of course, Marx also presents a critique of Capitalism as well. In this respect, Marx's *Capital* criticizes the whole tradition of economic thought that precedes him, that is, Classical Political Economy. Thus, those who depict Marx as a Classical Political Economist, often fail to distinguish between "modern" reformulations of Marx, based on this "surplus approach," and an accurate reading of Marx.

The authors describe the Post-Keynesian school as "non-mainstream" (2016, p. 6), but it is not immediately clear whether it should be regarded as such. Post-Keynesians generally reject the neoclassical synthesis which inaugurated Keynes into the neoclassical school of thought. But the Post-Keynesian school is still quite diverse. Some theorists like Hyman Minsky reinterpret Keynes, whereas others like Thomas Palley reject the neoclassical synthesis, but do not forward an alternative interpretation. In general, however, Post-Keynesians do a poor job of breaking with *neoclassicism*, i.e., people who want to theorize within the neoclassical paradigm. Many Post-Keynesians reject classical value theory (Smith, Ricardo), based on the method of use-value and exchange-value. Instead, they are *marginalists*, people who evaluate the usefulness of a good or service on the basis of its marginal use. Moreover, their fundamental unit of analysis is the individual, as opposed to class. Post-Keynesians also rely heavily on formalistic modelling techniques in order to study objects of inquiry. And more crucially, not all Post-Keynesians call for the dismantling of the existing socio-political order, i.e., capitalism. Thus, without going into specific details about a specific theorist in the Post-Keynesian tradition, it is hard to say categorically whether Post-

Keynesianism should be regarded as "non-mainstream". Indeed, it is unclear whether Keynes himself should be regarded as a mainstream economist, or a neoclassicist, however for the purposes of this essay we do not explore this question in detail.

In the second chapter, *Development and Growth*, the authors discuss the works of Daron Acemoglu, and James Robinson, authors of "*Why Nations Fail"* (2012), and Esther Duflo, known for her work on Randomized Control Trials (RCT). The authors write: *"one answer to the 'causes of' national living standards that lag behind the rest of the world is external impediments"* (2016, p. 23). They argue that *"the rules of the international economy are stacked against poorer nations, not simply because they are starting at a disadvantage, but because international actors have continuously placed obstacles in their development path"* (2016, p. 23). They discuss the ideas of prominent dependency theorist Raul Prebisch, who stressed an unequal relationship between a developed "core" and developing "periphery." They write: *"the development of the core nations conditioned the development of the periphery, frequently to the detriment of the latter"* (Chernomas and Hudson, 2016, p. 24).

According to this perspective, multinational corporations "siphon profits" and use rules of international trade agreements that disadvantage developing countries. They also write that multinational corporations can influence a country's economic development through political manipulation (Chernomas and Hudson, 2016, p. 26). However, they stress that multinationals can also influence the government in much more subtle ways through things like investment, employment and tax compliance. Chernomas and Hudson argue that international trade and finance institutions also operate to the detriment of developing nations. They give the example of the Washington Consensus standardized policy package, aimed at controlling inflation, eliminating government deficits, liberalizing trade, removing restrictions on capital flows, privatizing state-owned assets, deregulation and ensuring property rights (2016, p. 28). The authors argue that borrowing countries are forced to implement these policies before leading donor agencies like the IMF and USAID will approve loans. They end their discussion by writing: *"any analysis that ignores the external structural impediments to the development of many low-income nations in the current international economic system will be limited to telling, at best, a partial truth"* (Chernomas and Hudson, 2016, p. 30).

A major drawback of Dependency Theory is that it shifts the discussion away from the exploitative relationship between bosses and workers, into one about relationships of

domination and subordination among states. Instead of talking about how the capitalist class exploits workers, Dependency Theory changes the discussion into one about how the rich countries extract resources from poor countries using multinational corporations. The latter perspective neglects discussion of authoritarian social relationships *within the workplace*. Moreover, this perspective neglects any discussion of workers' alienation under capitalism. Hence, there is no discussion of improving workers control, things like being able to choose what to produce, increasing worker participation in the management process, and so on. The notion of "dependency" itself is also quite problematic, since dependency is not one-sided. Arguably, states are in a constant state of *interdependency*. Finally, their critique of free trade is based on a misinterpretation of David Ricardo's famous numerical example. Recent scholarly work on David Ricardo by Jorge Morales Meoqui (Forthcoming, 2021) disputes the popular contraposition of Absolute Advantage vs Comparative Advantage, arguing that both Smith and Ricardo used the same rule for specialization, that is, "*the classical rule for specialization."*

In general, the authors' critique of Duflo is quite good. The first issue they raise is that Duflo speculates about the motives of the poor. They write: *"the solution for Duflo does not appear to be to increase [the resources of the poor] but to teach them to live better within their meager means"*. The second issue, they write, is that RCT methodology *"greatly limits the questions posed by development economists"* (Chernomas and Hudson, 2016, p. 34). Indeed, Duflo (and Banerjee) fail to indict the capitalist mode of production as a whole and demand a *structural transformation* of the existing socio-political order. For a deeper discussion of why RCT methodology is problematic, readers should consult Krauss (2018), who makes a strong case that Randomized Control Trials inevitably produce biased results.

Chernomas and Hudson's discussion and critique of Daron Acemoglu, who often collaborates with Simon Johnson, and James Robinson is also very good. Unlike Duflo, who is more interested in micro-level interventions, the authors write that *"Acemoglu has a broad sweeping explanation for the failure of nations that rests on poor institutions, which are a legacy of colonization"* (Chernomas and Hudson, 2016, p. 37). Nevertheless, the authors' critique of Acemoglu, Johnson and Robinson (AJ&R) is quite limited, as they fail to address several important issues. The authors fail to challenge the underlying deterministic framework of AJ&R's approach to institutional economics. For many humanists, AJ&R's deterministic, or 'path-dependent' approach to institutional economics is quite problematic

because it overlooks the role of human agency in enabling social transformation. An implication of their path-dependent framework is that a country's institutional environment, such as whether or not it enforces "private property rights," *determines* economic performance, or "causes" economic growth. There are several issues with such claims. For starters, AJ&R provide a monocausal explanation for economic growth, where the single explanatory factor is private property rights. On the one hand, we know that that economic growth is a function of a variety of different factors. On the other hand, there is no generally accepted way of measuring or observing "private property rights." Various papers by AJ&R measure "private property rights" by looking at the risk of property expropriation. Arguably, however, this measure is quite narrow. Additionally, their claim that economic growth is causally attributable to private property rights is hard to test empirically, since, we do not observe the counter-factual. In other words, we do not observe what economic growth *would have been*, if a society did not adopt private property rights.

Chernomas and Hudson also fail to discuss problems with AJ&R's empirical framework for identifying the causal effects of institutions on economic growth. In their seminal paper, AJ&R (2001) used Instrumental Variable methodology to estimate the impact of institutions on GDP growth. The big issue, however, is with AJ&R's failure to establish "European settler mortality" as a valid instrument for institutions, the endogenous variable in their regression model. Chernomas and Hudson do not point out the difficulties in establishing valid instruments. As it is known, it is *extremely* difficult to establish whether an instrument is valid, since one would have to demonstrate that the instrument is correlated with the regressor (relevance), but also uncorrelated with the error term (exogeneity). At best, one can make an argument that that European settler mortality is a valid instrument for institutions, however, it is hard to demonstrate categorically, that an instrument is valid. Finally, one of the biggest drawbacks of the second chapter is Chernomas and Hudson's failure to discuss Marx's materialist conception of history. The neoclassical notion of "institutions" views property relations in abstraction from the definite historical conditions of economic development. By contrast, according to Marx's materialist conception of history, elements of the "superstructure" (institutions, culture, property relationships) correspond to the "base", i.e., the dominant mode of production (Feudalism, Capitalism, Communism), which in turn is shaped and maintained by the means of production and the relations of production. In this respect, Marx's materialist conception of history views property relationships as historically contingent, that is, as being specific to and corresponding to a given sub-period of human

history. By contrast, AJ&R's deterministic approach flips Historical Materialism on its head, arguing instead that elements of the superstructure (culture, institutions, property relationships) determine or "cause" economic growth.

The third chapter *"Labor, Income, and Inequality in the United States"* is also quite problematic. The authors present three problematic narratives that are characteristic of contemporary left discourse on inequality: 1) The Compensation-Productivity narrative; 2) The Household Indebtedness narrative; and 3) The Financialization narrative. According to the authors, the fundamental reason why inequality increased during the so-called neoliberal period (generally taken to be from 1979 to early 2000s), was due to insufficient aggregate demand. In particular, the authors identify contractionary monetary policies during the 1980s and 1990s which decreased consumption and investment demand (Chernomas and Hudson, 2016, p. 52). They argue that the rate of profit fell after WWII but recovered during the so-called neoliberal period. It is further alleged that these improvements in profitability were due to an upward trend in the degree of exploitation, which is said to have occurred through reductions in workers' compensation. Chernomas and Hudson do not explicitly state that the rate of profit rebounded due to an increase in the "rate of exploitation," however, they do argue that *"the restoration of business profitability came at the expense of the labor force."* (2016, p. 53).

Starting with the *Compensation-Productivity* narrative, the authors claim compensation and productivity grew in tandem between the post-WWII period and 1973, however, from 1973 and onwards average hourly compensation of employees diverged dramatically from productivity growth. They claim: *"inequality, which had decreased after the Great Depression of the 1930s, increased dramatically after the 1980s [emphasis added]"* (Chernomas and Hudson, 2016, p. 54). This divergence between compensation and productivity is supposedly captured in Figure 3.1 of Chapter 3. The authors then present the *Household Indebtedness* narrative, which is supposedly depicted in the graph in Figure 3.2, taken from Baragar and Chernomas (2012, p. 328). They argue that by the mid-1990s households became net debtors and corporations became net lenders. Their rationale behind this claim is that in order to sustain previous levels of expenditures, households increasingly had to borrow funds in order to finance consumption expenditure. For this reason, the authors claim, household debt and the ratio of total household credit to disposable income increased between the 1990s and 2008 (Chernomas and Hudson, 2016, p. 55). Lastly, the authors

present the *financialization* narrative. The authors claim that the rebound in the rate of profit during the neoliberal period was not accompanied by a corresponding increase in the rate of capital accumulation due to increased "financialization" of the economy. The reason for this, they claim, is because funds were diverted from production to financial markets. This supposed diversion, it is argued, was the main cause of the fall in the rate of capital accumulation. For instance, Chernomas and Hudson claim: *"the financial sector grew in size and became much more profitable"* (2016, p. 56).

All three narratives mentioned above deserve critical scrutiny in light of some recent empirical research. With respect to the first narrative, *Compensation-Productivity*, the fundamental issue is that Chernomas and Hudson do not specify which benefits they take into account for their measures of "compensation." For Figure 3.1, they write that real hourly compensation includes "the value of benefits as well as wages," but they do not indicate which benefits they include. Do they take into account Social Security payments (retirement benefits, disability, children's benefits, etc.)? Health-insurance also accounts for a significant portion of non-wage compensation, and depending on the circumstances, the employer might also provide the employee with things like dental, vision, gym memberships, car allowances and other benefits. Of course, the taxes levied on wage and salary income, for the purpose of funding social insurance programs (Social Security, Medicare) also counts as income for the employee, as per the Congressional Budget Office (2012, July).[1] In addition to Social Security and Medicare, there are other government social insurance programs such as state (as opposed to federal) pension plans for state employees, a similar program for railroad employees, and the unemployment insurance system, which gives (eligible) unemployed workers income using funds acquired by a tax on employers. Thus, if we take into account all of these forms of income, arguably the gap between "compensation" and "productivity" narrows significantly (see Kliman, 2015).

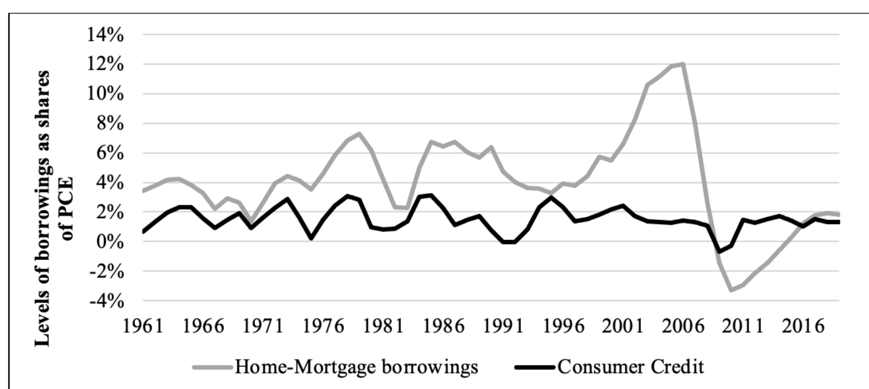
A popular presentation of the Compensation-Productivity narrative deserves special attention. The Economics Policy Institute (EPI) regularly publishes articles which claim that the gap between "pay" and "productivity" has diverged dramatically since 1979 (see Economic

[1] The Congressional Budget Office considers the amount of Social Security tax that the employer pays (on the employee's behalf) as compensation (and thus pre-tax income). They write: *"CBO further assumed—as do most economists—that employers pass on their share of payroll taxes to employees by paying lower wages than they would otherwise pay. Therefore, CBO included the employer's share of payroll taxes in households' before-tax income and in households' taxes* [Emphasis Added]*"* (2012, July, p. 20).

Policy Institute, 2019). The main problem with EPI's presentation of this narrative is that they use two different price deflators, the Consumer Price Index (CPI) and Net Domestic Product (NDP) deflators (see Bivens and Mishel, 2015). They use the CPI to deflate wages but the NDP (as opposed to Gross Domestic Product) to deflate productivity growth. This is highly problematic because the prices of consumer goods have risen more rapidly than prices of output as a whole (see Lawrence, 2018). Indeed, the two price indices have diverged dramatically since the 1970s, the very period where the EPI begins its analysis. Reasonable people can disagree as to which price index is superior for this type of analysis, however, the simultaneous use of two different price indices provides grossly misleading results, as it compares apples with oranges. Finally, the EPI only looks at hourly earnings for "production and nonsupervisory workers," as opposed to the whole population of the US labour force (see Bivens, Mishel, and Sheirholz, 2014).

Regarding the second narrative, *Household Indebtedness*, again it is not immediately clear from Figure 3.2 how the authors constructed their measures of household indebtedness. The authors write that they use Table S2, Selected Aggregates for Total Economy and Sectors, from the Integrated Macroeconomic Accounts for the United States. However, they do not specify what lines they use from this table. There are two net lending series, lines 38 and 47, and based on visual inspection alone, it is hard to identify which series they used. Moreover, there is no category for "corporations" in Table S2. At this point it is worthy to display the levels of home-mortgage borrowing and consumer credit as shares of Personal Consumption Expenditure (PCE).

*Figure 1: Home-Mortgage Borrowings and consumer loans as shares of Personal Consumption Expenditure*



Source and Note: Data retrieved from lines 43 (One-to-four-family residential mortgages) and 44 (Consumer credit), of Table F.101 of the Federal Reserve's Financial Accounts of the United States. Figures for PCE were retrieved from line 2 of National Income and Product Accounts (NIPA) given in Table 1.1.5. Data were retrieved from the online FRED database. Andrew Kliman (2014, May 16) initially developed this graph in his response piece to Sam Gindin called: "Clarifying 'Secular Stagnation' and the Great Recession". For this paper's purposes, the graph is updated by the author (1961-2019), with the exclusion of the figures for home equity extractions.

From the figure above, we can see that there is no evidence to support the claim that household borrowings and consumer credit have trended upwards during the neoliberal period. Mortgage borrowings did rise sharply starting in the mid 1990s, however, as we know now, this was due to the housing bubble which burst during the Great Recession. There is no reason to believe that these increases in household borrowings were characteristic to the neoliberal period as such. Furthermore, consumer credit as a share of PCE has been relatively stationary, oscillating around 2%. Hence, there is also no reason to believe that workers have become increasingly dependent on credit either.

With respect to the *financialization* narrative, the authors are quite right that the rate of capital accumulation has decreased, however their claim that this is due to the supposed "financialization" of the economy is not backed up by evidence. Kliman and Williams (2014) argue that *"productive investment did fall as a share of profit after the early 1980s, but not because profits were diverted away from production under neoliberalism"*. Rather, they argue, *"the investment share of profit fell because it was unusually and unsustainably high at the start of the 1980s, and the fall brought it back to normal levels through 2001 [emphasis added]"* (Kliman and Williams, 2014, pp. 2-3). Thus, *"none of the sharp fall in the rate of accumulation that took place between 1979 and 2001 was due to financialisation or neoliberalism"* and *"more than half of the fall is attributable to the decline in the rate of profit, whilst the remainder is attributable to the fact that the investment share of profit returned to normal levels"* (Kliman and Williams, 2014, p. 3). They further point out, convincingly, that *"official US government data (from the National Income and Product Accounts and the Financial Accounts of the United States) indicate that there has been no diversion of profit from production to financial markets under neoliberalism. On the contrary, the data indicates that the share of profit that was productively invested was slightly higher during the first two decades of neoliberalism in the USA than during the prior*

*three decades [emphasis added]"* (Kliman and Williams, 2014, p. 2). Thus, there is no reason to accept the financialization narrative either.

Another problematic claim the authors put forward is that *"workers are receiving an increasingly small share of the value that is being produced in the economy"*, implying somehow that an increasingly larger share of labour earnings has been ***wrested away*** from workers by capitalists (Chernomas and Hudson, 2016, p. 54). The famous counter position 99% vs 1% comes to mind, where it is claimed that the 1% have started to enrich themselves *off the backs* of the 99%. In other words, neoliberal policies have somehow redistributed or diverted funds from the bottom to the top. The evidence that workers' share of the value being produced has fallen is dubious. Kliman (2015) convincingly argues that the compensation share of net output did not decline between 1970 and the Great Recession. Moreover, there is no reason for accepting the claim that labour earnings are somehow being "snatched away" from the poor by wealthy elites. The problem is not that the poor are becoming poorer, nor that the amount of wealth at the bottom is declining. Rather the problem is that the rich are becoming richer ***at a faster rate than the poor***. Not only are such claims unfounded, but they also point the blame at political opponents. By starting their analysis in the 1980s, the authors fail to examine the impact of pre-Reagan Keynesian policies on inequality, implying somehow that positive trends in inequality can be attenuated if the "right" kinds of policy choices are made by the "right" kinds of politicians. Indeed, there is evidence to suggest that the trend in inequality was continuously increasing even ***prior*** to the neoliberal period. As Kliman (2011, p. 50) writes: *"the characterization of a period of capitalist development as 'neoliberal' is therefore not grounded in facts as much as in a strong tendency toward political determinism—the notion that the economic laws of capitalism can be fundamentally modified by political will and power"*.

The authors also examine Acemoglu's claim that income inequality is attributable to the decline in unions and skill-biased technical change. In general, Chernomas and Hudson do a good job of subjecting this claim to scrutiny. However, one thing they barely talk about is Acemoglu, Johnson, and Robinson's claim that inequality is causally attributable to whether or not a country adopts democratic political institutions. Acemoglu and Robinson (2001, 2006), as well as Acemoglu et al. (2008) spend a fair bit of time developing an axiomatic causal mechanism between inequality and democratic political institutions. However, the value of such causal mechanisms is dubious, as democracy is an incredibly complex social

phenomenon which can hardly be captured through empirical datasets. At best, researchers can provide *correlational* evidence, whereas Acemoglu, Johnson and Robinson infer *causal relationships*, i.e., that democratisation "causes" levels of inequality to decrease. Krauss (2015) makes a strong argument that analysing the potential relationship between democratization on levels of inequality is hard to capture econometrically due to fundamental methodological constraints. Furthermore, it is interesting to note that the dataset Acemoglu, Johnson and Robinson use for their analysis, that is, the Freedom House and Polity indices, developed by the US government funded think-tank, Freedom House. Arguably, the name of the dataset alone (Freedom house), as well as its stated objectives, reveal the libertarian and neoconservative bias of the individuals who comprised the dataset. For instance, long-time American ally Israel has consistently been rated "free" and US-friendly Arab states are rated "partly free." For a deeper discussion of the ideological nature of Freedom House's methodology for evaluating the extent to which a country is "democratic," one might consult Giannone (2010), who makes a strong case that these datasets are systematically biased in favour of Western and Christian countries.

In chapter "*Health, Healthcare and the Individual"*, the authors do a good job of discussing some of the reasons why the healthcare sector is particularly prone to market failures. They do a good job of discussing the concept of information asymmetry and issues like moral hazard and adverse selection. In "*Crime"*, the authors examine the interplay between crime and the economy. In many ways, this section of this book is a little bit out of place. The authors examine the works of Steven Levitt and criticize him for placing individual choice at the centre of inquiry and for dismissing the role of broader economic factors (Chernomas and Hudson, 2016, pp. 111-116). However, for this section, readers would have benefitted from a more elaborate discussion of the role structural factors play in enabling crime. Lastly, in the final chapter, "*Two Kinds of Crises"*, the authors discuss the reasons behind economic crises. However, as discussed previously, their claim that increased "financialization" of the economy led to higher levels of household debt, or their premise that the rate of profit trended upward leading up to the Great Recession is not very well established. Moreover, in this section the authors neglect any discussion of alternative ways the economy could have been restructured after the Great Recession. For instance, conditions could have been attached to large firms that were bailed out using taxpayers' money, in order to make them more democratic. Worker representation in the board of directors (codetermination), restructuring firms so that they are employee-owned, improving the administrative structure of the firm so

that workers have more autonomy, the ability to choose who your boss is (within certain limitations), having the ability to decide what to produce, are all conditions that could have been attached to firms that were bailed out.

Due to the objections raised in this review-essay, the 2016 book "*Economics in the Twenty-First Century"* by Chernomas and Hudson should be read carefully, with a grain of salt. The authors present a good critique of the mainstream, however many of their assertions are not backed up by empirical data. Proponents of the Compensation-Productivity narrative use a narrow measure of "income" that fails to take into account non-wage benefits. By including things like employer-provided health insurance, or social benefits like food stamps or unemployment benefit, the gap between "income" and productivity narrows significantly. Further, the EPI uses two different price indices when comparing "income" and productivity. This gives us misleading results because one index (CPI) has diverged rapidly from the other (NDP) since 1973. Regarding the Household Indebtedness narrative, BLS data does not indicate that household borrowings have risen, nor that consumer credit has trended upwards. Moreover, the claim that the rate of profit rebounded, but that it was unaccompanied by a corresponding increase in the rate of rate of capital accumulation due the alleged "financialization" of the economy, is also not backed up by empirical evidence. Additionally, readers should also be aware that the authors' critique of free trade is based on a misreading of David Ricardo. According to new research by Jorge Morales Meoqui, Ricardo was not a proponent of "Comparative Advantage." Rather Morales Meoqui convincingly argues that both Smith and Ricardo used the same rule for specialization, that is, the "*classical rule for specialization"*. Taking all things into consideration, the book provides a lacklustre introduction into contemporary theorizing in non-mainstream schools of thought in economics.

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