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Comments welcome

Economists and the Collapse

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1. Reckoning for “Problems and Prospects”

Comparing predictions with what actually happens is a way to learn more about the world and hone our methodology. Business cycle forecasters regularly study their own errors in predicting the economy’s ups and downs. One economist has spent his whole career studying the error record of business cycle forecasts (McNees, 1975 and 1995), and there is a substantial number of other publications on the subject.¹ Studies comparing the effects of tax changes and minimum wage increases with theoretical predictions have been at the center of recent economics controversies in the US. A study comparing the effects of deregulation in the US with what economists promised beforehand was titled “Days of Reckoning for Microeconomists” (Winston, 1993).

American students of the Soviet economy, long characterized by “... much mutual admiration and a curious absence or near absence of public disagreement ...” (Gerschenkron, 1968 (1964), p. 534), have not engaged in this type of score keeping. Yet no deep analysis was needed to see that the disappearance of the USSR was not considered in the countless publications with “Problems and Prospects” in their title. For a short while before the dust of the collapse settled, the outsiders took the appraisal of Sovietology into their hands, producing outspoken but ill-informed statements.²

This paper analyses the failure of American and British economists studying the Soviet economy (Sovietologists for short) to foresee the evaporation of their very subject.

Forecasting error is the gap between the predicted and the actual event. In the case of business cycle forecasting, “actual” is the number eventually published by the US Department of Commerce. Here, “actual” is the story of how collapse occurred, and there is more than one in

¹ See the references in Graham, 1996, pp. 231-32.

² references?????????

circulation. What one considers as errors of forecasting depends on which explanation of collapse one finds persuasive. Thus, it is commonly believed in the West that the Soviet system collapsed because of its economic weakness, and that the economists are at fault for exaggerating the size of the Soviet economy.³ Of course, the causes of collapse have to be established before errors of prediction can be identified. The fact of collapse does not itself prove that lower estimates of the size of the Soviet economy in the 1950s were correct, and higher ones wrong.⁴

This paper uses the explanation of the collapse elaborated in “Economic Fallacy” elsewhere in this volume.⁵ There it is argued that the USSR had a viable, if sluggish economy. An overconfident ruler tried to spur it faster by reforms, which only made things worse. Political changes, enacted at the same time and for the same reasons, destroyed the system. Slow economic growth and the international pressure of the early 1980s in all probability encouraged the ruler to take additional risks entailed in the reforms.

The Sovietologists may have well overestimated the size of the Soviet economy relative to that of the US. But this did not impair their ability to forecast the collapse, if the above story is correct. Having established (outside of this paper) what the mechanism of collapse was, we need now to determine which of its elements could have been predicted and which ones could not.

"Any prediction is extra-scientific prophecy that attempts to do more than to diagnose observable tendencies and to state what results would be, if these tendencies should work themselves

³ Becker (1994, pp. 291-293) reviews such charges.

⁴ As Malia (1992, pp. 95-96) seems to argue.

⁵ See also “Overview” in Ellman and Kontorovich, 1992; Ellman, 1993; and Ellman and Kontorovich, forthcoming.

out according to their logic. In itself, this does not amount to prognosis or prediction because factors external to the chosen range of observation may intervene to prevent that consummation; because ... observable tendencies, even if allowed to work themselves out, may be compatible with more than one outcome; and because existing tendencies, battling with resistances, may fail to work themselves out and may eventually 'stick' at some halfway house." (Schumpeter, 1942, p. 416).

By this standard, Sovietology cannot be faulted for not predicting the collapse back in, say, 1983. "Factors external" to economists' range of observation - rulers' personalities - were crucial for the timing of the collapse. Were Andropov to live longer, or Gorbachev turn out to be less self-assured, then the Soviet system might still have been with us. The wrecking ball of political reforms is also an "exogenous" factor in standard economic thinking. And without it there was no reason to consider the possibility of collapse even in 1989-1990.

This does not absolve the economists of responsibility for being systematically wrong.⁶ Sovietology did fail to "diagnose observable tendencies" which, in combination with the leader's idiosyncrasies, paved the way for the collapse. This paper focuses on three instances of misdiagnosis: the destructive potential of Gorbachev's economic reforms, the duration of the growth slowdown, and the place occupied by the military sector in the Soviet economy. This is not a complete list of the profession's failings which contributed to its missing the collapse. My choice of topics is dictated by my own research interests and space constraints.⁷

⁶ As Millar (1994, p. ??) suggests.

⁷ A complete treatment should address, for example, the failure to notice growing monetary disequilibrium, which later proved politically destructive for Gorbachev's rule. Other aspects of the Soviet economy that have been misdiagnosed, but did not directly con-

Each subsequent section analyzes Sovietology's treatment of an aspect of collapse by asking the same set of questions:

- i. what actually happened?
- ii. what the "majority" prediction/diagnosis was, and how it compares to i.?
- iii. was correct prediction/diagnosis possible (e. g., did someone at the time produce it)?
- iv. what was the origin of the errors?
- v. how would correct diagnosis have helped in understanding the coming collapse?

2. Economic stagnation

The decline in the rate of growth from the high postwar levels, which started around 1958, was anticipated by the experts (Grossman, 1953, p. 23; Gerschenkron, 1953, p. 25). However, they failed to predict that the rates will continue to decline for over the quarter of the century, and would end up transforming a rapidly growing economy into a stagnant one.

2.1. Forecasting record

Systematic forecasting of the Soviet economy in the West started in the mid-1970s. At that time, the economy was growing much slower than in the 1950s, but still at a healthy clip (see Table 1 in "Economic Fallacy"). Predictions concerned the economy's overall strength as measured by the growth of the GNP, and growth of production in the largest sector, industry.⁸ All the Western fore-

tribute to the failure to predict the collapse, are not dealt with here. This is not a history of Sovietology.

⁸ Some of the forecasters also published their predictions of the net material product (NMP), as measured by the Soviet official statistics. Everything I found for the forecasts of GNP in Tables 1 and 2 also applies to the forecasts of NMP (Kontorovich, 1981).

casts of these two variables in 1976-1990 that I have been able to locate are presented in Tables 1 and 2.

Comparison of the actual and predicted growth rates in Tables 1 and 2 shows that practically all the forecasts overestimated Soviet growth in 1976-85, often by a large margin. The stagnation of the late 1970s and early 1980s came as a surprise to the West.

Most forecasts in our sample originated, directly or indirectly, from the econometric model of the Soviet economy called SOVMOD (Green and Higgins, 1977).⁹ But this does not mean that the forecasting bias was due to the structure of a particular model. Econometric model forecasts are driven by the judgment of the forecasters at least as much as by the formal relationships specified beforehand. Also, the tables include forecasts derived by other methods, and those have an upward bias as large as that of the SOVMOD for the corresponding periods.¹⁰

Sovietologists who ventured a quantitative assessment of the economy's prospects were few in number. However, the rest of the profession did not challenge these numbers at the time. Moreover, large econometric models responsible for most forecasts were generally seen as the

⁹ This concerns Bond and Levine; Green, Guill, Levine, and Miovic; Green; and Hunter, Earle, and Foster. CIA forecasts (including Rowen, 1982, and Kazmer and Whitehouse, 1978 for 1980-85) were produced using a model based on SOVMOD (described in CIA, 1979).

¹⁰ Bergson (1971) and (1978), Rosefielde and Lovell (1977), and Weitzman (1983) were based on a production function, while Nove (1980) was an informal guess.

cutting edge of research. For these reasons, the forecasts in Tables 1 and 2 can be viewed as representative of the Sovietologists' opinion.

2.2. Sources of errors

2.2.1. Ignoring the long-run trend

Though produced by different authors using different methods, forecasts in Tables 1 and 2 all predicted the continuation of the growth rates achieved in the recent past. In 1976, GNP growth rate for 1976-80 was forecast at 4.3-4.7%; the actual rate in 1971-74 was 4.3%. Nove's forecast of 3% growth was published in 1980; growth in 1976-79 was 3.1%.

Large errors in the forecasts produced in the mid-1970s became apparent by the early 1980s, yet the lesson went unheeded. Forecasts of growth to 1985 produced early in the decade again in effect extrapolated the recent growth rates into the future. The result, again, was a sizable overestimation of growth. Forecasts of industrial production similarly ignored the long run trend and ended up as being too high. In Table 2, part B, forecast average growth rates are no more than half a percentage point away from the averages of the previous five years. Forecasters behaved as if they did not believe the long run growth slowdown was going to continue.

Texts accompanying the forecasts in the 1970s do not mention the long run trend. Nor is it mentioned in most of the writings on the prospects that do not make explicit predictions.

The trend that was ignored by the analysts was quite unusual, perhaps unique for macroeconomic data in terms of its durability and strength. Consider the largest sector of the Soviet economy - industry. Its growth rates have been known for their stability relative to other economies (Staller,

1964). Practically the only change visible on the graph of industrial growth rates from 1951-75 was their slow decline.¹¹

Acknowledging the trend could have produced more accurate (lower) forecasts of industrial growth in 1976-85. One would have to assume that the past trend will continue; fit a straight line to the growth rates from 1951-1975; and continue the line into 1976-85 period. The resulting “naive” forecast, as a rule, would have been more accurate than the sophisticated ones (see Table 1). Forecasting errors are known to increase with the time horizon. But the linear trend forecast of the early 1980s made in 1976 is almost as good as econometric model forecasts of the same period made just one year ahead.¹²

Gregory Khanin, the only economist to correctly predict the onset of stagnation in the mid-1980s (in a *samizdat* study circulated in the mid-1970s), focused on the long-run nature of the decline in growth rates.

2.2.2. The mystery of declining productivity

The growth slowdown had been noticed (Bergson and Kuznets, 1963, p. vi), and by the mid-1970s its causes had been studied at some length. Why did the forecasters (often the same people who analyzed the causes of the past slowdown) failed to incorporate this powerful trend into their projections?

¹¹ Straight line fitted to the Soviet official growth rates of industrial production in that period explains 71% of their variation. Straight line fitted to the CIA estimates explains 65%.

¹² A more accurate forecast of industrial production, the single largest component of GNP, would have improved the forecasts of the latter.

Economists analyze output growth by relating it to the growth of factors of production - land, labor and capital - and the change in productivity with which the factors are used. The latter is called total factor productivity (TFP). This relationship, called the production function, also underlies the forecasts of future production. Studies have shown that it was the slower growth of TFP, rather than quantity of factors, that caused GNP growth slowdown through the mid-1970s ().

Yet the forecasters were chiefly concerned with the future growth of factors, in particular with the labor shortage predicted for the late 1970s. In the publications, assumptions about the growth rates of factors and of the TFP underlying the forecasts are not always made explicit. **As far as I could determine**, the forecasts made in the mid-1970s underestimated the actual growth of labor and capital through 1980. The overestimation of output came entirely from an overestimation of TFP.

[What does TFP growth reflect and how it is estimated - to be written.]

Estimates for the early 1970s showed that TFP in the Soviet economy had been declining (Greenslade, 1976, p. 279). With so much advanced technology around the globe waiting to be stolen and used in the USSR, the decline in their total factor productivity appeared strange. One forecaster felt that assuming even zero growth of TFP was “unduly pessimistic” (Bergson, 1978, p. 238). But even this assumption would have led to too high a forecast of growth. And estimated TFP kept declining in the late 1970s and 1980s (see Table 3, row 5). The reason for that was the general error in estimating the rate of growth of capital stock.

2.2.3. The capital stock scandal

One of the main tasks of the Western analysts has been adjusting Soviet data in value terms, such as income and product, to fit economically meaningful concepts, and to control for the effects of price change. The need for the latter emerged because “comparable prices” used in the Soviet statistics were not actually constant over time. The CIA has been adjusting all the major So-

viet indicators, with the exception of investment and capital stock data. These were just being reprinted from the Soviet statistical yearbooks as is. This practice was explained by reference to Cohn (1981) stating that “estimate prices” - the type of comparable prices used in Soviet investment accounting - are in fact constant (CIA, 1982, p. 12). Both academic and government experts used the data as if they had been deflated.

It is difficult to understand how one could hold such a view. Standard Soviet textbooks clearly explained that “estimate prices” were not constant.¹³ Soviet experts worried in public about the effects price inflation had on the measurement of investment. Their writings inspired Nove (1981) to call into question Western use of the official Soviet investment series. A decade-long debate between the British doubters and American believers in the CIA practice followed.¹⁴

Soviet textbooks on economic statistics had none of the fun and games feel of the American college texts, and the writings of the Soviet economists were often inarticulate. But how could the organization equipped to peer deep behind the Iron Curtain miss the same information in plain English published by Cambridge University Press (Dyker, 1983, p. 188)? Finally, the very scholar on whose authority the CIA accepted the soundness of the Soviet investment and capital stock data was on record stating exactly the opposite ten years earlier (Cohn, 1972, pp. 144-5). Cohn never explained the reversal of his position, and no one seems to have noticed.¹⁵

All the Western researchers of Soviet economic growth in the 1970s and 1980s have been using the official fixed capital stock data that incorporated price inflation. The data therefore over-

¹³ See references in Kontorovich, 1989a.

¹⁴ Wiles, 1982 and 1982a; Hanson, 1984 and 1987; Bergson, 1987.

¹⁵ This is symptomatic of the whole field of Sovietology. It seems to have forgotten in the 1970s and 1980s several important things it knew in the 1950s and 1960s.

stated growth of capital stock. TFP, estimated as a residual, grew slower because of this, and turned negative in the 1970s.

If investment and capital stock data were adjusted for inflation, as they should have been, the TFP would have been increasing rather than falling. For example, Khanin's (1991, p. 176) estimates of real capital stock (row 6 in Table 3) yields significantly lower rates of growth than the official series (row 3). If Khanin's deflated capital stock data are used to estimate TFP, the story of the Soviet growth looks different (compare rows 5 and 7, Table 3). Total factor productivity grows substantially in the 1960s and then slows down to a crawl in the 1970s and early 1980s, but it does not decline.

2.2.4. The really tough part

If the TFP bore such large share of the responsibility for the slowdown, then successful forecasting of growth required getting a handle on TFP's future growth. This is a notoriously difficult task compared to forecasting the growth of labor and capital (for which current demographic and investment data provide some clues). Forecasting future changes in the TFP requires an understanding of its past trajectory. Two approaches are possible here: growth accounting and grand theory.

The former means getting a quantitative handle on at least some elements of the TFP by obtaining data on changes in quality of labor and capital and other relevant variables, and their impact on growth. As new sources of growth are introduced, and parts of the growth rate are attributed to them, the unexplained residual shrinks (Stone, 1980). In the process, one gets quantitative measure of the additional determinants of the residual in the past. This provides a firmer footing for predicting the future changes in the residual. Growth accounting even for the US economy requires laborious data collection. This has been urged for the Soviet economy by Bergson (1983, p.), but never conducted on a scale remotely comparable with that of, say, Denison (1974). Growth accounting for

the Soviet economy would have required a tedious search of turgid and obscure Russian-language sources for new information on the determinants of the residual. Instead, the study of the Soviet growth proceeded by fitting the same labor and capital series to the same output series time and again, while varying the statistical technique. The findings generally confirmed the importance of the residual for the slowdown, but were by definition incapable of explaining it. Therefore there was no basis for projecting the residual's future growth.

Even the most ambitious growth accounting project leaves much of the residual undeciphered. Here, "grand theories" about the social, political, and economic forces underlying growth are needed to complement growth accounting. And if such a theory is really good, it may substitute for growth accounting. The predictive record of grand theories for the capitalist economy, such as Olson (1983), has not been very good. But the Soviet system was a much simpler contraption, hence easier to predict.

No such theories can be found in the mainstream Sovietological literature of the 1970s and 1980s. Bergson (1978, pp. 28-9) briefly mentioned that central planners in a hierarchical economy must be making increasingly poorer choices as the size of the economy grows. This extension of the well known organizational idea of "loss of control" was never developed, tested, and seriously applied to judging the future course of the Soviet economy. It seems to suggest that economic growth in the USSR must eventually come to a standstill, but this has never been stated explicitly.

This is all the more regrettable since a correct forecast of the Soviet economy was actually made over a surprisingly long time horizon using a theory of the type discussed here. Barrington Moore, Jr. (1954, p. 71) argued that the sole source of dynamism in the Soviet system was the top ruler's energy and the pressure he exerts on his subordinates. Should this pressure cease, the system will stagnate. In the 1970s, one could hear echoes of Moore's theory in the Soviet complaints about "discipline relaxation" as the cause of all the troubles. In his *samizdat* forecast made in

mid-1970s, Khanin used detailed Moore-type arguments to explain the past slowdown and project the future course of the economy. Yet in the West Moore's prediction appeared to have been forgotten on the eve of it coming true.

One reason why Moore's theory fell into disuse in the 1970s, and the Soviet complaints about discipline relaxation were not picked up, was that they did not fit into the reigning paradigm of the time. This paradigm, originating in the late 1950s but more fully developed in the 1960s and early 1970s, saw various levels of the economic hierarchy bargaining with each other, with superiors offering monetary inducements and inferiors deciding how hard they want to work given the offer.¹⁶ Moore's (and Andropov's) view of the Soviet system did not translate naturally into this language.

Without an extensive growth accounting for the residual or a "grand theory" to explain its movement, the forecasters were helpless in charting the prospects for the 1970s and 1980s.

2.3. *A CIA conspiracy*

The CIA's forecasts were the most accurate among those represented in Table 2.¹⁷ They overestimated Soviet growth in the 1970s by a smaller margin than the rest of the field and were the only ones who got it right in the early 1980s. If there were just deserts, in the late 1980s the Agency would have been congratulated for a good job. Yet it became faculty club wisdom that the CIA in those years was making the USSR appear stronger than it actually was in order to provide a justification for the American military buildup.

¹⁶ The emergence of this paradigm is a riveting story that does not fit into the format of the present paper.

¹⁷ Kazmer and Whitehouse (1978), and Rowen (1982) are CIA forecasts.

This libel came on top of an earlier wrong. In the late 1970s and early 1980s, the same organization was widely believed to paint too bleak a picture of the Soviet economy, again for political reasons. The very fact that the CIA analyses of the Soviet economy depicted “Soviet economic prospects as more desperate, and Soviet policy choices as more dramatic, than most specialists consider plausible” (Hanson, 1981, p. 41), detracted from their credibility. Eventually, the CIA stopped publishing its forecasts because of the negative public reaction they generated.

Even as the CIA was issuing its lowest ever growth forecast, it declined to consider catastrophic scenarios (Rowen, 1982, p. i).

Predicting the imminent collapse just on the basis of inexorable economic trends would have been a correct prediction, but for the wrong reasons.¹⁸ First, the forces underlying the long run slowdown could not be expected to operate forever (Ellman, 1986, pp. 531-33). Second, even if the trend were to lead to zero growth, this would not necessarily mean a collapse. Economies are sturdy; they may stagnate or even contract, but do not collapse by themselves. Political systems are relatively more fragile. Economic collapses in the modern world are invariably caused by political ones.¹⁹ A functioning political system is capable of counteracting the ill effects of economic trends, as Andropov’s reign promptly demonstrated (Kontorovich, 1985).

The slowdown encouraged the ruler’s risky destabilizing moves, but did not in itself force them. Contemporary writers noted a variety of options open to him (Hanson, 1981, pp.). Consider the sense of surprise at collapse that persists even several years after the fact: “How could Mikhail Gorbachev ... suddenly get drunk with the dream of his ‘new thinking’ and give up everything - missiles, cannons, the empire, satellite countries - as a losing gambler, without

¹⁸ Kontorovich (1989b), written in early 1985, suffered from this.

¹⁹ Thus, famines in Europe in the last three centuries could have been averted by a political action (Fogel, 1988, p. 100).

any reason, without getting anything in return? Why? What for? ... How could a colossal empire fall apart without a shot, without an obvious cause, amid universal silent indifference - who can comprehend this?" (Radzikhovskii, 1993, p. 5). Yegor Gaidar calls the speedy collapse of the Soviet economy "the mystery of the XX century" (Bekker, 1994). The *post factum* sense of surprise and the failure to foresee the collapse come from the same source - the Soviet system was perceived as viable both by the outside observers and by its own citizens.

3. The military economy and the arms race

Economists writing about the Soviet prospects in the 1980s were trying to figure out the threshold for low growth to trigger a large response from the Soviet regime. This type of prediction required some assumption about what economic growth was needed for in the Soviet system, i. e., about the rulers' objectives. Once upon a time high rates of growth had propaganda value in and of themselves, but by the 1980s this was no longer true.

The economists' "majority" view underestimated the weight of military might as an objective of the Soviet rulers, and therefore largely misinterpreted the actions of the latter in the 1980s. Demonstrating this contention of mine is more difficult than collecting and comparing numbers on forecast and actual Soviet growth. Correspondingly, I will now discuss the actual status of the military sector and the "majority" Sovietological view of it at length.

3.1. Why does grandma have such sharp teeth?

Economists' preferred measure of the role of the military sector is the share of military expenditures in GNP (military burden). Estimates of the military burden (and such related measures as the share of machinebuilding output preempted by the military or the share of investment going to the military sector) produced by the CIA and by a number of independent analysts were extremely high. There has been criticism of the CIA numbers from some academics.

However, there has been no independent validation of these estimates. The Soviets, apparently, never bothered to compute the burden of defense on their economy.²⁰ The numbers they cite are the same sort of indirect estimates foreigners produced. In principle, one could go through their archives and compute a number that would resolve the dispute. There are two big problems with this approach. Firstly, the relevant quantities are still for the most part secret. Secondly, in order to add up tanks and missiles, one needs economically meaningful prices, and it is not clear what these should be. In any case, I do not know of any attempt to establish *post factum* what the Soviet defense burden, or the size of the military sector, was.²¹

An alternative approach would be look at the qualitative information about the place of the military sector, supplied by the former rulers after the collapse. (After all, we are interested in the subjective weight the leaders gave to defense.) Former top Soviet officials tell us that the military sector was the most important one in the economy.²² It had the first crack at resources, and concen-

²⁰ See Luboshits and Tsymbal, 1992; Treml, 1992, p. ??.

²¹ There has been some revision of data pertaining to the particular elements of the military sector. Thus, the number of nuclear warheads turned out to be one third larger than estimated (Broad, 1993). The Soviet chemical and biological warfare program "is roughly 10 times larger than U. S. ... intelligence specialists had estimated prior to the Soviet breakup." (Douglass, 1995). Other things being equal, this suggests that the CIA underestimated the Soviet defense burden.

²² Soviet rulers' memoirs are surveyed in Ellman and Kontorovich, 1997. Ellman and Kontorovich, forthcoming, contains statements by former Soviet officials. Soviet/Russian economists writing about

trated practically all high technology. The burden of defense was bleeding the other sectors dry, and by the mid-1980s was becoming unsustainable. It was a separately planned and managed secret economy, in all respects more privileged than the civilian one. It was also the most effective, allowing the USSR to maintain military parity with the rest of the world. We also learn of the panic caused in the Soviet leadership by the intermediate range nuclear missiles based in Europe and SDI right before Gorbachev's assuming the helm.

3.2. *The missing sector*

The economists' view of the military sector was not as uniform as their sunny expectations of Soviet growth. Some got it right even back then. However, the majority opinion through the 1970s and early 1980s diverged significantly from the "sharp teeth" picture sketched above.²³ In order to document this assertion, I will examine three categories of Sovietological publications: textbooks; scholarly volumes dealing with the whole economy; and books on technical innovation and research and development (RDI). The first two types of books give comprehensive overviews of the system and should be expected to note the features they consider important. The third category deals with the most militarized sector of the Soviet economy besides machinebuilding.²⁴

the conversion of the military industry made similar assessments (Iaremenko, 1990; Ozhegov, et al., 1991).

²³ In my own work on Soviet growth and the R&D sector, I was with the muddled majority.

²⁴ In a departure from our announced methodology, we include books by non-economists on research, development, and innovation (RDI). There are simply not enough books by the economists on this topic.

An important sector should merit a chapter in the book dealing with the economy. Text books, readers, and most scholarly books listed in Table 4 have chapters on agriculture, the economy's least successful and for a long time lowest priority sector. Some also include chapters on construction, trade, and finance, the latter two also low priority, peripheral sectors. Yet with two exceptions, there are no chapters on military industry.

The early volumes on RDI have no chapters on the military (with the exception of Amann and Cooper, 1982). Of course, no one can quarrel with, say, Berliner's (1976, p.) decision to focus his book on civilian industry. Yet the aggregate result of such decisions has been a body of literature missing the larger part of the RDI scene in the Soviet economy.

The two late-1980s volumes that have chapters on the military indicate an apparent increase in the recognition of this sector's importance as the decade drew to a close. (Witness the inclusion of a military chapter in the later edition of Gregory and Stuart and the discussion below.) The gradual recognition of the military sector's central role as the system was unraveling does not contradict my main thesis, and probably strengthens it.

The presence of a chapter is admittedly a very crude indicator. I also calculated the share of the pages mentioning words "defense", "military", and their derivatives (based on subject indexes) in the books' total number of pages.²⁵ Out of 20 textbooks and scholarly volumes, 6 do not mention these words at all; 5 mention them on 1% or less of their pages (third column of Table 4). The only textbooks that break past the 3% mark are by Krylov and Hutchings, both non-establishment figures.²⁶ The situation is better with scholarly books: two "establishment" volumes devote substantially over 3% of their pages to the topic (Millar; Bergson and Levine).

²⁵ Each page number was counted only once, no matter how many times it was listed under the appropriate entries.

The two formal indicators considered here suggest that Sovietologists for the most part did not hold the military sector to be of special importance for the Soviet economy. The main sponsors of Sovietological research were Western governments concerned first and foremost with the military threat. Government compendia on the Soviet economy, such as the Joint Economic Committee volumes, invariably contained chapters on military industry or expenditures.²⁷ In its neglect of the military sector, the profession exhibited a surprisingly high degree of independence.²⁸

Western descriptions of the Soviet economy without the military sector have an obvious source: Soviet accounts. Indeed, the most important sector of the economy was also the most secret. It practically did not exist in the Soviet economics literature, which served as the main source of information for Western Sovietologists. Guarding strategic information was not the only objective of the Soviet ban on description and analysis of the military sector in the open press. The other objective must have been propagandistic. It seems hopelessly naive that a state with a virtual war economy during peacetime may project a peaceful image by simply keeping mum about the guns. But the

²⁶ The author's status in the profession can be measured by the number of citations. I use a cruder, binary indicator of whether the author has a position at a research university or an equivalent institution (e. g., Brookings). Neither Krylov nor Hutchings did.

²⁷ See US Congress 1976, 1979, 1982, 1987.

²⁸ This neglect of the main sector carried over into the transformation studies. The military legacy of the USSR proved to be a powerful hurdle for market transformation (Kontorovich, 1996). This was predicted by Iaremenko (1990), but not the Western writings (see, for example, Peck and Richardson, 1991).

number of Western textbooks in Table 4 that do not mention the military sector testify to the success of this simple ploy.

The Soviet secrecy was not an insurmountable barrier for understanding the qualitative aspects of the military sector. Its role as the main objective of growth was appreciated early in the discipline's history (Grossman, 1953, and Nutter, 1962, p. ??). **Military might has been recognized as the main (only?) fruit of the Soviet economic growth** (Bergson, 1981, p. 42). The role of military balance and growth in the 1980s has been correctly perceived by Hanson (1981, p. 30).

One would think that the CIA had a monopoly on studying the exact size and the fine details of the operation of the secret sector. But even here academic researchers have done a great deal. In addition to Amann, Cooper, and Davies (1977) and Amann, Cooper (1982), one should mention the steady output of Rand Corp. scholars and the heroic efforts of independents such as Dmitry Steinberg.

The lack of appreciation of the military sector's primacy precluded most Sovietologists from seeing the desperate bind in which Soviet leadership found itself in the early 1980s, squeezed between the stagnant economy and the loud trumpets of the Western rearmament.

3.3. What motivated Gorbachev?

The 12th five-year plan (1986-90), which had been rewritten to reflect Gorbachev's objectives, envisioned a rapid growth of military expenditures ().

Hardt and Kaufman (1987, pp. xiv-xv) summarizing a compendium of papers by government and academic Sovietologists, concluded that "military priorities no longer have overriding primacy": military expenditures were to grow slowly and procurement was to remain flat through 1990.

Rumer (1986) read the 12th FYP correctly by a very simple method: the plan is oriented towards the machinebuilding, which in the USSR was a euphemism for military hardware.

Becker (1986, 1987) wrote extremely perceptively on what he termed “Gorbachev’s dilemma”.

Again, a correct diagnosis did not necessarily lead to a correct prediction. It was conceivable that the response to Western rearmament would be a wholesale sacrifice of consumption and a massive redirection of resources towards the military sector, coupled with tightening the screws domestically, or, alternatively, a strategic use of a new détente to make Western rearmament politically insupportable at home. All these predictions would have been compatible with the objectives of the rulers.

4. Market reforms

Most experts saw the Soviet system as infinitely malleable, hence capable of various degrees of liberalization and marketization. Market-oriented reform represented to them rationality and economic efficiency, evolutionary change, and a correlate of political liberalization.

4.1. The basis and the superstructure

Sovietology's crowning error was also its original sin. As early as 1921, liberal-left Russian émigrés like P. N. Miliukov saw the New Economic Policy as the beginning of the regime's evolution towards democracy. P. B. Struve objected that the system could not evolve, and that attempts to bring economic vitality to it would spell the end of Bolshevik rule (Pipes, 1980, pp. 315-19).²⁹ Sovietologists of the 1970s and 1980s were, for the most part, Miliukov’s heirs.

²⁹ Dmitry Shlapentokh pointed out this analogy to me.

The Struvian view that “... the dictatorship must be as it is or not be at all. Its long-run changeability appears limited indeed.” (Gerschenkron, 1968 (1962), p. 312) was rarely heard. The political risk of economic reforms took on a new meaning in this period, reflecting the group theory eruption in political science. Since economic reform inevitably hurt some established bureaucrats, the latter had reasons to dislike it, and the ruler had to brace himself for unspecified consequences of their displeasure (Hanson, 1981, pp. 32-33). Economists did not invent this theory, they just bought into it.

The Soviet collapse does not allow us to determine whether Miliukov or Struve was correct (it does reject the predictions of group theory, but this is what all the empirical observations always do). The political system had been wrecked by political and ideological changes long before the economic reforms had any chance to hurt it. One can see how a tumbling political system hurt the economy. It is hard to detect any systemic influences in the opposite directions.

4.2. *Markets: beware the granted wish*

Sovietologists ascribed the past failures of decentralizing reforms to bureaucratic resistance that could have been surmounted by a more decisive leader (Schroeder, 1971, pp. 37-43, and 1979, p. 325). Reform was seen as a feasible and even unavoidable - though politically difficult - step. But the profession did not stop at prediction, going so far as prescribing market reforms for solving

the USSR's economic problems. A section in Schroeder, 197? was entitled "What is to be done?", with reform as the answer. Thus most Sovietologists cheered when Gorbachev started the reforms.

The results disappointed the expectations of even the most modest reform mongers.³⁰ In sector after sector, market-inspired innovations clashed with the rest of the system, at best making no difference, but more often depressing economic performance. The mechanism of rejection of reform in various sectors is documented by the former Soviet officials and researchers in Ellman and Kontorovich (forthcoming).

This outcome was eminently predictable. There existed a line of writings doubting the compatibility of market elements with the Soviet-type economy going back to Grossman (1963, pp. ???). It was shown that even the minimal use of money and quasimarket institutions in the classical Soviet economy (*khozraschet*) conflicts with central planning (Granick, 1959, pp. ??, and Grossman, 1965, pp. ??). As so many other promising departures in the early Sovietology, this line was abandoned and picked up only in the 1980s, when the mechanism by which the Soviet economy rejected market elements got a closer look.³¹

4.3. *Alchemy and economics*

The belief in the government's ability to order society at will is often called "social engineering". Yet engineering is explicitly concerned with what can be built with a particular material, and what cannot. The closer analogy is the ancient quest for turning base metals into gold.

³⁰ Such as Bergson (1981, p. 42): "Uncertain as the results of such a reform might be, some improvement in performance over that of centralized planning must be possible."

³¹ Dyker (1981), Kushnirsky (????), Kontorovich (1988), and Ickes (1989)

Soviet economists were totally in the thrall of a perfect malleability assumption. This may be due to the Marxist idea about the jump from the realm of necessity into the realm of freedom as society becomes communist. It is the influence of this idea that accounted for the near-absence of positive economics in the USSR (Goldmann, 1969). The lack of understanding of economic processes further reinforced the illusion of perfect malleability.

This is not a specifically Soviet or Sovietological idea, but a venerable Western fallacy. Hayek (1973, pp. ??) calls it “Cartesian dualism” and traces it back to the French philosophers of the 17th and 18th centuries. The idea made its latest appearance in the literature on the transformation of the Communist economies (e. g., Peck and Richardson, 1991).

4.4. Correct diagnosis does not lead directly to a correct forecast

Those who saw Communism as unreformable did not expect serious attempts at change, since these would have been suicidal for the rulers. The advent of Gorbachev seemed to prove them wrong, and the majority of the profession right. As the system was being reformed into extinction, Miliukov's heirs kept believing in the eventual evolutionary outcome, but the heirs of Struve were not predicting a speedy collapse either. A defensive reaction, a counter-move to save the dying mechanism, appeared more likely. This reaction - Gorbachev's conservative turn in the fall of 1990 and the coup of August 1991 - did materialize, but turned out to be too weak to save the Soviet system. The majority of the Sovietologists believed (incorrectly, as history has taught us) in the reformability of the system and for thirty years constantly anticipated market-type reforms. The small minority who understood the fragility of the system were skeptical: why would a ruler put his own position at risk? The understanding of the potential destructiveness of the early Gorbachev (1986-89) policies could not serve as a basis for confident predictions of the collapse. A reaction to save the system would have been a more plausible conjecture.

5. Conclusion

Calculating and comparing errors in the forecasts of GNP is the most straightforward and objective part of analyzing economists' forecasting record. As we move from quantitative, single variable forecasting to a diagnosis of complex social phenomena, it becomes less clear what the forecast was, and what actually happened. Establishing the causes of errors (what part of the written record seems to correlate with the error) is even higher more ambiguous.

Speculating as to why the forecasters did what they did is the most ambiguous of all. Why was the continuation of the 1951-75 trend in industry not considered as one of the forecasting scenarios? Why were there no growth accounting exercises undertaken? Why was the chief sector ignored? Why believe in the infinite malleability of institutions? The studies of economic forecasts cited in the beginning as the inspiration and model for this paper did not go that far, but I will try to sketch the possible relevant considerations.

The study of the Soviet economy has been pushing against the genuine limitations of economic knowledge. For several centuries, economists were concentrating on the study of markets and practically ignoring the non-market organizations, such as the internal structure of the firm. When there emerged a whole marketless economy, economists had no tools to analyze it. They still don't have many, as the study of the "visible hand" is in its infancy (Coase, 1991, pp. 38-39; Williamson, 1991, p. 160).

The system of incentives in academic economics puts a premium on formal modeling and innovative mathematical techniques (Leontief, 1971, pp. ??? and 1982, pp. ???). Time consuming data gathering work therefore does not pay. For Sovietologists, getting new information involved learning and using Russian, which further lowered returns on empirical work. In terms of these in-

centives, applying a slightly different statistical technique to the same capital, labor, and output data that have been exploited many times before is better than trying to introduce fresh evidence.

The same incentives favored the use of the models developed for market economies for the analysis of non-market ones. Developing new models specifically for the case in hand would force one to start with quite unsophisticated, simple (hence, less prestigious) techniques that take decades to refine.³² Hence Barrington Moore Jr.'s understanding of the importance of pressure from the superiors was supplanted by models of bargaining and trading relations between superiors and the underlings.

Sovietology was a small field.³³ There were barely enough people to specialize in all the important aspects of the Soviet economy, such as investment or innovation. Each specialized expert relied on the results of the others. Yet there were not enough Sovietologists to generate debate within most subfields, or to replicate and cross check each other's results. Shoddy work would go undetected for decades and form the foundation of research in related fields. This would explain the capital stock scandal.

In a small field, everyone was a graduate student or a great graduate student of the founder, and knew personally everyone else. This accounted for the lack of controversy and self-policing described in section 1. above. The small size also interacted with the incentives of academic economics. Say what you will about the misallocation of effort in "big" economics, there are enough people left there pursuing fruitful strategies and moving the whole discipline ahead. In the sparsely populated field of Sovietology, misdirected effort was too often the only effort.

³² This seems to be behind the controversy surveyed by van Brabant, 1989.

³³ See Birman, 1980, on the industrial organization and sociology of the Sovietology.

The present state of the economics, the structure of incentives in the academic world, and the industrial organization of Sovietology explain most of the misdiagnosis of the Soviet economy. There is little left to be ascribed to the political pressures.

Also, unlike in other social science fields, there have been no publications in economic Sovietology dealing with political bias in the profession. This is not necessarily the sign of its absence. It may be that the economists are just smart enough not to brag of their biases as proudly as Gleason (1995) did. Or they may not be brave enough to confess in their own biases and challenge those of their colleagues, as Genovese (1994) did.³⁴

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³⁴ "No one should be surprised that none of our leading historical associations have thought it intellectually challenging to devote sessions at their enormous annual meetings to frank discussions of the socialist debacle." Genovese, 1994, p. ???.

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Table 1. Forecasts of Industrial Output (CIA Methodology).

Table 2. Forecasts of GNP, average annual growth rates, %.

Notes and abbreviations for Table 2.

* "in the next few years". It is not clear that Nove speaks about GNP as calculated by the CIA.

** "for the foreseeable future"

*** "1980-2000"

GGL&M 1976 Green, Guill, Levine, and Miovic, 1976

HE&F 1976 Hunter, Earle, and Foster, 1976

K&W 1978 Kazmer and Whitehouse, 1978

Table 3. Estimates of TFP using the official and deflated capital stock data, average annual growth rates, %.

	1961-65	1966-70	1971-75	1976-80	1981-85
1. GNP	4.7	5.0	3.0	2.3	2.0
2. Labor	1.6	2.0	1.7	1.2	0.7
3. Capital	8.8	7.4	8.0	6.9	6.3
4. Land	0.2	0.0	0.1	-0.1	-0.1
5. TFP estimate	-0.1	0.6	-1.4	-1.4	-1.2
6. Capital deflated	4.2	3.2	4.1	3.2	2.0
7. TFP estimated with 6.	2.0	2.5	0.25	0.22	0.73

Sources: Rows 1.-5. from CIA, 1986, p. 70.

Row 6. from Khanin, 1991, p. 176.

Row 7. estimated using data in 1. - 3., 5., and the CIA (1986, p. 70) factor weights.

Table 4. The role of the military sector in the Soviet economy.

	Chapter or section on military industry	“Defense” & “military” in index, %% of total
<i>Textbooks, readers, non-technical surveys</i>		
Campbell, 1960	no	0.0
Nove, 1961	no	1.3
Krylov, 1965	yes	4.4
Sherman, 1969	no	0.3
Spulber, 1969	no	1.0
Cohn, 1970	no	1.0
Kaser, 1970	no	3.0
Gregory & Stuart, 1974	no	0.4
Bornstein, 1981	yes*	0.0
Hutchings, 1982	no	7.8
Dyker, 1985	no	2.6
Gregory & Stuart, 1986	yes	2.8
Nove, 1986	no	0.8
Buck and Cole, 1987	no	0.0

<i>Scholarly volumes dealing with the whole economy</i>		
Bergson, 1964	no	0.0
Ames, 1965	no	0.0
Millar, 1981	no	20.6
Bergson & Levine, 1982	no	5.6
Hewett, 1988	no	3.1
Ellman, 1989	no	0.0
<i>Scholarly volumes on R&D and technological change</i>		
OECD volume 1969		
Berliner, 1976	no	1.1
Amann, Cooper, & Davies, 1977	yes	2.6**
Cocks, 1980	no	n.a.
Lubrano & Solomon, 1980	no	n.a.
Amann & Cooper, 1982	yes	23.2
Balzer, 1989	yes	17.4
Fortescue, 1990	yes	9.4

* A chapter on military and economic aid to the less developed countries. ** Large number of references to particular weapons systems (ballistic missiles, tanks, etc.).

Table 5. Military considerations in the 12th five-year plan.

Source	priority high	expendi-tures to increase	investment geared to mili- tary
Hanson, 1986			
Hewett, 1986			
Rumer, 1986	yes	yes	yes
Hardt & Kaufman, 1987	no	no	
Becker, 1986, 1987	yes	no?	
Dyker, 1987			
Ellman, 1989b			