

**Day 2**  
**Morning Session**  
**(Transcript)**

Date: 12 December 2008, Morning Session  
Venue: San Francisco, California

Prof. Kazumasa Iwata, President, Economic and Social Research Institute (ESRI): Good morning, ladies and gentlemen. Today, we have three sessions. The first session is on “The consumption, land prices and the monetary transmission mechanism in Japan.” We have two professors who will make their speech; first, Prof. Muellbauer, please.

Prof. John Muellbauer, Nuffield College, Oxford: It is very nice to be here, partly also because my alma mater is just across the bay—Berkeley, California. One of the good things about being in Oxford is that we attract great students. So, over the years, I have had some wonderful Japanese students including Wataru Takahashi, Toshi Sekine, and Keiko Murata. This paper is closely connected with Keiko’s doctoral thesis at Oxford which she completed 10 years ago. We have updated and extended that research and amazingly enough, everything that we found then still holds.

The paper is about aggregate household behavior in Japan and throws some light on the monetary transmission mechanism in Japan. We argue that Japan is very different from the US and the UK in the way that monetary transmission works via the household sector. The argument is that lowering short-term interest rates in Japan is far less effective for stimulating the economy than it would be in the US and the UK.

We are not saying lowering interest rates has no positive effects in Japan. Indeed, our reduced form forecasting equations for GDP or personal income find the conventional negative interest rate effects. It is just that these effects are quite a lot weaker in Japan than they are in the UK and the US. This has the implication that when monetary policy was eased in the ‘lost decade’, it was far less effective than it would have been in the UK and the US. This is rather important to understanding comparative historical experience and policy choices..

A brief outline: the model that we use is a modern version of the Modigliani and Brumberg—or if you like, Ando-Modigliani—solved-out consumption function. That is so that we can analyze the asset effects explicitly and also the interest rate effect. This is not a Euler equation. Let me also say that we do not have a general equilibrium explanation for the Japanese economy, although as it turns out, in the process of checking various exogeneity issues, we have actually estimated equations for an interest rate reaction function, a Phillips curve as well as some reduced form output and income forecast equations, a debt equation as well as consumption functions. These are elements of a complete system but we do not have a complete system with all the feedbacks.

Previous studies on Japan have found a significant wealth effect for financial wealth. Findings on interest rate effects are mixed, though some also find positive effects. Model specification has quite a large bearing on how robust are the estimated interest rate effects. This paper also links with recent work that I have done with other colleagues using the same approach of the solved-out consumption function to analyze the UK and the US consumption behavior. For the US and the UK summarizing Muellbauer (2007)'s Jackson Hole paper, we found the marginal propensity to spend out of liquid assets minus debt is around 0.1, and rather lower for illiquid financial assets. For the US and the UK, we find negative real interest rate effects but positive housing collateral effects, which have increased with credit market liberalization. When we estimate income forecasting equations for these countries, we find some evidence for Ricardian effects: in other words, higher government deficits tend to have negative effects on income forecasts.

Let me examine the implications of this approach for monetary transmission. Our estimated solved-out consumption equation contains a number of controls. Here are the various ways in which interest rates could operate: first of all, there is the direct interest rate effect, given income and wealth, and expected income. The argument of the paper is that the sign of this effect depends on the context. It depends on the net asset to income position and also on preferences. Because of the high level of liquid assets in Japan, one is far more likely to find a positive effect in Japan than a negative one.

Secondly, there are wealth effects, via asset prices. Everyone agrees that via financial assets, interest rates have positive effect on consumer spending, while with physical assets, it is far from clear. The latter effect depends very much on the structure of the economy. Depending on the availability of credit in the economy, the sign of the interest rate effect via physical assets could be positive or negative.

There is a particular credit channel effect in floating rate economies. In economies like the UK where short-term interest rates affect very strongly the interest rates paid by debtors, when interest rates change, so do the cash flows of debtors and this can have large effects on consumption.

There is also a more general credit channel effect. As opposed to the classical wealth effect, the credit channel effect via asset prices works through collateral constraints

and their release and potentially through the down payment constraint. First-time buyers have to save up for a down payment to enter the housing market. If that down payment is large, then higher house prices can have a negative effect on consumption. So, credit conditions have a big effect on the composite sign, the combination of the collateral and down payment effects.

Finally, there is an effect via expected income growth which depends on the system response of how investment, exports and so on respond to interest rates. This discussion can be clarified further by taking the two key equations in our paper.

First of all, consider the equation for income growth expectations. This is important because we know from life cycle consumption theory that permanent income rather than the current income ought to be what drives consumption. So if we are going to estimate a solved out consumption function, it is crucial that we model the income expectations which are embodied in 'permanent' income.

We use the conventional rational expectations approach to impute the fitted value of future income growth to households, as a representation of their expectations. Thus the dependent variable is the one-year-ahead growth rate of non-property income. Among the determinants is the feedback to current income, expected to have a negative coefficient. We include trend effects picking up the slowdown in the trend rate of growth in the Japanese economy after 1973. The Ricardian effect is represented by the government balance to GDP, included as a three-year moving average. The change in the nominal interest rate captures monetary policy. The effect of the rest of the world on Japan is proxied by United States GDP.

The solved out consumption equation is a relatively simple extension of the textbook permanent income hypothesis. In the simplest textbook model, there is no interest rate term, and no land price term. But the asset income ratio would be there as would be the deviation of permanent income from current income. With habits or costs of adjustment, there is a partial adjustment mechanism. Effectively, we have incorporated four additional variables on top of the classical permanent income hypothesis. So, what are these four additional variables doing? The inclusion of the log change in current income reflects the possibility that some household may be credit constrained, or some households simply spend a given proportion of income and are not inter-temporal optimizers to the same degree as 'textbook' households might be.

Another term deals with uncertainty. Our discussant is one of the leading researchers who has examined the effect of income uncertainty on consumption. Essentially, in our model, consumers discount future expected income growth by a measure of income uncertainty. So, the greater income uncertainty, the less future income growth matters, as suggested by literature of the last 30 years on precautionary behavior.

A third additional term is the real interest rate whose presence is implied by inter-temporal substitution. Finally, the inclusion of the real land price simply comes from an extension of the classical model to two assets: namely, housing as well as financial wealth. This implies an additional role for the asset price of housing, and this could be positive or negative, depending on the institutional structure of the economy and the availability of credit.

Two brief comments on data issues: one of the key features of theory is that non-property personal disposable income matters rather than conventional PDI. We use the Blinder-Deaton adjustment to estimate non-property income. In Japan, we have the 68SNA data and 93SNA data. We spliced them in 1980, but the results are quite robust, even if we take the old data as far as it goes, to the late 1990s. Regarding household debt, our measure excludes unincorporated enterprises. Many other data issues are discussed in the paper.

Empirical results for the equation for the forecast income growth are shown in Table 1. The feedback term is highly significant and confirms that in the long run, trends really matter and that there was a slowdown after 1973. The nominal interest rate effect is negative and the United States GDP effect is positive. The Ricardian effect is highly significant. We also have an alternative specification, where instead of using the three moving average of government balance to GDP, we simply put in the government debt to GDP, with both one-year lag and four-year lags. These are highly significant. The last equation produces very similar forecasts to the other three. Stability for the different samples is fine. Thus these are robust equations and we think they make very good sense. We think Japanese households do actually think about the government budget constraint and take that into account in the way they view future income growth.

Prof. Keiko Murata, Tokyo Metropolitan University: Table 3 presents the results of consumption function. Having dropped insignificant variables, the last graph shows the final result. I would like to emphasize three points. First of all, we found that the real interest rate has a positive effect on consumption which is shown in the third row from

the bottom. Second, the marginal propensity to consume out of net financial asset to income ratio is around 0.06 in the long run, which is obtained by dividing this coefficient shown in the second row from the bottom divided by this coefficient. Third, the real land price or fiscal wealth to income effect has a negative effect, which is shown in the bottom of this table. Although it is not shown here, we included fiscal wealth to income ratio instead of the real land price, we also had a negative sign.

After having this result which is shown in the fourth column in this table, we did various robustness tests, some of which were following the comments we obtained from our discussants in the March conferences. The first four columns of this table show the results using the different sample periods. I would not go into details because time is limited, but the results generally show that our model is quite robust. In the last column, the estimated coefficients were not statistically different either when we estimated the model using instrumental variables to avoid possible endogeneity problems. We also have done further robustness tests. This table shows the result using only 68SNA national account series as John introduced just now, and the results are quite robust. Also, we did further tests, such as the stability test which is shown here. As you see, all the coefficients look stable. The long-run parameters obtained in the consumption equations are not statistically different from those obtained in the system analysis of co-integration. Also we did a weak endogeneity test which are accepted, and the results are also supported by IV estimates as I just explained in the earlier table.

Now, this slide shows the long-run contribution to the ratio of low consumption over income by using the result of our model. Please remind that this chart shows those contributions given net financial assets and income. From these figure, let me underline three points. First, we can see there is a positive correlation between the real interest rate which is shown in the blue line here—this is the real interest rate—and the consumption to income, which is the red line here. Second, among these three variables, which are the real interest rate and the net financial asset income ratio and the real land price, the upward shift of consumption to income since mid-1975 is mostly attributable to the rise in the net financial assets, as shown here. Third, the contribution of real price of land is found to be relatively small as shown in the green line.

This is the result of the system analysis of co-integration, but I will skip here. With regards to the positive real interest rate effect, although robustness test was passed, someone might argue that this result might be biased due to the positive financial liberalization as in the UK between the end of the 1970s and the mid 1980s.

Considering the fact that we obtained the stable effect in consumption, we can say it is unlikely. Also in Japan, the net financial asset to income ratio increased steadily even in the bubble period. As we see here, during the bubble period the debt income ratio increased, but the net financial to income ratio also kept increasing. On the contrary, if we look at the US case, during the bubble period, the debt income ratio increased substantially and the net financial assets substantially declined and eventually turned to negative around 2000 and 2001, as most of you might know.

To test further we estimated the simple debt equation to see whether there are any structural breaks which might suggest a structural break in the long mortgage market, as John obtained in the UK or in the US in his paper presented in Jackson Hole last year. This shows the result of the debt equation, so I would not explain in details, but the debt equation on Japan generally shows that we have very stable results, and there is no fact which shows any structural breaks in Japan. So, in this paper, we argue that the positive real interest rate effect is not affected by financial liberalization.

Let me conclude. First, the results suggest that the fall in interest rates reduce consumption in Japan, given income and assets. Second, there is a negative real land price effect, even if the effect might not be so large. Thus, our results explain one of the possible reasons why Japan responded only weakly to lower short-term rates, and also helped explain why fiscal policy was fairly ineffective.

Finally, let me say a few words about the policy implication to the US and the UK suggested by our results in conjunction with John's paper last year. First, in the United States and United Kingdom, households have negative net financial assets, hence a lower real interest rate boosts consumption. Second, the asset price channel is larger, given higher stock market wealth to income ratio and the positive housing collateral effect. However, it is important to fix a transmission from the policy rate to the rates faced by households as well as the asset market.

Lastly, although we have not looked at the Eurozone in our paper, we suspect that the Eurozone might be relatively similar to the case of Japan. This is because for instance, in Germany, the home equity loan is not as popular as in the UK and the US and household debts are not so high. Thank you.

Prof. Iwata: Okay. Thank you very much. Then we have discussant, Prof. Gourinchas please.

Prof. Pierre-Olivier Gourinchas: Thanks. Okay, well thank you very much for having me here and asking me to discuss this paper by John and Keiko. This is a paper that, as they presented, investigates the properties of the Japanese aggregate consumption function; and along the way they find a number of interesting results, one of which is that consumption seems to be increasing with real interest rates, and also that it seems to be decreasing with housing wealth or residential land prices and they derived implications from these results for the conduct of monetary policy in Japan versus other countries.

Now, let me put this in a little bit of context which, as a matter of fact, is probably the main context of the conference here. Many countries have experienced a significant economic slowdown in the context of the collapse of the housing bubble and significant weakness in the banking sector when we think about the US and the UK in particular, but many other countries as well. In response to this, in many countries, there has been an aggressive monetary policy easing, with policy rates that are now close to zero, for instance, in the US or dropping really fast in the UK or the European Union.

In addition, we have a deflationary environment that is emerging due to the decline in economic activities as well as significant retrenchment in commodity and energy prices since last July, and therefore the specter of liquidity trap in many countries. So, that is why all eyes are on Japan in a sense and everyone is eager to learn about the experience of Japan in the late 1990s and the early 2000s. In this context, this paper is really interesting because it argues “Well, one has to be careful in drawing lessons from the experience of Japan because Japan is different and it is different in the way in which monetary policy is transmitted to the aggregate economic activity.”

So, how do they establish this result? Well, they estimate what they called the solved-out consumption function on aggregate datum. The solved-out consumption function is based in the life cycle model, but then they augment it with the number of reasonable right inside variables that might influence consumption in a sort of augmented model. That is where they find that when they bring this solved-out consumption function to the data, that consumption increases with the real interest rate and decreases with residential land prices.

How do they interpret the findings? They say, well, the fact that consumption increases with interest rates may be because Japanese households have a relatively low

intertemporal substitution and a relatively high financial wealth, so they are unwilling to have consumption moving a lot from period to period. Since they have a lot of financial wealth, maybe the income effect is actually going to be quite strong. As for the link between housing prices and consumption, the argument is that Japanese households face somewhat tighter credit constraints in other countries, the absence of collateral markets that would allow Japanese households to borrow against the value of their house or the down payment constraints that forces Japanese households to save a significant fraction of the value of the house, might imply that an increase in house prices forces Japanese households who want to acquire a house to save more, and therefore reduce consumption.

Overall, the paper argues that this implies a weaker transmission of monetary policy through the interest rate channel than in a country, say, like the US or the UK. Okay, so overall I find that this is a very interesting argument although I would say also that I do not find it completely convincing. To tell you why I do not find that it is completely convincing, I am going to start my discussion with some remarks about the link between consumption and interest rates and partial equilibrium, and then move on to think about the link between consumption and interest rates in general equilibrium that will maybe cast some doubts on the kind of partial equilibrium intuition we may have. I will go from there and discuss a little bit what I call the perils of estimating the solved-out consumption function, especially on aggregate data. Finally, I would sort of come to this question about the lessons that can be learned from Japan in the late 1990s and early 2000s, and wonder whether looking at the interest rate channel, what is the best way to approach the question of the transmission of monetary policy in the context of monetary policy regime, where we are hitting the zero bound on nominal interest rates.

So, let me start with the sort of warm up, sort of a standard two-period consumption problem; you consume today, you consume tomorrow. You have utility that is at present discounted that is of the expected utility form, data is the discount factor, and you face an intertemporal budget constraint which is totally standard here, and I am assuming that there is no initial asset, but it can readily be added to the discussion, and you have utility per period, that is the constant necessity of institution  $\sigma$  here.

So, write it down; solve for consumption in the first period and you are going to have consumption that is proportional to your present value of resources,  $y_1 + y_2$  divided by  $1 + r$ , and the marginal propensity to consume here will depend on the discount factor and the interest rate on savings with some exponent  $\sigma - 1$ . The  $\sigma$

represents a substitution effect, the fact that the higher interest rate would lead you to shift consumption toward the second period. The minus one represents the income effect that you can afford more consumption overall if interest rates are going up. The one plus  $r$  over there is the wealth effect, the fact that your income in second period is worth less than present value when interest rates go up. So the overall effect of a changing interest rate on your consumption in the first period will reflect these three standard effects: income, substitution and wealth.

If you sort of calculate what the total derivative of consumption with expected rate is, you obtain this expression here, where the second term in the numerator is your substitution effect, it is controlled by  $\sigma$  here; that has to do with the fact that holding future consumption constant you want to consume less in the first period. The net of the income and wealth effect is sort of captured by this term here,  $y_1 - c_1$ , and you see that depends on whether the household is the first period borrower or lender. If you are a first period borrower,  $y_1$  is less than  $c_1$ . This first term is also negative and the consumption is certainly going to decrease as interest rates go up. If you are a lender so that  $y_1$  is bigger than  $c_1$ , then your consumption may increase, if you are sufficiently enough of a lender then that will offset the substitution effect, and your consumption may increase with interest rates.

There is a discussion in the paper that is very much as this flavor; sort of looking at the response of consumption to interest rates and they say, "Well, you know,  $\sigma$  may be low for Japanese households because the typical Japanese household likes to have a very smooth consumption profile," and here in their discussion, they do not just have income but they also have initial wealth and argue "If the Japanese household has already a lot of financial wealth to start with, then that income effect is going to be strong and that might overcome the substitution effect." So, that is the sort of the background for the interpretation of the empirical result that they discuss in the paper. And it is all perfectly fine, but one has to be worried about the fact that here in this discussion, output and interest rates are taken as given. We are sort of doing this partial change, we are assuming that we change interest rate holding so we take the interest rate as given and we are also taking out what is given.

But in general, these things will be determined jointly with consumption and that is where things become a little bit more complicated. So, for instance, consider a very simple setup where you have a closed endowment economy and in this closed endowment economy without the government or any fancy things, you will have

equilibrium that consumption is equal to output. That will be true in period one. That would be true in period two. What this implies, well, it implies that the real interest rate is going to be the variable that adjusts to make sure that this is the case. Actually, there is a typo here, it should be minus sigma here. The interest rate will be related to income growth between period one and two and the discount factor. If you plug the  $c_1 = y_1$  into the partial derivative, in other words, if you estimate the slope of the consumption function, add the equilibrium values, what you will find is you will be left with only the substitution effect which should always be negative. You would not have any income and wealth effect, that would basically cancel out exactly. Because on average, the representative of the household of this close economy would not be a lender or a borrower. He would just be consuming the endowment of the economy.

So, one has to be a little bit careful in sort of taking this intuition from the partial equilibrium and plugging into an aggregate equation that you want to estimate, because you are sort of bypassing the fact that a lot of these variables will be adjusting so that you satisfy the long-term constraints, the markets are clearing, all the general equilibrium considerations.

Now here just to labor this point a little bit more, since consumption is equal to output, one could not estimate the aggregate consumption function on this aggregate data. It is still possible to get a slope for this consumption schedule that would be positive if you enrich the model a little bit. For instance, you say, "Well, this is not a closed economy," in that case then it is possible for the Japanese households and average to be increasing their consumption with the interest rate, if they are net lenders to the rest of the world, and that after all is not such a bad characterization of the position of the representative Japanese household. So, maybe we still have this effect because of the net external position of the Japanese household vis-à-vis the rest of the world, but then we would have to sort of ask questions about "What is the interest rate we are talking about? Is it the Japanese real interest rate that is relevant or is it the rate of return on these external assets that Japanese households are holding?" In many models it would have to be the same in expectations but in practice, they may not be. So, we would have to ask ourselves, well, what is the interest rate we are talking about? How is it determined? And how is output determined as well?

Now, this is just a simple 2-period real economy. If I move on to monetary environment... so here I am showing you the stylized neoclassical monetary model with steady price that everyone is using. It consists of three equations; one is this is equation

which is just a linearized version of the literal equation of the consumer problem so it will capture the substitution effect. In other words, holding constant expected future output gap, the current output gap decreases with the nominal interest rate set by the monetary authorities.  $x_t$  here is the output gap, the gap between output and the natural level of output.  $i_t$  is the policy rate. This is expected inflation.  $r_t^n$  is the natural rate of interest which is the rate of real interest rate that would obtain in a frictionless environment where output would be equal to its natural level. You supplement this with a rational expectation Phillips curve where inflation depends on the output gap and expectations of future inflation, and this natural rate of interest here is itself a function of the natural rate of output. In other words, it is a function of all the real disturbances to the economy, whether it is productivity shocks or other things.

Now here, we have both output and this natural rate of interest that is endogenous, and this policy rate could be set in a number of ways, maybe with the policy rule, maybe with some disturbances. Let me consider one of the ways in which it could be set just to, again, highlight the point I want to make here. Imagine that the monetary authorities will stabilize inflation at a constant level  $\bar{\pi}$ . Well, there is a very simple way of doing that if you can commit to it which would be to set the policy rate equal to this natural rate of real interest rate plus this target inflation  $\bar{\pi}$ . If you do that in every period, then it is obvious that if you plug that back into these equations above there, you will obtain that inflation is always equal to  $\bar{\pi}$  and you will obtain that the output gap is also equal to a constant. So, the output gap being equal to a constant tells you that output—or consumption which is again the same thing in the standard model with capital accumulation—would be equal to the natural level of output plus the constant.

So, the policy rate here in this simple example—this is the situation where the Bank of Japan would just be able to observe all these real disturbances and be able to calculate the appropriate natural interest rate that results from that—would be just a complex function of all the shocks that are hitting the economy on the real side, just like output and consumption themselves should be also functions of the same things; once again, you would find yourself in a situation where you cannot just from the aggregate data recover the consumption functions. Not because  $c = y$  in equilibrium that it implies that  $c = y$  is the consumption function—that is just the solved-out, if you want; the solved-out completely for the general equilibrium but it is not the consumption function.

Here, what happens is that income adjusts endogenously; the policy rate responds to the state of the economy as well... If you are unhappy with the formulation I have in how to

set interest rates, well, you could assume some sort of data rule or any kind of interest rate rule that would also respond to the state of the economy, and therefore capture endogenous features of the shocks that are hitting the economy. Moreover, if you think about putting asset values on the right hand side, things like housing wealth or liquid financial wealth, they are also things that embody expectations about future output or consumption through, for instance, the stochastic discount factor that is applied to prices of these different objects and also the sequence of future payments that different assets are making. So, it also is not separately observed from the shocks to the economy.

So, most of the right hand side variables here that are in the solved-out consumption function are endogenous at the aggregate level, and that leads me to be somewhat worried about how I should interpret the results from the empirical estimation. My own conclusion would be that this reduced form relationship has to contain relatively little information about the shape of the theoretically-grounded consumption function, and therefore on the effect of interest rates on consumption or output.

Now, what is satisfied is the Euler equation in a framework like this, and the other equation here is sort of its linearized version, and this is basically saying that conditional on expected future consumption, current consumption declines with the relevant real interest rate. Now, we know—and this is the point that is emphasized in the paper and I fully agree with that—that this is far from being a panacea and the Euler equation approaches imposes very tight restrictions on consumption behavior that derive from intertemporal optimizing even over relatively short horizons, and we have a body of evidence that in this case is rejected in the data.

So, clearly this is not necessarily a better way to go, but it does not imply that the solved-out consumption function is necessarily a reasonable alternative especially in aggregate data. I think if you were to use it on microdata, on household data, then you could make a case that a lot of the right hand side variables might actually be predetermined or exogenous to the household. But at the aggregate level, I think this is an argument that is much more difficult to make. So, the literature instead has moved somewhat toward structural consumption rules that are coming out from solving the underlying intertemporal optimization problem and estimating the parameters of the consumer problem on household level data. This is much more complicated and often it does not allow us to have the kind of flexibility that I like in the way you approach the problem. You were able to sort of put a number of additional variables that we are interested in. As economists, we would like to how consumption responds to a bunch of

things, but again I am concerned about how to interpret those coefficients that are estimated in the empirical part of your paper.

Now, this is a problem also if we think about the effect of housing prices on aggregate consumption. If we think—this is the second result in the paper—that consumption does not seem to be increasing very much with house prices or housing wealth, and the argument that is there is that it makes it more difficult for renters to actually be able to buy a property and therefore, they have to save more. So, it makes housing less affordable to renters. If I think about renters who want to acquire property as being relatively young, there is an offset which is that everyone who owns a home ends up being richer and that is typically the older households in the economy. So, then again if we want to think about how everything closes, we want to think about the intergeneration linkages between young and old. Here you might say, well to the extent that older households transmit their wealth to younger generations when they pass away or when they do in vivo request, the young are also likely to be richer in present value and therefore it is not clear that necessarily an increase in house prices would make young households want to save more instead of less.

Now, in the context of Japan, I think there is a particular twist on this argument that maybe it is what is going on. I have no particular evidence whether it is a case or not but I am throwing it as part of the discussion. We can always make claims; we do not have to back them up. So, here I am going to say “Well, maybe it is a combination of increasing house prices which make older households richer relative to younger household, together with an increasing life expectancy or longevity that means that the time at which you expect to maybe receive these transfers from your parents is moving back into the future.” If, for instance, your parents are going to live forever, then clearly the fact that house prices go up is not making you richer. So, it is a combination maybe of the change in the demographics with the change in house prices that might be the right combination or the right way to think about this issue. So maybe that suggests looking at some sort of interaction terms, or looking at different consumption of different groups in the population and trying to see whether that kind of effect is there.

Finally...

Prof. Iwata: Excuse me. One or two minutes, please.

Prof. Gourinchas: Yes, I am almost done. So, one final comment is: what we learn from this, more broadly, about countries like the US and UK especially in the context of this liquidity trap that is looming upon us and that Japan has experienced in the last decade... insofar as we are interested in the experience of Japan with the liquidity trap, it is a little bit awkward to look at the interest rate channel of monetary policy only, and the reason for this is that, well, once the policy rate hits the zero-bound, in a sense, you do not have a direct interest rate channel anymore except through the manipulation of inflation expectations.

Now, that is an interesting way to think. A number of people argue that the way in which you want to conduct monetary policy in a liquidity trap is by manipulating expectations of inflation, and then I would suggest that perhaps one of the ways in which Japan might be different from the United States and United Kingdom is in the way these expectations are formed, and that would be an interesting thing maybe to add to the paper. Just like you have forecast equations for output, you could maybe think about the way in which expectations of inflation are formed. But otherwise, once you are at the zero-bound, in a sense it does not matter really very much, if expectations of inflations are going to be the same, how much transmission you are going to get from that to get a consumption for aggregate demand.

So concluding, a very fascinating topic. It is very, very relevant. It is a difficult question. On aggregate data, I think I would be a little bit cautious about the interpretation of the results and I would urge the authors to look beyond purely just the interest rate channel but maybe look at inflation expectations, or maybe even investment. Thank you.

Prof. Iwata: Okay, thank you very much. Do you have any quick response? John?

Prof. Muellbauer: Yes. Using a closed endowment economy used to analyze the Japanese economy does not seem tremendously appropriate. As far as the neoclassical monetary model of Woodford is concerned, I was interested to meet Jordi Gali at a recent business cycle conference. Gali is the other father of the 'new Keynesian' approach. He admitted that these models have nothing to say about the current crisis.. I think these models are also limited in analyzing the upswing, because they exclude financial accelerators and a bank balance sheet channel. They do not have inefficient housing markets or inefficient stock market.. Robert Barsky's paper suggests that models with rational expectations and efficient asset markets are really not entirely appropriate for analyzing Japan's recent history.

Of course, we accept that endogeneity is a big issue and a good deal of the paper deals with the issue. For example, one of the methods we use is co-integration analysis. We estimate co-integrated systems, and the evidence is perfectly clear that there is no issue of endogeneity bias: the parameter estimates for single equations are validated by the co-integration analysis. Secondly regarding the interest rate rule Pierre-Olivier was talking about, we get a stable and perfectly reasonable empirical interest rate rule, and perfectly sensible, Phillips curve-type inflation equations. Using these two instruments, for variables that he claims are so endogenous, the results are essentially unchanged. So, I am not sure what more we could have done in terms of handling the endogeneity issues if we are going to analyze aggregate data.

We recognize the merit of analyzing microdata. In Keiko's thesis for example, not only does she look at nondurable versus durable consumption, she also looks at the consumption behavior of different age groups using the many cross sections of household survey data available from Japan. One of the other things that she does in the thesis is to examine the issue of aggregation. The aggregate consumption function reflects the micro behavior of households. One of the questions is whether the common shocks for the economy as a whole are large enough to overcome the kind of noise that aggregation problems through heterogeneity might throw up. Her results suggest that using micro data with different assumptions on the propensities to spend varying by age and you aggregate up, our results are pretty robust. So, I do not think that the objection of aggregation bias is very serious. Thank you.

Prof. Iwata: The floor is open to the Q&A. Please pass it through Saito and then through Hoshi.

Prof. Takatoshi Ito, University of Tokyo: I think the micro data will give the clearer picture for some of the propositions. Theory predicts that there will be differential effects on housing and also real interest rates, whether you are a borrower or lender or you are in the first period, second period, whether you hold equities or bonds. So, those questions could be analyzed if that has been done. Probably you could cite those micro data results at least to back it up. The grain of the society, that is the period 2 people getting more in the weight in the economy, should have some implications; so changing the population weights of period 1 or period 2 would be very important if you are looking at it from the 1970s to 2000. So, that should be in somewhere tomorrow. So, it

is not just the two-period model but it has to be really overlapping generations with differential income growth and population growth.

When we talk about whether interest rates should be lowered or raised, this is the policy question back in, on the Hayami regime... Governor Hayami insisted that increasing interest rates will stimulate the consumption and be good for the economy and we vehemently to that thesis. So, these results may please Governor Hayami. So, if this is right, that real interest rates stimulate the consumption, that means the more deflation with zero bound would stimulate the consumption. So deflation is a good thing, and it is hard to believe. Even Governor Hayami, I think he was talking about nominal interest rates, not real interest rates, I think, and his argument was within the period 2 people, that higher interest rate will give the more income and that will stimulate the consumption. He was not thinking about the real interest rate. Why? Because the retired people in Japan face uncertain life spans and they are reluctant to spend down. So deflation in the rational expectations model the people should spend the principal, but if you have uncertain lifetime and money illusion, then you do not want to spend down the principle.

So, with nominal interest rates higher, then they get more stream of income they are willing to spend. I think that is what Governor Hayami was thinking. So, money illusion, uncertain lifespan would give probably the results that nominal interest rise will give the consumption to rise. So, uncertain lifetimes and sort of the reluctance to spend down either the financial wealth; or housing wealth may distort some consumption behavior in the period 2 people which is, I think, an interesting such topic.

Prof. Iwata: Yes, excuse me. The time is extremely limited, please make short the question, please.

Prof. Takeo Hoshi, UC San Diego: Okay, very briefly. I agree with Taka that you should mention more on the micro study result and according to John and Keiko has always studied this micro behavior and aggregated results. When you aggregate those micro consumption functions in the way of aggregation, the demographic structure of how many people are old and how many are young should matter for the aggregate consumption. I do not see that in your aggregate consumption functions specification, and I am wondering why. So, if you can explain that that will be good.

Prof. Anil Kashyap: I was going to make that Hayami point. I am actually very worried that the cabinet office is going to be accused of defending Hayami and the government. That is a somewhat serious question, so I think you could do two things that would help us out. First of all, it would be useful to actually estimate the Euler equation, just to show what the numbers are, to remind people that if you estimate, you get a substitution effect that is negative and so on, because the people that are crazy enough to think that raising interest rates stimulates the consumption will not understand that your results are given income and all these other stuff. So, unless there is some place in the paper that shows when you do not control for income and everything else that the usual way economists talk about this is right, you risk having your results hijacked to defend things that I do not think you would want them to, so you ought to preempt that.

Prof. Iwata: Okay, thank you very much. The time is already delayed so I would propose to squeeze the coffee break, but anyway, please respond.

Prof. Muellbauer

Regarding Taka's points and the policy debate: our paper makes it very clear that we think that output has the conventional response to interest rates in the aggregate. So, we are not saying "Raise interest rates in order to stimulate the economy." That is quite untrue. Our model is about consumption conditional on certain controls including income, income growth expectations and asset prices. . Regarding income, our view is that taking into account interest rate effects via investment and trade, the usual story emerges. Interest rates also have effects on financial asset prices, and therefore they have an effect on consumption in the conventional direction. So, in the aggregate, our view is that the conventional effects work as they do in the US and the UK, but they are far weaker. . It is not that the direction is different. Our point is just that they are weaker.

On the graying society, well, we certainly agree with the principle of the points made in the discussion. Why did not demography show up in the aggregate equation? That is a very interesting question. I think part of the answer is that while Japan is a graying society, with an increasing proportion of older people,, life expectancy is also increasing at the same time in a way that reflects Taka's point about the uncertain life span that households face. Now, these two things offset each other to a substantial degree. That is part of the reason why, even though we try very hard to find demographic effects in the aggregate equation, we could not find any. The other reason is that these equations are conditional on asset accumulation; and as Pierre-Olivier

pointed out, the right hand side variables in our consumption equation are endogenous, certainly in the long run. So the asset accumulation reflected on the right hand side of our conditional consumption model incorporates households' views on their life expectancy and the evolving demographic structure of the population. Demographic effects are therefore in the background of the model, which should be thought of as most relevant for horizons up to three or four years rather than about the very long run. Keiko, do you want to say something about the interest rate effects by age for which you have some interesting evidence?

Prof. Murata: It was a little bit sometime ago but when I wrote my D Phil thesis, I estimated consumption function basically following this similar formulation, but also by age groups. The real interest rate effect was higher for the aged households and lower for the younger households.

Prof. Iwata: Okay, then shall we finish this first paper? Excuse me for the delay of the first session. Now, we will move to the second paper. Prof. Dekle and Prof. Fukao will make the presentation. Please keep the time, 10 minutes.

Prof. Robert Dekle, University of Southern California: Thank you very much. I am going to present the paper and Kyoji is going to answer the questions. So, everyone knows this chart; the fluctuations of the Japanese yen since 1985. Now, in this paper we want to answer three questions. We will try to answer three questions the best we can. What was the impact of the rapid yen appreciation on Japanese industries? That is question one. Question two: was the yen exchange rate appreciation match by productivity improvements so that the ability of Japanese industry to compete with the US industries was not diminished? The third question: was the yen appreciation related in some systematic way to the sectoral shifts in employment in the Japanese economy? Now, we added this third section because we were asked by the editors and others to relate the relative production average costs at the industry level to economic outcomes, so we decided to focus on the employment shares and the changes in the sectoral employment shares in the Japanese economy.

So the first question, the impact of the yen appreciation on Japanese industries. So, in most industries after 1985, the average cost of Japanese firms rose relative to that of US firms. In terms of US dollars, the gap was largest in 1995. Since 1995, the gap in average production cost between US and Japanese firms narrowed, owing to US wage

increases which raised US costs and also in some industries, rapid Japanese relative productivity growth improvements.

So, in the analysis at the industry level, we looked at 14 industries, manufacturing industries at the industry level, and we divided them into the high and low productivity Japanese industries. So, in the high productivity industries where TFP growth was robust relative to the US prior to the 1990s, the gap in Japan-US average costs did not widen as much after the yen appreciation. So, these are the high productivity industries. In Japanese industries in which TFP growth was not robust prior to the 1990s, the gap in Japan-US average costs did widen substantially after the yen appreciation. So, these are low productivity industries.

This is the panel of TFP growth comparing Japanese and the US TFP growth between... the first, Panel A, is 1980 to 1990 and the second, Panel B, is 1990 to 2004. We can see from the top panel that Japanese TFP growth has beaten the US TFP growth in several of these industries like chemicals and optics, and these are the industries that have had relative success since the yen appreciation to keep their average costs not rising very much relative to the US industries. So, the high productivity industries we define as optics, general machinery, transport equipment and some others; the low productivity manufacturing industries are defined as food, beverage and tobacco, pulp and paper, rubber and plastics and some others. So, for space reasons, we do not present these charts for all these industries, but we just present some selective ones. So, you can see generally that Japanese average cost, which is that line that shoots up, really shot up after 1985 and then after 1995, and then it is approaching the US average costs after 1995.

But in low productivity industries such as rubber and plastics and apparel and footwear, the gap after the yen appreciation, the gap in dollar terms and average cost between the US and the Japanese firms has remained large. But in these high productivity industries such as transport equipment, by 2003, Japanese average costs have returned back to the level of the US average, production average cost. So, there is big return to competitiveness in these high productivity industries. So, this is another one: chemical and the chemical products where average costs have converged back after the yen appreciation.

So, just some quick patterns, the average cost in terms of yen, they trended along with the US average costs, but in terms of dollars, Japanese average costs really increased

and this has meant large cost disadvantages of Japanese industries relative to the US industries. But since 1995 after the rapid growth in the US wages, there has been a convergence of Japan and US average costs, and especially in the high productivity industries where TFP growth in these industries were robust. In low productivity industries, convergence was a lot slower and only narrowed after the faster US wage growth.

So, the answer to question number one. Question number two, are exchange rate movements matched by Japan-US relative productivity improvements? Clearly, if you look at these industry by industry comparisons, certainly no; in some low productivity industries, by industry-industry comparisons, the exchange rate movements have not been matched by Japan-US relative productivity improvements. If they are not matched, then what should the very long-run real exchange rate be to be consistent with Japanese and US productivity patterns over time?

We used this model to also look at the sectoral transformation of the Japanese economy, but to determine long-run equilibrium real exchange rates, we need a model. So we built a very simple, sequential, static, two-country, three-sector macroeconomic trade model with three industries: high productivity manufacturing and low productivity manufacturing—because as we have seen with this industry-level study, this heterogeneity has really mattered—and services. We see that the model is entirely driven by labor productivity levels and growth in Japan and the US. One way to think about it is putting numbers to Obstfeld and Rogoff, Chapter 4, and Matsuyama 2008.

So, we aggregated the 19 industries from KLEMS using the cost comparisons above into low productivity, high productivity and services. This is the equilibrium real exchange rate we get from this model. So, the top red line is 1981 set as one for all three series, and the top red line is the actual US dollar-yen Japanese exchange rate, and the dotted line is the model simulated US-Japan real exchange rate, and the green one... we just took the Summers and Heston PPP real exchange rate from the Summers and Heston database. What we see here is that by 2003, we see at first a very strong Balassa-Samuelson effect in the model, and that by 2003, the Japanese real exchange rate is basically close to equilibrium, but of course there was undervaluation of the yen until 1985, and then an overvaluation of the yen since 1985, which was severely accentuated in 1995, but by 2003, if you just have a simple model that is driven by US and Japan productivity differences and also different sectoral productivity growth rates

including different productivity growth rate and services, this is sort of what you get from the model.

So, measuring the yen misalignment, yen is always overvalued compared to the Summers-Heston purchasing power parity which has the yen purchasing power parity exchange rate of about 220 yen per dollar even now, but according to our model simulations, by 2003 the yen does not appear to be overvalued. Just as an aside, I was in Japan this summer and for the first time in 30 years, I thought things were not that expensive. If I go now, I think I would find things expensive, but not this summer. The model has strong Balassa-Samuelson effects.

The last question: yen appreciation and sectoral labor transformation. How is the yen appreciation related to the expansion of the services sector and the decline of the manufacturing sector in Japan? Both the yen, the real exchange rate and the sectoral transformation are endogenous variables driven by productivity, so in a sense it is not a direct relationship between the yen appreciation and the changes; just, both are outcomes of this productivity difference process. So in terms of our model, this is Japan-US average cost, and the red line is the Japan-US average cost in the data, and the dotted line is the Japan-US average cost which was simulated by the model. Now, in this as we see, in the low productivity industries, because the lower productivity cannot really hold down Japanese average cost increases, the relative average cost of Japan and the US increase in the model, but if you look at the high productivity industries, the very high productivity, the higher productivity in Japan really holds down the rise in average costs of these industries, so it is rather flat according to the model. Now, of course, let me just make an aside that we have no money or price rigidity in this model, so we are going to miss almost entirely the short run fluctuations. So, this should really be viewed as a very sort of... just looking at trends of very long run of how things should converge.

Now, this is what I find very interesting or most interesting. It is: what the model can say about the changes in the sectoral employment shares in Japan? So, the red line is the data and the dotted line is the model, and in the data the low productivity manufacturing share has rapidly declined. The high productivity manufacturing share decline has been relatively more muted. But the services sector has rapidly increased. And the model matches the employment share of the high productivity sector fairly well, but we really miss the employment share of the changes in the employment share of the low productivity sector and the employment share of the services sector. So, what we are

missing is the shift in employment over the long run from low productivity manufacturing to services. What is useful is that we can kind of investigate what is really going on. What are we missing? What is missing in the model that we cannot explain this? So, we captured the decline in high productivity manufacturing in Japan very well, but we missed the movement of labor from low productivity manufacturing to the services sector. What is going on is that in these structural transformation models, if you have high productivity, you do not need as much labor, so you are kicking out labor from that sector to the other sector, right? So, we just need much more higher productivity growth in the data and low productivity manufacturing to drive out the workers from that sector to services, or another thing, or falling US prices in the sector so that the Japanese demand will shift to the sector. I will talk about one other, I think, important factor that Kyoji and I were talking about.

Similar lack of success of the model for the US is a two-country model, so we catch the high productivity sector but completely miss the low productivity manufacturing sector and the shift from labor from low productivity to the services sector in the US.

What is really missing in the model to explain the movement from low productivity manufacturing to services? There are several things we thought about and we have some of these already in the model. But there is a decline in housework by men and women, so the rising procurement of market services over time—for example, elderly care, childcare, food bought at convenience stores—if all this was done at home and that has shifted to market procurement that would have the effect of appreciating the real exchange rate and accentuating the shift in people from the low productivity manufacturing sector to the services sector.

Another related factor is the demand composition effect in the 1990s. There was big government spending in Japan that fell mostly on non-trade and good services. At subcontracting at Japanese firms, they use to do all the cleaning, for example, in-house but now that has been subcontracted to the services sector. So, these factors will tend to raise the service sector employment demand, so these are demand composition effects, and may explain the shift from low productivity manufacturing to high productivity manufacturing.

Last is the rise of... we only have a two country model so the third trading partner, China and India competing in low productivity manufacturing. Because you see this phenomena in the US and Japan and all over Europe. So, the exchange rate

misalignment... yet, can the yen overvaluation explain the shift in employment? We do not think so. Exchange rate misalignment cannot explain it, because while the yen was overvalued versus the dollar, the dollar was undervalued versus the yen, but both witnessed the same sectoral shift out of low productivity manufacturing. So, it is a long-run global phenomenon. Anyway, thank you for your time.

Prof. Iwata: Thank you very much. Prof. Harrison?

Prof. Ann Harrison, UC Berkeley and NBER: Thank you very much for inviting me, David, to come and give comments on this very interesting paper. So, let me just briefly summarize for you the goals of this paper which Prof. Dekle did very well.

First, the goal is to identify the effect of the sustained appreciation of the yen on the gap between US and Japanese average cost primarily in manufacturing, that focuses on the manufacturing sector here. The second goal is to show that despite the adverse consequences for cost, some sectors were able to offset the effects depreciation through productivity increases, such as, for example the automotive sector or telecommunication sector. Then finally, the other goal of the paper is to try to understand how productivity changes, exchange rate changes, the consequences for rising cost in Japan actually led to the perceived employment shifts out of manufacturing and into services. So, those are the three ambitious goals of this paper.

So, my comments are sort of threefold. I have three sets of comments. First part, there are very strong assumptions made in both the data and the model: perfect competition, constant returns to scale—finally, the models assume that productivity is exogenous, it sort of drops from the sky. Second set of comment is how we actually measure the effects of exchange rate changes on competitiveness. Then my third set of comment will deal with sort of the potential importance of imported intermediate inputs, and then the model predictions versus the reality. I have actually something to say about where I think the explanation lies for this enormous shift out of low productivity employment into services. I think I might be able to provide some ideas on that.

So, first let me start with the assumptions in this model. First assumption is exogenous productivity growth which... I understand from Pierre-Olivier the typical assumption in macro, but in micro where I work, one spends ones life trying to understand and determine the productivity growth, both within firms, across sectors and at the country level. So, that is a very strong assumption. Clearly, the same critic was made in the last

paper, productivity growth, exchange rate changes are all jointly determined in some simultaneous sense. So, that is one assumption that bothers me.

Secondly is this assumption of perfect competition. So, that is particularly problematic when trying to understand the transmission of exchange rate changes to cost in general, because one would believe based on many studies including some by Prof. Dekle himself that there is a lot of pricing to market implying incomplete pass-through. Then third, I would not say anything about this, but there is another assumption which is constant returns to scale, which also affects the missed measurement of total factor productivity.

I do not know how well you can see this, maybe you cannot really see it. But this is taken from Jorgenson and Nomura who have number of papers looking at comparing productivity between the United States and Japan. So let us just focus if you can see it on the motor vehicle sector. So, this is Japan. Japan is going up, and this is the US. US is not just flat but in the early 1982s, productivity in the US in this motor vehicle industry fell. So, at this point in 2008, we totally understand why GM needs a bailout because GM is down here and Japan is up here. Now, you can see that what happened when the US started to have problems, the US auto industry begged for assistance and got voluntary export restraints. Starting about here, you will start to see... now of course if we blew this up; we would see enormous falls in productivity in the United States. Why am I telling you this? Because clearly the path of productivity change is endogenous and it is very much a function of trade, restrictions, institutional factors, all sorts of things.

Another sector—if you can see it. I do not know if you can—but this one is really striking. This is the computer sector. So, the US productivity in computers is going through the roof, and look what is happening in the productivity in Japan. This is a tenfold difference, if you really believe Jorgenson and Nomura, which are pretty well known guys. But I mean this is really an amazing difference. They talk about why there was this difference and this difference—we will come back to this, what we are trying to understand, the shift of workers out of low productivity manufacturing into services. So, where was I?

So, the point is that TFP is affected by many factors, right? It is affected by market structure, the degree of international competition, I just wanted to highlight the role of information technology, and the difference in prices of information technology will be

difficult. So, it is hard for me to accept that class industry variation in TFP is exogenous, or even that country-level TFP is exogenously determined.

Second assumption, the paper assumes perfect competition. This is a problem in several dimensions. First, it is a problem because I am guessing that in a number of sectors, firms were able to offset rising costs by squeezing excess profits or by pricing the market. You cannot have pricing the market if there is perfect competition. Secondly, I will not go into this very much, but I have an early paper which shows that observed productivity which is here, which is  $\mu$ , is a function of mark-ups plus actual productivity which is  $dA/A$ , and basically what happens is when you have upwards procyclical periods, then observed productivity is going to be higher because of this mark-up term relative to actual productivity, and then in bursts observed productivity is actually going to be too low as mark-ups fall.

So, TFP is very much affected by the business cycle, by changes in mark-ups, etc., etc., and this is a problem both when you have enormous swings in the real economy, and also when you believe that there are different degrees of imperfect competition both across industries and over time. So, that was the whole first set of questioning of the assumptions and model.

Second set of comments has to do with how you actually want to measure the effects of so-called exogenous changes in productivity or exchange rate movements on relative cost. Well, the problem is that it is really hard to get a sense of what is important just by what I call “eyeballing” the data. What I mean is just sort of looking at those figures that you saw, how could we really figure out what is going on? I really cannot do it. So, I would suggest, why not use a relative cost definition with the paper? You cannot really see it here, but you can decompose class into the different factor prices, into factor shares, into changes in productivity, that is this term over here, and you could just do a simple decomposition and that way at least tell us something, or you could use regression analysis. You could use vector autoregressions. You could use some of the techniques that Prof. Muellbauer and his coauthor use. That is not to say that is going to solve the endogeneity problem. However, it would get us a little bit further into understanding the links. Now, if the authors do this, there is a problem circularity, because factor shares to determine the degree of TFP, they also determine cost, and so you might get actually a mechanical relationship because of the joint determination of the TFP and cost. So it is not that easy, but it would be worth doing.

Other issues; I am really interested in this. I said that there is pricing the market. There could also be extensive off shoring or importing of inputs, which would offset the negative effects of exchange rates on cost. Take Japanese auto industry; with the appreciation of the yen, they did not all move here but many firms moved here, and that offset the appreciations. So, that is something that is not taken into account. There is a number of satellite countries all around Japan that play a major role in providing cheaper inputs, and have received offshore foreign investment. Prof. Dekle himself has done work in this area. I am a little concerned about the simulations; that, although the authors state that their simulations work well, when I look at these, it is not clear to me that the figures that we saw, the figures nine and ten indicate the model is working well.

So, let me just keep going. The model only has one input, labor. I believe that the shift that we are seeing out of low productivity manufacturing into services is very much a function of falling prices for investment goods, and so when you only have one factor of production, you cannot actually capture that in the model. So, let me talk a little bit in the time that I have left for what I think is the interesting puzzle. This is a picture that you already saw, right? So, the puzzle is that the model predicts flat employment in manufacturing both in Japan and in the US. I think this is Japan, the top thing is Japan, the bottom is Japan. So, if you believe the model, there is no movement out of low productivity manufacturing in the services. It is pretty much slack. But in reality, there is a shift of employment out of low productivity manufacturing and it is all moving into services. Exactly the same thing is happening in the United States except if you can sort of eyeball it—which I am saying is not a good idea but in this case, we will do that—you can see that the slope is deeper in the US than it is in Japan. So, whatever is happening in Japan, it is happening even more in the United States. In the last five years, the manufacturing sector lost five million workers which has been a big issue.

So, why is this going on and what do we think is causing this, that the model does not pick up? Obviously as Prof. Dekle pointed out, it is not the exchange rate since it is happening in both countries. So, I have a solution. Let us see, where is my solution? Okay, so hypothesis number one. There is a really important role for information technology as the falling price of investment goods leads firms to substitute computers for either routine workers or unskilled labor. Now, remember what we saw that was going on in the United States, enormous increase in productivity in computers, a huge fall in the price of computers in the United States. Similar in Japan, but much smaller, which could explain the different slopes.

Hypothesis number two, increase the offshoring of low-skill, “routine” jobs. Pierre created a model for you so I am going to give you some empirical results. I do not know if you could see this, but this is only for the US, not for Japan. But for the United States, we can actually understand why these five million workers are leaving manufacturing—actually over the last 15 years it is more like 10 million workers. First of all, it is not productivity. Total factor productivity explains zero about the shift out of manufacturing, so we do not need to worry about that. What is going on, and this here, your hunch was absolutely correct, is that increased offshoring to low-income countries is leading to a reduction in manufacturing and employment. You are seeing these negative numbers. But it is not a big effect.

What the really big effect is, is this IT effect, a lot of the real price of investment. As the price of investment goods i.e. computer has fallen, people have replaced low-skilled workers with machines and you can see it because you do not see it for advanced degrees, right? But you see this huge effect for high school and college. As the price of investment goods has fallen, then since it is a positive coefficient, the manufacturing sector is using fewer of these unskilled workers. So, that is my contribution here.

So, let me just conclude. So this is a really interesting paper on a fascinating topic. I would urge the authors to go further and calculate the contribution of different factors, so do a decomposition kind of like growth decomposition but do a cost decomposition just as a starting point. You could also estimate sort of vector autoregression models and calculate. I think you should explore the robustness of your results to your assumptions of perfect competition. Finally, I think it would be very interesting to investigate the relative importance of falling computer prices and falling prices of investment goods for cost savings and low skilled manufacturing, both in US and Japan, as well as the role of what we call pricing to market, an important intermediate which can explain how firms have offset exchange rate changes.

Prof. Iwata: Thank you very much. A quick response, yes please.

Prof. Kyoji Fukao, Hitotsubashi University: First of all, the decomposition. Yes, we can do that and actually, we have done that. One problem is because of the nonlinearity of the cost function, the summing up of the contributions is not always equal to 100%—say, 20% larger or something like that happens. That is one reason. And the other reasons are intermediate inputs also depends on exchange rate and the US is not the only foreign country. So, the story becomes very complicated. The exchange rate

rate affects Japan's import price and it contributes to the change of production cost. So, we skipped that issue but we can provide the decomposition. We have already prepared it.

Second issue is perfect competition. Actually, for example, in the first part, the relative competitiveness calculation part, the perfect competition assumption, we do not need. Of course when we calculate, we need perfect competition in capital markets but in output markets, we do not need that assumption. Of course, to calculate TFP, if we use income share, then the issue you raise exists; but in the case of, for example, Japan, we also calculated using the cost share. If we use cost share here, then the TFP is not biased even under imperfect competition.

Prof. Harrison: Yes, it is biased, because the cost share reflects...

Prof. Fukao: No, no. Okay, let us talk that later. I wrote a paper on that issue so I can show that and the third point is that... while we cannot explain the very rapid decline of the low productivity manufacturing sector, I think one reason, we can take one approach to answer that question is to check the demand structure. We have a very detailed IO table for each sector. So, from the viewpoint of real demand or nominal demand, what is the factor that changed? And we found, net export change substantially, but also the composition of consumption and government expenditure and the investment played a very important role; especially not the composition in consumption or investment, but in 1990s, Japan's investment declined very rapidly and the consumption and the government expenditure share in total expenditure increased a lot. Investment contains much more manufactured products than services and the government expenditure or the consumption more goes to the non-tradable sector, and that is I think why. Our model does not take account of that kind of change of composition of expenditure, and I think that is one reason why we cannot explain the movement.

Prof. Iwata: Thank you. Discussions, we have Prof. Ito and Anil, do you have? Hoshi, please yes.

Prof. Ito: We tend to think that there are two kinds of service sector; one is high productivity and the other one is low productivity. So, it may be impossible but if you could make it a four sector model that would be great. We tend to think that the US has... as I mentioned the use of the computer has been far more pervasive than Japan. Not only the IT sector but the IT-using sector made the difference, and that was the high

productivity growth in the US and not so widely used in Japan. So, my sense is that Japan shifted the low productivity manufacturing to low productivity services, and US is low productivity to high productivity service sector. That explains sort of the reversal of the fortunes in the second half of the 1990s.

Prof. Iwata: Yes, then we will have the accumulation of the questions; then Prof. Lane?

Prof. Philip Lane, Trinity College Dublin: There is a very large literature comparing productivity in the Europe to productivity in the US. It could interesting if you do this on the model, at least in terms of the literature view is the US, Europe literature giving similar types of lessons to the US and Japan. Also, thinking about the composition of employment across sectors. I did not notice too much about different immigration patterns. Clearly for the US, with the massive amount of immigration of Mexicans and so on, and the expansion of low skilled service sectors is related to that. Third, coming back to the big issue of financial problems in Japan. One of the rules of the financial business is to reallocate resources from contracting sectors towards expanding sectors and it will be really interesting if there are any evidence of sectors which were financially constrained where inhibited from expanding employment at rather manufacturer's maybe whether or more of those firms and so on. So, maybe it is possible with the KLEMS data and using in the scenes of financial constraints across different sectors to get something there.

Prof. Iwata: Okay then. Please, next, Prof. Ariga and then Prof. Kato.

Prof. Kenn Ariga, Kyoto University: Okay, I have one simple question and one comment. One simple question has to do with one of the figures that shows the Japan-US on the ground of real exchange rate with red line that is actual figure and the blue line, the model prediction and the one in green line that was...yes, Summers and Heston the PPP. It seems that all of them start out with more in the same level at 1980. Is this sort of normalization? So, then it should be incorrect to say a varieties over value and...so, that is not the correct interpretation. Only the relative movement can be inferred, not the absolute level, right?

Prof. Fukao: Yes, you are right.

Prof. Iwata: Finally, you should respond then. Please continue.

Prof. Ariga: Okay, now I am cleared. About the discussant's comment on the impact of computer, IT use on the possible substitution effect of those skilled workers, I think they are real and now we have several important studies, micro studies for the use of computer and IT using Japanese sample service. I recommend that also to refer to those studies but I am not quite sure whether that could be good enough to differentiate the possible impact like the use in low skilled manufacturing versus the similar impact on the service industry. My suspicion is that the impact is probably very different but I am not sure whether there is any study pointing strongly to that effect. But that could be one part of an important story.

Prof. Iwata: Okay, thank you. Then Prof. Kato and Prof. Hamada, please.

Prof. Takao Kato, Colgate University: Yes. I find this differential movement of wage rates during this 1990s, especially late 1990s and 2000 between Japan and US very intriguing. So, the US is just kind of really surging, and then Japan kind of starts... kept it at the constant or even decline and that is really intriguing and interesting so I just want to know more about the wage rates and it is total labor compensation divided by the quality or just the labor input, as I understand. So, which part is really making the Japanese wage rate so constant and stagnant? Is it really the labor was so understanding and that they just kind of, that is my kind of personal impression but do they really restrain the wage demand or is something else, the use of the non standard employment which changed the quality adjusted labor input or number one was work or the actual? What about the healthcare cost and fringe benefits and all those benefits changes? So, a lot of the interesting questions about this wage rate—the gap between US and Japan during this time period. So, if you could just shed some light on that, I would really appreciate it. Thank you.

Prof. Iwata: Okay, thank you. Then Prof. Hamada?

Prof. Koichi Hamada, Yale University: I enjoyed the discussion by the commentators. But my question is macroeconomic borrowing, Prof. Ariga's comment. The usual procedure of macroeconomics now is to assume some kind of current account equilibrium and to start from that. So, you are starting from 80, you may have some theory that 1980 has some normal situation. However, if government spending different with population growing differently and productivities into the future different and the rate of time preference are different, then there can be various possibilities of finding

some “normal” current account balances. So, the real exchange rate should be considered from that time.

Prof. Iwata: Okay then I think I have number of questions to be answered so please.

Prof. Fukao: Takao’s comment about high productive service and low productive service and also ICT using issue and it is related to Ariga’s comment and also it is related with Ann’s comment about endogeneity of TFP, which we cannot do here. We will mention preceding researches and we know that in Japan, the ICT investment in service sector, such as commerce and eating and drinking place, is stagnated substantially and we will refer preceding works and the US /Europe comparison, within European Union KLEMS project, we are member of the European Union KLEMS project and there are some researchers going on so we will try to refer to these works on Japan/US /Europe comparison.

Financial constraint, if Hoshi-san or Kashyap provide us industry-level data, for example, percentage of the zombie firms’ activities or something like that then yes, probably we can mention about it. Kato-san’s comment about wage rate, yes, our wage index is total labor cost over quality index of labor and in the case of US, it is provided by Jorgenson and in the case of Japan, our Japan industrial productivity database team provides the data. So, we can check what is going on behind these data, we will do this.

Prof. Harrison: [*inaudible*]

Prof. Kato: They just went along with the US, so some interesting things going on, so really...

Prof. Fukao: For example, part-time workers increase and labor quality decline substantially; for example, yes, we can check that.

Prof. Dekle: Yes, just on Prof. Hamada’s comment. It is really hard to do two country intertemporal models because they do not quite fit the data, but I do take your point very well. Now, the thing is, I agree that the real exchange rate maybe affected but the allocation, the allocation of labor among the three sectors and allocation problem... So, I do not know if the current account is really going to affect that because that is like the central plan... allocation across the three centers. But I have to think about that some more.

Prof. Hamada: If you have over time considerations then data macro...as well.

Prof. Iwata: So, now the session three is over. I am sorry for the delay of this session three. Somewhat, I would like to propose squeeze coffee break. We are going to start again the Session 4 at 11:10, please.

[*break*]

Prof. David Weinstein, Columbia University: Okay. Why do not we start the next session and try to keep things moving along? We may eat a little bit into the lunchtime period when we try to keep things, try to kind of be tough on everybody. So, remember presenters have 10 minutes, discussants 15 and we will try to keep it, I will try to keep you on time, okay? So, our first paper is Takeo Hoshi, Ulrike Schaeede and Satoshi Koibuchi, and I guess Ulrike will be doing the presentation. I will turn it over to you.

Prof. Ulrike Schaeede, UC San Diego: All right, thank you. First I want to apologize for the environment. I had painstakingly put together some black and white PowerPoints for handouts. I am sorry, that was going to be printed but I hope that world will survive. I am going to present a joint work with Takeo Hoshi, Satoshi and we will do like Kyoji and Bob, please direct all the compliments at me and all the questions at them.

So, what we are doing here is we are looking at changes in corporate restructuring over this period between 1981 and 2007. We have this from good research that many of you have contributed to. We have a stereotypical image of corporate restructuring processes in postwar period when the main bank would step into the distressed company and dispatch directors, push management to restructure debt, and the main bank would maybe organize some debt forgiveness sessions and sales of assets were primarily done or principally done to generate cash. We know that these processes were mostly informal and we are also having a quite good understanding about the economic interests of the bank. It had a high exposure as the largest creditor and owner and also it had to uphold its reputation and reciprocal delegated monitoring. There was also some politics going on as we now we know from General Motors, the difference in Japan being that the government actually asks the bank to do the bailout and then would give rewards and put pressure on banks to do these restructuring.

All right, so we have this image of the postwar period but the two main things have happened that might affect these processes. The first is of course financial deregulation which started in the 1980s and initially was skewed towards deregulating large firm financing first, and what that meant was that the savers of the banks were stuck were the large firms wounded up so banks change their client pool towards smallest firms that had real estate at collateral and then the burst of the bubble, of course, caused problems with that loan portfolio. This was followed in 1998 but “Big Bang” financial deregulation when in particular, I want to highlight, accounting and disclosure reforms that greatly improved the quality of information we have on companies and so, the general public could now better evaluate companies’ financial situation.

And this was then followed by Heizo Takenaka’s 2002 financial revival program which change, which turn politics on it said that was now push towards direct loan write-offs, forget all these 10 year wait-and-see. We need some action and there is no longer too big to fail so, a large number of changes on the financial side.

Also, and this was the second stream of change that might affect corporate restructuring is what I have and other research called Strategic Inflection Point on the side of the companies and this materialized in the series of legal reforms of the commercial code beginning in 1997, 1998 that culminated in the 2006 Corporation Law and for the purposes here, what is important about those changes is one, there were legal changes that allowed companies to more flexibly reorganize spin-offs, layoffs, form new companies, reorganize into holding companies; all kinds of new things that were previously very cumbersome. And also, the financial means by which large firms could reorganize change. It was easier to use stock swaps and conduct mergers and accounting reforms, all signs, all sorts of good things. There were other laws that also changed in that period. The 1998 revision of the Foreign Exchange Act made it easier for foreigners to invest in Japan, so now you only have to report if you drop a shipload of cash in Tokyo so you could actually buy a Japanese company if you want it to. The 2004 Labor Standard Law changed made it easier to adjust employment and there were also important new laws or the old laws on bankruptcies were revised and so there were new ways for companies to just basically exit, liquidate, file for Chapter 11, all kinds of good things.

These things we hypothesized might affect corporate restructuring in four main ways. One is given the change in main bank relationship, it is possible that banks just play a different role when they have a distressed client in the hands. The new transparency in

accounting might invite other players like private equity funds, just launch a hostile takeover of a distressed company. The greater flexibility in corporate reorganization might actually change the way companies want to reposition themselves on the market and finally, of course, bankruptcy laws introduced new viable options, not only for the banks, but also for the companies that are no longer as dependent on banks savers.

So, what are we interested in this project is whether these kinds of changes have really occurred and then so what we know. So, we want to know whether these processes of corporate restructuring have changed and the way we are going to go about this is we have built a database on “restructuring” events. I would say a little bit more about it in just a second. We then going to link those data to financial data and identify changes over time in the likelihood that a distressed company will undergo restructuring, and the means and processes by which this restructuring occurs.

Here is our database. What we did is we hired a whole army of research assistance to help us with the following task. We searched the Nikkei Telecom database which is a newspaper database for Nikkei, for the four big Nikkei newspapers for the word *saiken* which is Japanese for restructuring for by listed firms on the period of 1981 to 2007. At this point, we have odd years only, so we have 14 years of observations and we understand that our articles represent the lower bound of true frequency but between the researches, we have substantial dedication to more than 20 years of research on Japanese business. We have eyeballed our database and we are confident that we have all the big cases in our database. We code main measures of restructuring such as with management displaced, finance where assets sold, with labor adjusted, salary adjusted, what kind of legal processes were used and who led the restructuring event, and then we match this information with financial information.

We have a total of 1610 episodes of restructuring identified in these 14 observation years; for purposes of identifying changes over time, we divide the period at four sub periods, our entire period which we have labeled for convenience purpose, pre-bubble and bubble recession period of the 1990s and then the strategic inflection which is the last period from 1999 to 2007. We use as a marker of distress two different measures and we want all of our specifications with both of these to find little substantial differences. So the stress is either having an interest coverage ratio below one for the previous two years or negative net profits for the previous two years.

Let me just give you some visuals. We were still working on this and working on our database but basically just for you to eyeball this, when we use negative net profits as a distress variable, we find some sort of trend so it seems that the distressed company, this is the ratio of distressed companies that undergo restructuring. There seems to be a decline. If we use ICR, we will find more variation. So, there is both something over time happening as well as something happening year to year that we can now play with. Also, this visual, let me show you how distressed firms and distressed firms undergoing restructuring different in terms of employment adjustment. We see that firms undergoing restructuring reduce employment more than other distressed firms and same for debt reduction, those undergoing restructuring reduce debt more. I am most likely going to run out of time, so let me give you our findings first and then I will introduce some of the data analysis.

We find that our restructuring variable is a real event so firms that identify undergoing *saiken* are different in their measures. The likelihood of firms in distressed to undergo restructuring seems to have decreased over time and I will show you this in just a moment and the third finding we have is that it does make a difference who leads the restructuring event as banks and private equity funds appear to be more aggressive in reducing debts and loans. So, let me talk a little bit about the likelihood of the restructuring event to happen so this is our first analysis. Our dependent variable here is *saiken* which is the company undergoing *saiken* as identified by a newspaper article. We have as independent variables financial distress, dependence on bank borrowing which we measure as total debt to total asset, the size of the company. You would assume that might have an effect. Then we also look at the history of restructuring which is log and you would see whether this is control for some zombies if you wish or whether these events occur for a longer period of time and we also are interested in dependence on the main bank which we measure as loans on the largest lender over total bank loans.

Okay, so here is our base specification and what we find is that yes, high bank debt increases the likelihood of restructuring as does distressed as does size. When we then in the second specification look at whether this company was undergoing restructuring two years ago that is also important. Industry dummies do not change the picture so we are happy about that and what we find here and I will say a little bit more about this in just a second, what we find in this base, in this first line of specifications is that main bank dependence does not seem to increase the likelihood of undergoing restructuring for this entire period.

All right, so now we were interested in change over time and the first thing we look at is, and again so this is the likelihood that a company will undergo restructuring. We find that it is negative over time that is that if we look at the first period as the base here, we find that by period four, the likelihood that a distressed company will undergo restructuring has declined. We also find that debt to asset ratio is while important, it is becoming less important, and the main bank dependence—and this is very interesting—had an impact on the likelihood of restructuring in the early periods but over time, has declined. So, as of the 2000s, whether a company has a main bank or not is no longer increasing the likelihood of this company undergoing restructuring.

All right, these are all first findings, distressed firms are more likely to undergo restructuring when they have high bank dependence and our large tendency has declined.

Before David will cut me off, maybe I can give you a little bit of flavor for the processes of restructuring, and here are the exercise is to look at employment adjustment, capital growth, growth in bank loans and growth in total debt, and see what we can learn about that. What we find is that for employment adjustment, distressed companies reduce employment and distressed companies that are undergoing restructuring reduce employment even more. That makes perfect sense, so we are happy. So, this is the interaction and that is also great. What we find is here, what that says is that restructuring consistently leads to reduced employment. So, we find big effects of statistically significant differences between in terms of employment adjustment by firms undergoing restructuring.

In terms of capital growth, the story is somewhat similar. Distress means reduced investments, and undergoing restructuring means reduced investment, and both mean even more reduced investment. Here, interestingly, in the period three dummy which is in 1990s, we find that capital growth may... that restructuring means less reduction of investment over time. So, that is an interesting finding. We are still trying to figure out how exactly are going to tell that story.

Growth in bank loans—and this is of course something that many of you have contributed to in the past as there is this whole question about whether restructuring automatically means that bank loans will be reduced and I think Joe will tell us a little bit more about this in the later session. Fundamentally, we do not find an effect, and our interpretation here is that this can go either way; that in some cases, a bank might want to infuse more money to help, and in some cases it might want to reduce outstanding bank loans to

reform the company. So, we think, let us just watch. And then we will do the same thing with debt. Here, and I am out of time so I am going to run you through this: companies that are undergoing restructuring are reducing debt, and that does not seem to be changed over time. And I will just going to summarize for you what we do... finally, you can read the paper yourself. We actually looked at whether it makes a difference with our bank leads or private equity fund leads. What we find is that banks and private equity funds seem to be more aggressive in pushing reduced bank loans and reduced debt. So, there is a difference. That then leads us to do some more exercises on what we can learn about bank-led restructuring. What we find—and there are some interesting stuff here and we can maybe discuss and the discussant might want to pick this up—but I want to just stop on the main bank dependency here... There is quite some effect that we see—the high exposure to one bank does not increase the probability that a bank will lead a restructuring. Okay, so with that, here are my overall findings, and I look forward to the discussion.

Prof. Francisco Perez-Gonzalez, Stanford University, GSB and NBER: Thank you to the organizers for inviting me to discuss this paper. As the presenter describes, this paper talks about changes in corporate restructuring in Japan between 1981 and 2007. So, let me start with a disclaimer. I basically know nothing about Japan, so I did tell Anil when he invited me to discuss this paper that that was a risk. On the other hand, it was to me a great opportunity to learn about the massive restructuring, and of course we are super interested in these topics in light of what is happening in the US institutionally, and broadly interesting in governance restructuring in emerging economies, economic reform, so it is a fantastic topic and for me, a great opportunity to revisit and look at this literature.

To just summarize what the paper does, the paper has the objectives of looking at how restructuring has evolved over time. As the presenter illustrated, there has been a fair amount of economic reform going on in Japan or attempts to reform which might affect the way Japanese companies reform. They have a particular interest in the main bank system, which from the literature we are expecting to be quite important.

All right, the data in the paper comes from publicly traded firms between again 1981 and 2007; the financial information comes from those sources. The paper defines firms... basically kind of puts firms in two pockets: distressed firms are those that have coverage ratios of less than one for a couple of years, so negative income for couple of years that is negative. They compare distress firms relative to restructuring firms, and

the way they identify restructuring firms is by looking at the news media and looking at which firms had actually restructuring.

Okay, the methodology is quite simple. They are just comparing the restructuring firms to those that are distressed and they compare alternative leaders in different time periods, for example, to others. Okay, the main finding is that restructuring leads to significant declines in employment, capital and debt—what the authors call real effects—so restructuring is real, the paper indicates. Second, the likelihood that distressed firms restructure declines over time, and if you look at the sponsors or the leaders of restructuring banks and private equity, they seem to be the most aggressive in restructuring.

All right, so that is the summary of the paper. So my discussion centers on three themes. First, kind of motivate this topic with what we are experiencing right now. Second, I will talk about the main results and the main challenges in making inferences based on the paper. Then the third part would describe some suggestions to link the paper a little bit more tied to the theory, and maybe sharpen the empirical test so that the inference is a little bit clear from the analysis. All right, so in terms of the big, big picture, I think it is very easy to think that this is something we should really, really care about from the organization of economics perspective: how important are firms? Should we save GM or not? These issues of restructuring are very important in terms of who is going to restructure or not. For example the issue of incentives; you are the CEO of GM, should you restructure if you know you are going to be bailed out or should you just wait and get bailed out? So, issues related to financial intermediation: does the banking sector add value? Which are the intermediaries, for example, private equity that could substitute banks? All of these are very important questions at the reform level, which if you think about problem evaluation, which reforms are meaningful and important, which are the unintended consequences of reform? For example, in bankruptcy reform do we see unintended consequences that we could learn from... if we look at Japan? Then, more macro topics that I really have a little to say anything about.

Okay, so again, this is fascinating... If you think about all these topics from the micro to the macro, fascinating topics, that have this in Japan which is the second largest economy in the world, so this is a very important topic. So, comments from the results.

First result, restructuring firms versus distressed firms. Okay, so if you look at for example this table which the presenter showed, we could look at, for example,

distressed firms in red and the restructuring firms in blue. What we find is that those that restructure are more aggressive than the ones that are distressed, but not restructured. Now, my main comment there is: what is the definition of a restructuring firm? According to the paper, it is a firm that makes it to a newspaper. So, if Bank of America says, “Well, I am going to fire 35,000 people,” that is going to make it to the Wall Street Journal. That is going to be prominent. But that does not really mean that restructuring versus a distressed firm that might be quietly restructuring is any different. It just says what we know through the newspaper; we are identifying the big stories. So, what do we make of this difference. It is kind of hard. I think that if newspapers care about the readers, maybe they would select the most prominent stories, and my hunch is that that would be the case in Japan.

So more broadly, I think, there is the issue of selection. Okay, basically and currently, the analysis assumes that restructuring firms and those that are distressed are identical, which of course are unlikely to be the case. Okay so, if that will be the case, I would like to know, what is the friction? Why is it that the firms that are distressed are not restructuring themselves? What are the benefits or costs that prevent them from restructuring? So, as a result, I think this result is kind of complicated to really understand.

The second thing which I had actually quite big expectations given that again I am not completely attuned to the Japanese literature—I have read papers that many of you have written but I am not an expert—I was expecting a big, big role of banks in restructuring. So, when I went to the data, I was quite shocked that the bank role is not as important. So, the main bank only initiates 8% of the restructuring cases. So, we are talking here about very few observations. There seems to be a lot of restructuring that is initiated by the firm itself or by other companies, by orders of magnitude. So, restructuring does happen but it seems that the main bank is not driving the restructuring process, or the prominent role is not by the main bank. And then I thought, well 8% of all... and if you think about from the lecture that the main bank in Japan tends to have 20% to 30% of the loans of the firm, typically equity stakes, many times the director, this seems like a very low number. So, I was kind of interpreting the paper kind of the other way around. The result seems to suggest that the main bank is kind of very reluctant to re structure, or maybe I am missing something.

Okay, so other comments about the fact that again, selection, the banks are not going to be sponsoring or leading restructuring process where the firms are likely to be

comparable to other firms. So, if you think about the data, it seems like the default is that the firm would restructure itself; If not, someone has to step in. Well, that means that whenever the bank leads the restructuring, the process might be a little bit late; then the firms may have more leverage and might be different. Maybe when we look at the result, it is hard to interpret them as direct effect. So, we do see from the Summers statistics that liquidation is more common whenever the bank is sponsoring the restructuring, which suggest that leverage might be higher to begin with.

All right, so those are of the main comments and the main suggestion I have is... On the first hand, link the work more directly to the theory of what we know about Japan and again, I know a little about it, but if you think about, for example, group memberships, if you think about banks, what are the tradeoffs? That would be the first comment. Why is it that firms or banks are not optimally restructuring? Okay, so we know from the work of various members of the audience that, for example, banks have not have incentives to liquidate firms. Why not just push that? Push the incentives of the decision maker either at the bank level or the firm level, and I am really worried about this because the paper analyses restructuring decisions over time; and we know subsidies have changed over time, the health of the banks has changed over time, the importance of group membership has changed over time. There are many things that are changing over time. So the result... it is very hard to interpret what the results really mean.

So, one way to get a sense of the cost and benefits of restructuring is to think about groups that have, let us say, different business structures. Some of them might have asymmetric investment opportunities; so let us say one side of the business has great investment opportunities, the other side may not. The incentive to restructure might be higher than if you have just one group without investment opportunities—just to flush out the tradeoffs. Compare that to business groups where we just have flat, weak investment opportunities.

All right, so that will be one way to link it to the theories and focus on incentives. The other is to think about selection. Is it that just maybe managers are incompetent or they have difference preferences? We think that everybody should have maybe the Anglo-Saxon preferences but suggestive...a more descriptive work that I read about Japan suggests that people are just focusing on other objectives, focus on surviving. Could we get a sense of how important that might be? So on the margin, they are not going to change because they have a different objective function.

Then the last comment is to really try to corner the variation. Japan has had structural reform, has had massive asset declines in value. It is very hard for a micro person like me to really understand what is causing what is causing what; to understand what happens at micro level. So, just pushing a clear empirical strategy, trying to understand what is driving what, might really help the reader. So, for example, just thinking about variation in investment opportunities that may affect firms, and then ask the question of what happens to distress or restructuring status? How long does it take for a firm to get into distress? How long does it take to get it to restructuring? How large are these distress or restructuring stages as a cause of a common shock? And maybe we know from previous lecture, group status is important. So, focus on those differences and I would say it would be great to link the analysis to something we care about like productivity or profits, not only whether you restructure or not but what happens to the firms thereafter, not that we do not care about the outcome variables they have.

So, some potential source of our variations, I would say exposures to real estate values, exposure to terms of trade, if the exchange rate moves and your exports... that might be important. Last comment, maybe bankruptcy reform. We know bankruptcy would affect firms very differently as a function of their financing. So, if you have a structure where you have only one bank, bankruptcy maybe is less important than if you have dispersed lenders. That is where, for example, bankruptcy or a Chapter 11 type of structure might help a firm. You have automatic stay on assets and maybe you could restructure. It would be great if the paper could flush out the cross section, and then focus on specific source of variation so that the results would be clear as the result.

So, conclusion: it is a very interesting paper on a very relevant and timely topic. The main findings that I get from the paper is that there is differential decisions for restructuring versus non restructuring firms and that there are differential restructuring responses as a function of the identity of the sponsor. The time series results I find very hard to interpret. I think that, well, I have some comments about these results. I think that inference would be clear if the analysis were closer linked to theory and if the [inaudible] show you a little bit clear.

Prof. Weinstein: Why do not we just go around collecting comments and then we will let you respond at the end, because we are running short in time. So, I am just going to come go around this way, start with Koichi.

Prof. Hamada: I think this is an interesting attempt to find how legal reform or very legislative changes make firm behavior different. I appreciate it, but I share the opinion of the discussant that more can be done to connect to some theoretical or incentive issues. Those fact findings are interesting, but why certain institutional change or certain factors will affect differently is the first. Particularly in the western hemisphere, the firm is considered as common agent of shareholders, but looking at Japanese firms, people have some different ideas. Masahiko Aoki thinks he is always sort of an intermediate of shareholders and labor and so forth, and Katsuhito Iwai had not representative agents theory but some kind of fiduciary relationship on the part of the firm. Sometimes, we dispatch shareholders through the firm, but in the Japanese case or in some other companies the other way—that management will control some of the shareholders. I would like to have some of the same vein with the discussant, connection to the theoretical consideration.

Prof. Kashyap: I just have two questions. That was very interesting. I wonder whether the reason why you get so many that are coded as being firm led is because they have in fact violated debt covenants. So, if the covenants violated it, they have to do it so you do not get it looking like it is coming from some sort of financial motive, but in fact that is what would happen and if they do not do anything, the lenders would come after them. I am not sure whether you can check that in some way but it would be interesting to know. The second thing is whether you could do something with some measure of what the value of continuation would be. So, I realize that the firms on stock prices can be totally endogenous, that is not going to be very helpful. But I wondered if you could do something with either... put the firms share price in their and instrument it with the industry cue or something, so construct a cue and instrument it that way, or just put the industry cue in as a control, to see whether or not the selection is mostly just “the viable guys get rescued” or not. I do not think either of those are perfect but it could a first step.

Prof. Weinstein: Great. I am just going to make one quick comment which I thought was, I think it is a really interesting paper and one of the things I thought was most interesting was the increasing irrelevance of main banks. Whether you want to push the results—I think it is table five—and kind of take a stand and whether the main bank structures essentially becoming irrelevant or became irrelevant.

Prof. Ito: In the same line of comments: is there any possibility that the main bank role is underreported in the sense that successful main bank interventions would be *saiken*

before it hits the newspaper. So, replacing directors and making sure that rescheduling can be done without hitting a newspaper headlines. That will be a successful main bank intervention. If that was the case, most probably in earlier cases, probably that is the usual method of the main bank. Your data collection may be missing that; so maybe prominent cases of those *saiken*, make sure that those are in your data set and that will be good.

Prof. Kato: Yes, I have three quick comments. First one: really, one way to get at the selection issue that the discussant mentioned, there are some surveys done on the employment adjustments, so just to look at the sort of incidence of those employment adjustment methods to those survey results... It just really strikes me as a little, not strange, but it is really interesting... Like “stop new hires”; and if I am reading it right, it is only 8%, 1981 to 1995 and then 2%, 9%, 5%, so this will be first thing that sort of the restructuring companies tend to do according to this employment adjustment surveys. So, this maybe picking up really the end of their process, and especially those ones... I am really following of Taka’s comment, maybe the successful one already did it and no problem. It did not show up in the newspaper. It is really the tail end of that process picked up by you and so, these people have already done... Stop hiring? Yes, of course, we did it three years ago. We are still struggling. That maybe what you are capturing here. Just related to this, I really like to know what happened to those companies after this. Did they succeed or they disappeared and so the *saiken* ended up with another *saiken* and eventually disappeared? That will give us some little bit more about the nature of those *saiken* companies which will probably shed some light on the selection issue. Lastly, unions and labor management relations... I am just personally interested in whether that has some impact on the *saiken* incidence.

Prof. Ariga: I share with Takao on the interest in the eventual outcome of restructure process, so that will be really interesting on top of what is a very, very interesting paper. One minor comment is that there is a fair amount of the research done on the individual panel, using individual panel data on the restructuring mandate from the labor economies. I was of course already aware of it, but they should refer in the references and they are quite relevant to topics they are studying.

Prof. Weinstein: So, let us give you some time to respond.

Prof. Hoshi: Thank you all very much for the comments, and as many of you found out, there are lots more things to do using this data. Just to pick up some of the comments

we received, the selection is an important issue. More in general, what is the restructuring that we are picking up, and what is the restructuring we want to pick up. We use newspaper articles and I think I am pretty much sure we identify all the big cases including the successful cases that Takao mentioned. So the newspaper talked about the success stories of the *saiken*, some times looking back and in those cases, we call those at the *saiken* which happen a little bit... a couple of years ago. So, the newspaper seems to do a pretty good job reporting all those, at least the major *saiken* cases.

There was still the selection bias that you talked about on the restructuring both in reporting and also in observed restructuring; some distressed firms, they may not go under restructuring, but some distressed firms do, and we are looking at the difference. So, I think we should be careful in collecting for those selection biased. There was, well, the simplest way to do that is to do the statistical collecting, using, estimating some selection model which we happen to do in some of those variables, and then collecting for the selection bias from that model, and we ran those regressions this week and the result does not change; actually results are stronger. But we need to try some other possible selection models and see if the results stay the same.

The role of the main banks: we find only a few cases in terms of the proportion of the total restructuring cases, and we have the small cases of the bank-led restructuring. One reaction is your reaction, that there are very few the main bank-led cases even though we talk about the main bank-led rescues all the time in this literature. I think there are two things here; my own reaction to that is actually this shows that a lot of restructuring cases that happen without involvement of the banks. So, that seems to be an interesting thing, interesting observation. The firm seems to restructure without intervention from the main banks and this may be related to Anil's point on the violation of the debt covenant. But what we call firm-led is led by other firms, not self-led; self-led is another category which is possible.

Another possibility is: our sample starts from 1980s and through some other research that some of us did—for example, I did with Anil—we know compared with the 1970s, the incidence of the bank-led restructuring cases declined already by the early 1980s. So, in that sense, we may be already looking at the period where the main banks role has started to decline. We link the result to the theory, and the incentives of the people that Francisco mentioned and Koichi mentioned are also important. We need to do that. But at this point, I think the most important thing we wanted to say in this paper is therefore,

whatever the reason, we see the declining incidence of the restructuring over time, and whatever the reason, that may have an important implication for the real economy or for the Japanese economy during this period. But looking into what caused those changes is an important next steps that we need to move on. Another important next step we need to do is related to what Takao pointed out that what happens to these firms after the restructuring, the outcomes. We have not just looked at those data.

The stopping new hires: the information we get from these labor adjustment comes from the newspaper articles. So, there may be some underreporting—not underreporting of the cases, but underreporting of the contents of the restructurings. So, we need to check that. If you happen to have firm-level of data for stopping new hires, we would like to use that, same thing for union membership. That is a great idea. We do not have the data but if you can share that data with us, that will be great. David's point on increasing irrelevance of the main bank, I think that is correct. It was not clear from the paper we should stress that. What else?

Ken's point on the literature in labor economics; yes, I know some of those papers so we should refer to that. I think that is about it.

Prof. Satoshi Koibuchi, Chiba University of Commerce: Just one thing, in my experience to collect and coding the article by myself, the Nikkei article covers almost all information on listed firms because the Nikkei newspaper is a newspaper specialized on news in the financial market. So, it is related to your first comment. So, Nikkei newspaper coverage may be a broad and comprehensive, so there is no risk underscore for tiny news for small listed companies.

Prof. Weinstein: Okay, why do not we move things forward? So, we are actually going to change to be on the schedule and we are going to have Obstfeld to his presentation next.

Prof. Maurice Obstfeld, University of California, Berkeley: In the interest of trying to not get too far off schedule, I am going to skip over a lot of these slides, but just give you the main flavor of what I am trying to do. I will start by showing you the, by now, well known picture of the yen's nominal and real exchange rates, real effective and bilateral against the dollar. As you can see, over the period since the late 1970s and including the bubble period that this conference is covering, the yen is undergoing epic gyrations. These gyrations have interacted in interesting and well known ways with the

real economy. Andy Rose argued yesterday that international financial integration may have nothing to do or little do with the Japanese economy but certainly the behavior of the exchange rate has had a lot to do with it. This picture illustrates the correlation between real appreciation and GDP growth, and the pattern that is well known is that real appreciation is correlated with lower GDP growth overall for the Japanese economy. Exporters get slammed and Japan is a very export-driven economy, so this has a big effect.

The goals of this paper are to try to understand these exchange rate movements within the framework of some standard models, and the contrast is really to some hypotheses that have been raised. One very famous one is through McKinnon and Ohno in their 1997 book who argued that basically the yen's behavior is exogenous. The yen moves and it is driven largely by tensions over trade. This in turn affects the economy. This idea kind of begs the question of what are the policies that actually make exchange rate move and response to tensions over trade; if there are political tensions, there must be an expectation that policy will in some way drive the exchange rate to the desired level. So, how does that happen? So, goal number one of this paper is to try to chronicle some of the links between monetary conditions and the yen/dollar exchange rate; then secondly, to look longer term and study the factors moving the yen's real exchange rate in the long run.

One tool that I used because I think it does throw some light on the yen's movement is the real interest parity idea. Now, we know of course that certainly in the short run, this does not hold very well. We know that there are risk premium and changes in long term equilibrium rates that may make this somewhat less than useful as a complete explanation but I think it has a surprisingly large amount of power in at least guiding a narrative about the exchange rates movements.

One form of a couple that I look at in the paper is to write a version of this which works in terms of deviations of real exchange rates and real interest rates from natural levels or full employment flexible price levels. If we assume that in the long run, people expect the real exchange rate to be at its natural level then one gets an expression such as the one I have down here.  $q$  was the real exchange rate defined as the number of yen per dollar in real terms,  $r^*$  as the foreign real interest rate;  $r$  as the Japanese real interest rate and the tildes indicate natural levels and I come to this a couple of times in the paper.

Now, if one tries to look at the relationship between the real exchange rate and the real interest rate, it is problematic of course, because unless there is an index bond which we do not have over this entire period, you do not observe the real rate of interest in any direct way, so I tried to proxy it using 10-year bond rates and centered 12-month inflation rates to get some sort of measure of real interest differentials and just graphing these variables together, there is a surprisingly high correlation. It is 0.45. Most of the literature has taken a pretty lean view of interest parity—at best looking at very long run or sort of low frequency movements where you can find some correlation—but it turns out that for the Japanese case against the dollar, the correlation is reasonably high.

The basic relationship between the exchange rates movement and the bubble period, the bubble's emergence crash and stagnation afterwards, have been quite widely discussed and debated and this picture shows some of the major movements and issues. In the mid 1980s, the yen was actually had returned to its 1960 to 1972 trend line prior to the Plaza Agreement which brought about a major depreciation of the dollar from the very high level. The yen appreciated sharply through the end of 1988, and this was a period in which the bubble emerged but in which there were also some real difficulties for the Japanese economy. The yen began to depreciate at the end of 1988 as I said. This is the period which is has been greatly discussed in the literature including in survey paper by Taka Ito and Rick Mishkin, this is a period in which there was limited evidence of inflationary pressures, and a hesitancy of monetary policy to react to the bubble in part because the appreciation of the exchange rate which had happened so recently was something that everyone was relieved to be over. There was no appetite to arrest that. Nonetheless eventually, inflation pressures emerged. Monetary policy was tightened quite sharply as we know and the bubbles in real estate and equity markets burst.

Okay, so let me skip ahead. Okay, here is about where I am. A major episode in bubble collapse and post-bubble stagnation, I believe, was the appreciation of the yen from April 1990 through April 1995. The real economy was slowing down. This appreciation, no doubt, interacted with that slowdown in ways that made it more severe and I think this is something of a puzzle. The reasons I think come from looking at both some of the policy actions that occurred at the time and at some of the trade tension issues that McKinnon and Ohno actually emphasize. There was a sharp reversal of Japan's tight interest rate policy, the anti-bubble policy as it were in the spring of 1991 but by then US rates were also falling quite rapidly. There was an uptick in trade tensions and fears which might have been associated with the incoming Democratic administration which had talked tough about trade. I think paradoxically, the fact that US growth was higher

than Japanese growth might have accentuated matters because US imports were high, Japanese exports were high.

Furthermore as of 1994, there were some signs which were not really well realized in the event of Japanese recovery. In a very nice paper on Japan that was written in the early 2000s looking at the bubble period and the inflation, Alan Ahearne and some coauthors hypothesized that the market real rate of interest may have actually declined, and this according to the equation I showed you would have put some upward pressure on the yen relative to the natural real rate of interest. The end of this appreciation was marked by joint intervention, reversal of the trade pressure from Washington and monetary actions that supported a weaker yen and from that period on, there has been a trend of real depreciation of the yen.

Now, one would like to make sense of the longer run movements, and the paper says more about the short run movements, but the time is really limited. Here, the leading theory which is actually fairly robust if one is willing to assume factor market adjustment and—a bigger assumption—the law of one price or purchasing power parity for traded goods, is the Harrod-Balassa-Samuelson point of view which is kind of a differences in differences approach, because it is based on the difference between differential traded and non traded productivity growth in Japan and the trading partner country—the idea being that when relative tradables growth goes up, the price of non tradables relative to tradables also goes up. There are some early empirical works that look at Japanese data. David Hsieh did a study but it ended in 1976 so there was not much floating rate data there and the floating rate dynamics are quite different from those under fix rates. I think Marston conducted a detailed study, which supported the theory and subsequent researchers have done a lot of empirical work on this issue.

One study that I think is notable is the one by Canzoneri, Cumby and Diba where they looked at the panel of OECD countries and argued that if you look at relative labor productivities and tradables and non tradables, you get a fairly robust version of the Balassa-Samuelson prediction. That turns out to be valid at least for Cobb-Douglas production functions. They did two things in the study. They looked at the law of one price per tradables in a co-integration framework and they found that against Germany, it worked pretty well if Germany is your base country against the US, it worked less well.

They also looked at relative prices of non tradables compared to relative sectoral productivities, and here they found support for a building block of the Balassa-Samuelson view. They did not look directly at the link between real exchange rates and productivity. So they viewed this as a sort of limited confirmation of Balassa-Samuelson. In general, attempts to relate a real exchange rate directly to relative productivities, Balassa-Samuelson have not been that successful. Phil Lane presented some progress on that in his paper yesterday. There is also an interesting paper about Japan and other Asian countries which is more supportive which leads one to the hypothesis that perhaps looking at more open trading partners may make a difference.

Now, part of the problem with these studies is that if you look at the sort of breakdown of the changes in the real exchange rate in a way that several researchers have done... In this equation, basically  $\gamma$  is the relative weight of tradables in the CPI, you see that the rate of change in the real exchange rate depends on relative non tradables to tradables prices which are the first term, and these are likely to move very sluggishly, but also on the relative tradables price levels which depend directly on the nominal exchange rate as well as the tradables price synthesis. The variability in the nominal exchange rate is high and so this tends to dominate the movement of the real exchange rate, certainly in the short run. Another way of saying this is that the internal price ratio contributes less to the variance, at least in medium and short horizons, than does the external price ratio.

For this paper, I looked at some of these data. I used the European Union KLEMS data and looked at some TFP numbers and labor productivity numbers. They seem to be very, very highly correlated in the short run. I followed Canzoneri et al in focusing on average labor productivities. It has some advantages they talk about and the empirical regularity which is also consistent with the GDP data I showed you, and which holds not only for Japan versus US but for Japan versus Germany, is that if we try to correlate the real exchange rate in the short run with the Balassa-Samuelson relative productivity variable, the difference in difference variable, the short run correlation is exactly the opposite of what the theory tells you is true in the long run. This I would hypothesize is due to the kind of procyclical productivity effects that are well known in the domestic macro literature, but corresponds well to the idea that in the short run, productivity is exogenous when exporting industries are faced with a high real exchange rate and have to maintain excess capacity. Measured productivity tends to fall.

One could look alternatively at a level's analysis with productivity. Now, here again it is the same productivity. Now, here again it is the same productivity data, so it is endogenous. I suggest some ways in the paper in which one might try to purge it of some of these endogeneity, which will be an interesting experiment to do. But if you basically assume PPP for tradables as Balassa, Samuelson and Harrod do and put together a relative productivity variable like Canzoneri et al, you basically get a level real exchange rate. Looking at the US-Japan comparison here, you will have a picture like this. So, obviously the much more variable purple squares is the real exchange rate, the blue diamonds are the relative productivity variables. Just a caveat here, these are arbitrarily based at the beginning of the sample. So, there is like a constant in here that I set to zero. So, really we are not looking at absolute overvaluation or undervaluation. They are just movements away from that or towards that. So, we can see very clearly the periods in which the yen's real value caused distress. We can see the mid, the early 1980s Reagan era. On the whole it tracks, but there are really very big deviations in between and it makes you question the usefulness of this measure except in the very, very long term perhaps.

The organizers encouraged me in their comments to look at the Germany versus Japan comparison; and interestingly, this works a lot better. My guess is that these look like co-integrated series. But still you see some deviations that are very large, including in this case, the large depreciation of the euro following its introduction in January of 1999, so we here we get something that is more encouraging. Now the deviations in all cases as I said are highly correlated with large movements in the nominal exchange rate which we believe affect the relative price of tradable CPIs in these countries, and I have started to look at these and we would like to add some more to the final version of the paper. If you are willing to sort of make some heroic assumptions about the absence of pricing to market and the absence of distribution costs for domestic tradables, you can actually proxy the relative tradables price indices using export and import price indices. I do not think this is a very good proxy but I think it is a little bit indicative of how the nominal exchange rate works on this synthesis.

Basically doing a little bit of simple algebra, you find that you can write the relative tradable price synthesis in terms of... basically if we think of this as Japan and the US, the relative export prices of Japan and the US in head to head competition and the terms of trade of the two countries separately, there are overall multilateral terms of trade, price of imports divided price of exports for Japan and the US. If we... I should apologize because here, actually alpha is 0.85 so this is one minus alpha... but just

taking that as the weight of imports for the two economies taken to be the same, you can plug this and what you find is that you get something like this.

So, as one would expect, the cycles here are very closely correlated with cycles in the nominal exchange rate. What is interesting is that there is a distinct downward trend in the relative Japanese price level. It is falling secularly over time and this is something that I do not think... it is not clear that if you just take the Harrod-Balassa-Samuelson view and add this, you would get something that matches actual real exchange rate. So, this is something that I think deserves further scrutiny. Why is this happening? What is behind it?

So, just some conclusions, I think it would be a mistake to take the yen/dollar rate as being related exclusively to trade tensions, not to policies. I think monetary policies have been important in the short run and in ways that do make sense. This is not to say that every fluctuation can be explained—far from it. Expectations are extremely volatile and drive the rate in the short run, and trade tensions have been important. The long term Harrod-Balassa-Samuelson theory may have some bite, but especially in the US case, the long run seems to be very long indeed, and it would be very hazardous to use a simple version of the theory to try to forecast the real rate. Here is where augmented versions such as Phil's may have some more to say. It is possible and this comes out in the Germany-Japan comparison, that HBS—not Harvard Business School but Harrod-Balassa-Samuelson—may work better against more open trade partners. That is what the German data suggest and also some of the evidence on East Asia. More work is needed on relative international tradables prices and certainly the trends in these look very fascinating and would deserve further study. That is it.

Prof. Ito: Okay for the interest of the time, I will skip most of the slides anyway. Okay, it is a real pleasure to comment on the Maurice paper and state a goal. I would skip and link between the yen and monetary skip and the growth rate policy skip and the McKinnon and Ohno hypothesis. I think the one missing link is I think that they emphasize yen appreciation expectation, and from there they used the interest parity to come back to the monetary policies. So, that is my understanding on McKinnon and Ohno.

Okay, so my comments. First I will make some observations about which causality are we talking about either, from exchange rate to macro or macro to exchange rate. Sometimes, the paper and also the literatures which is from one causality to another so

that is important to make sure which way are we thinking. Section one is the volatility of the yen and narratives on how the yen moved around which I skip in my comments and section two is more on the real exchange rate movements which I will make some extensive comments. This slide is from my teaching that I usually ask my students: what was the value of the yen when it was introduced in 1871? And of course everybody knows, it was one yen, one dollar. So, it was a parity long time ago.

This is a long, long view from 1879 to most recent. The prewar period when Japan was clearly an emerging market economy, that real value of the yen stayed basically stationary and when it was appreciated was the period between interwar period when Japan struggled to go back to the old parity and like European countries suffered some deflation and so on. What saved the Japan was of course the devaluation of the yen in 1931 ahead of the US. In the postwar period as you see in pink which is the CPI bilateral real effective exchange rate, you see tremendous increase which we usually think Balassa-Samuelson but that is not there if you use wholesale price indices which is more tradable prices, until 1971 when the nominal exchange rate starts to appreciate.

So, looking at WPI or CPI gives you different views and that reflects more of the tradables and non-tradables differences. So, this is the Balassa-Samuelson, what many people talk about. Later slides, I will talk about what happen after 1995. Briefly about causality, that the paper make some comments on the exchange rate shocks and suspecting that is causing difficulties in Japan, especially the 1990 to 1995 appreciation causing the difficulties in the already burst bubble economy. And also there is mentioned about the yen appreciation causing the monetary authorities' response, the reaction function in the post-Plaza period.

So, these are the sort of examples of exchange rate. Shocks were in exchange rate causing the macro fundamentals. To give additional comment, I think many firms in the second half of 1990s remembered 80 yen per dollar of April 1995 and they said, anytime yen may go back to 80 and they feared that and put off the investments in Japan. I think that is more like the [*inaudible*] argument which may be interesting to look at. They forgot about 80 yen by 2003, but now as of today as they saw 88 overnight, again there is this 80 as the floor, ceiling whatever. People start to focus.

The other side is macro fundamentals driving the exchange rate which is more in line with the macro tradition and either growth acceleration causing yen appreciation, Balassa-Samuelson or monetary tightening causing yen appreciation, which is the

interest rate parity. So, there are two ways we can talk on the relationship between the exchange rate and macro. So, in the paper it switches over one to the other as it progresses.

If you try the VAR using an exchange rate, usually exchange rate comes as [*inaudible*] cause macro variables. So, I am going to talk about nominal, bilateral, real bilateral, nominal effective and real effective which is repackaging what is already in Maurice's paper. Here is the bilateral; this is a very familiar picture of the nominal yen dollar exchange rate. So, there is great appreciation from the Plaza period up to 1995 and there is, I would say the 1995 episode is aberration. It is a very, I would say, bubble and burst in the yen/dollar exchange rate because you see it is more... this is a particular case of the trade conflict causing the yen dollar exchange rate response which was brought back very quickly to 100 level within a few months. So, as you see that is a glip in history. What is peculiar is that nominal stayed very constant in the box range in technical terms after 1998, and stayed until two months ago in the box. So, we are now out of the box and we will see how things will play out.

This is a real bilateral exchange rate. What I want to draw attention is the real depreciation which was mentioned in the paper but it is really striking after 1998 that there is a trend of secular depreciation in real terms. Putting them together, until 1995, we see the core movement between nominal and real which is usually established in the literature that most of the real exchange rate movement is from the nominal exchange rate that we learn in the textbook but what happened after 1998 was the trend deviation between nominal and real. So, how about effective exchange rate? We are going to see the same thing. By the way, in the nominal effective, we are already in the record high level which we surpassed. The yen surpassed that in the April 1995 episode; a record high, as we see in the past few weeks. This is a real effective and it is really striking that the level we saw earlier last year and earlier this year was very similar to the 1973. So, yen in real effective terms was so weak that you can go back to 1973. So, it is, I would say great depreciation, from 1998 to 2007. So, this is, I would say, great depreciation.

So, Balassa-Samuelson: this is the growth rate. Growth rate has been coming down in the stages about 9% until 73%, 74% in the 1970s and 1980s and 1% in this bubble burst thing. This is the growth rate compared to Japan and US and as we see, Japan is growing much faster than the US until 1990; then US was growing faster in the 1990s during Japanese stagnation. You could see the TFP. TFP, Japan was more or less the same in the first half of the 1990s but clearly lost competitiveness in the second half.

So, comparing the real exchange rate and growth differential, then we see that in the 1990s that we think, I think that there was a reversal of the Balassa-Samuelson, and that is probably explains the great depreciation in real terms. So, the possible one is the turning point. Why we had the appreciation until 1995 and it lasted well the bubble burst in the early 1990s. Maybe the exchange rate response was five years late, maybe the yen was responding to the trade conflict which is more or less McKinnon and Ohno, and maybe Japanese investors are repatriating yen just like US investors are repatriating US dollars now, which I think unlikely. C is unlikely but A or B, there could be the explanation.

Another part of the puzzle is nominal and real. We are all again taught that the nominal, real exchange rate fluctuations are mostly explained by nominal exchange rate volatility, which is not the case in the last 10 years because the deflation in Japan was persistent and the inflation differential of 3% or 4% persisted for 10 years, and that explains the deviation which I showed you. So, the great depreciation of the yen was the results of the great deflation of the Japanese economy. I am curious to hear Maurice's comment whether there was a precedent in the history.

Summing up, I think the fundamentals are very important. This reminds me of the Casablanca. This is the influence of Takeo on my presentation skill and this is the cover and let us try...this is an innovation. Can you hear? Maybe not. Maybe I will sing. Ingrid Bergman is trying to ask the some to sing and Sam sings.... [*Movie playing*] Okay, so the fundamental things apply, as time goes by—that is a real exchange rate. Thank you very much.

Prof. Weinstein: We are over our allotted time, so we are just heading to our lunch time, so what I would like to do is ask people to make very short comments or questions. Koichi?

Prof. Hamada: I think that this can be well connected to Fukao's and Dekle's paper, how the real exchange rate and so forth can be dispersed. Already, I am commenting, but I would like to ask the role of the terms of trade. I think Maurice already decomposed some of the domestic price and external price but my impression is... when in terms of trade it is improving, then the rising real exchange rate for Japan did do very much damage.

Prof. Kashyap: One thing we might ask you to do, Maurice, since we in the end did not have a full-fledged evaluation of monetary policy, I wonder how you would feel about just adding four or five pages to just the history of what the BOJ was up to as a precursor. I know it is not a perfect fit, but it kind of make some sense that could then be the section of the book that we would refer to that kind of gives some background. I mean, there is a lot of place to find it.

Prof. Weinstein: Other comments or questions? Yes, I think it would be good for you guys to link your papers together, because you have now have a bit of a story for why the exchange rate was moving and then you can talk about some of the real effects on that. Anyone else? Okay, so lunch...oh, I am sorry. I did not give you a chance to respond. I am sorry, excuse me. Maurice, you will have a chance to respond.

Prof. Obstfeld: No, I want to thank Taka for the excellent comments which provide a good framework for thinking about some of the issues which will help me when I revise. Just couple of things: the McKinnon and Ohno thing is so puzzling because basically McKinnon says, "Look, people expect the yen to appreciate forever; therefore interest rates in Japan are low." But if you are from the BOJ, you would say, "Well, we set interest rates." So, I think there is a disconnect there which is what I try to address. Also, in terms of the remark you just made to Dekle about his paper with Kyoji, I think what we have to sort out is that basically this: I find a mild Balassa-Samuelson effect looking at the classical productivity variables. In their simulation, they find very strong Balassa-Samuelson effect going in the opposite direction, and Taka says there has been is a very strong Balassa-Samuelson effect pushing toward depreciation of the yen. So, we have three totally different views of the world.. Taka, if you basically just look at overall average productivity growth, which reminds one of the Kravis-Lipseye effect, it does seem to be there for Japan, and maybe that is something I should discuss more in the paper—the fact that richer countries have higher price levels. That does come through in the Japanese data.

Prof. Weinstein: So, I just want to say lunch today is actually going to be just outside the conference room, so we do not go up the fourth floor today; we will restart at 14:00, so we have about an hour and 20 minut

**Day 2**  
**Afternoon Session**  
(Transcript)

Date: 12 December 2008, Afternoon Session  
Venue: San Francisco, California

Prof. Anil Kashyap: Okay. If people can take their seats I have got a couple of administrative announcements about the next step for the project before we start the session.

Let me begin by giving some information about the deadlines and then we will talk about the process. Plan from the editors is that we will divide up the papers, and you will get a referee report from one of the editors that will be assigned to your paper. We will get those to you by no later than the week after the AEA meetings. Then, as per the contract, you have until March 25th to get us back the final versions of the papers. At that point, we are going to ask to have them copy edited. We have hired Larry Meissner whom some of you might have worked with before. He was involved in last MIT press book that was done on Japan. He will do the copy editing. The procedure we are going to follow for your approval for both the copy edited version and galleys is to insist on very short turnaround time. We would like to try to get the book out by the end of the year. To do that, we need to be draconian. Basically, we will give you warnings as to when you get the material back and then you will have like a week to turn it around. We hope everybody can cooperate with that.

We also want to tell you that we are going to try to make the papers a somewhat standardized format. For the papers that have a serious model and a substantial theoretical component, we would like the papers to begin laying out the stylized facts and motivation for your paper. Presumably these papers are being written because there is no accepted framework for explaining the Japanese experience so you should connect your theory to the related ones that have been previously pursued. If you have a strictly empirical paper then we would like you to just explain again the motivation and give a more detailed summary of the previous literature to put your analysis in context.

It would be very helpful if you would read the other papers in the volume that are closest to yours, so we have regrouped things kind of as follows. There are two papers on labor those go together, two papers on Restructuring and the paper on the stock market those go together. There is consumption paper and the medium-term productivity paper. Those should probably be looked at. And then there are three international finance papers. If you could look at the papers that are most closely related to yours, you can make things simpler for us by drawing the appropriate connections and avoiding contradictions.

To try to promote interest in the book, we would like your permission to take the versions that were produced for this conference and put them up on the website. There is a website that my colleagues at Chicago have been running on the financial crisis that gets fair bit of traffic – something like 500 visitors a week or so. We propose to put all papers on that website one after another in the box and let them sit up there a month so that people as they are coming will see them. We will eventually take them down because as the book moves toward the publication, MIT Press actually does not want to be giving away something that they are going to be paying for. But we figure in the interim that would draw some attention to them and maybe you will get some comments. If anybody does not want to have paper up there just let me know, but if we do not hear otherwise by January 1 we are going to presume that everybody thinks that it is a good idea and post them. Once they are posted we will send you a link to the web site and we encourage to forward this to your colleagues.

Finally, we will need to make a choice on word processing. It looks like some of the papers are written in Tex and going from Tex to anything else is very difficult. So we will probably end up taking the papers that are written in Word and migrating them into Tex at the very last stage. This should be transparent to you. We just mention it now so that when you get the galleys back you will be aware to check for the kind of errors that might occur in this type of migration.

Okay. Are there any questions anybody has?

Okay, seeing none. We now continue with the last session. Where we are going to see the second papers on employments? So, Takao, who is doing the presentation, you?

Prof. Takao Kato, Hitotsubashi University: I am doing it.

Prof. Kashyap: Okay, thanks.

Prof. Kato: Good. Thank you very much and this is a joint paper with Ryo Kambayashi, Hitotsubashi.

The Japanese employment, what happened to Japanese employment system after the bubble burst? That is the theme of this paper.

Let me give you what we think is the Japanese employment system very briefly. It is a cluster of complementary employment practices and a number of things that have identified as key elements of the system by a number of different scholars and I think they are seems to be some consensus among those. So, let me just list them up.

First is a lifetime employment, which is the focus of this paper although we do some work on the rest of it, because they are really complementary. So, we really need to talk about them together at some point in the paper, but the focus is the lifetime employment. It is implicit long-term employment guarantees for the core labor force.

The second is employee involvement at the grass roots level, innovation activity or problem solving activities.

The third is incentive schemes, such as profit sharing and employee co-ownership plans and so forth. We have some data on this as well at the end of the paper, on the extensive information sharing and then careful screening and extensive training.

So, there are some studies which show that these things did contribute to the rise of the Japanese companies in the 1980s, some positive productivity and so forth. But as Japan's stagnation continued, so the first thing is that the popular rhetoric kind of shifted about the system and then some even argued that the Japanese employment system became a major structuring impediment to its robust recovery.

Now, there are some lively public policy debates, concerning this Japanese employment system. For example, there are some studies and some observers of the Japanese employment system that argued that following the first oil crisis in the 1970s, the legal environment changed. It became difficult for Japanese companies to lay off workers. That generated produced some rigidity in the labor market, which turns out to be a problem for the recovery process.

However, most recent studies tend to challenge that view. So, there are some interesting important public debates, a lively one, ongoing. Of course, [Jason *inaudible*] theorists, and these people tend to argue that the Japanese employment system is resilient, the enduring nature of this system has been stressed because of negative incentive effects and reputation effects of breaking this kind of system in response to the shock.

Okay, so, what we do? We would like to inform the ongoing debates. I think we believe that we really need to have a good latest, recent, up-to-date evidence and some simple questions. One, whether the Japanese employment system indeed evolved in response to real adverse shocks—did it really change and if so, in what way and how quickly? Those are rather simple questions. But answering those simple questions empirically turns out to be quite challenging and the data limitation turns out to be a big one. So, that is what we do. Again, our focus is on the lifetime employment, the first pillar, because that is really the foundation in our view for the rest of the remaining elements of the employment system.

Some prior studies. They tend to show that there is no evidence. They failed to find evidence for the significant change in lifetime employment system. However, there are two major limitations. One is a simple one. They used the 1997 Employment Status Survey to calculate the retention rate and obviously the stagnation continued after 1997 and for another five years. Those five years, maybe that is when the actual change occurred, so we just do not know.

Also in terms of the vehicle change that occurred in 1998, there is a revision of the Labor Standard Law allowing Japanese companies to offer multi-year fixed contracts, which used to be banned. There was some potentially important change in 1998 in the legal environment. Recent macro studies tend to show that the employment adjustment speed accelerated during the last five years.

The limitation is no detailed disaggregate analysis. They will use aggregate data studies. We believe that is a major problem because the practice of lifetime employment was never claimed to apply to all Japanese workers, so we know that. So, that is really disaggregated analysis. It is really important. Some studies show that the scope of lifetime employment actually expanded during the high growth era to include some mid-career people, women, and there is a possibility which we are going to really explore in the paper, which is that during this great recession, maybe what happen is the Japanese companies started to withdraw their promise of long-term employment from this added segment, we call it “expansion members.” That might be the story that we may be telling in the end.

What we do? We persuaded Japanese government to give us access to microdata from the ESS, including the 2002 survey. The most recent one was given to us. So, we now have three separate time periods; 1982-1992, 1987-1997, 1992-2002, and we calculate

10 years of retention rates. That is the first thing we do. We also have done some cohort analysis of new graduate hires, and seen what happens to the percentage of the new graduate hires as working age at the population.

Conclusions and implications. The first thing is I think the evidence shows some resilience of the system—little decline in job retention till the final years of the great recession. I have one thing to show here; this is the 10-year job retention rate. The blue is the 1982 to 1992, the bubble and then red is the kind of recovery in the great recession excluding the final years, and then the yellow includes the recent five years. So, as you can see there is no change in job retention rate from the bubble to the first stage of the recession, but it did eventually go down. It is not substantial. So, that is one observation, and also if you focus on male regular employees with college degrees, virtually no change even for the most recent time period. So, there is some resilience of the system. I am going to skip that.

This does not mean, however, the complete rigidity of the Japanese Employment System because job stability after all did go down in the end, and then substantial decline for some groups. This is for the male regular employees, not a huge decline. But if you look at females, that is where there is the big decline in the job retention rate. So, the female regular workers actually experienced some major change. I will skip that.

Now, the college graduate, high-school graduate; again, not much change for college graduate—they somehow survived. The high-school graduates; some substantial change even for male regular employees, so the education matters. This is the biggest loser. So, this is one of the punch lines. Female regular employees working in large firms, they got hit the most. There is a rather substantial decline here, especially for this mid-career group.

Interpretation. I am going to interpret these results and then I will conclude. What does that mean? Well, this Japanese employment system became rooted, well established during the high growth period, and that means, as the theorists suggested, there is a strong institutional complementarity. What that means is instinctive and hasty changes even in one element of the system caused the whole system to halt; in other words, there is a complementarity there so you cannot just take one out and then have the rest of them continue. That makes the changes are less dramatic.

Now, the reason is, more specifically: if you break the implicit labor contract, two adverse effects will follow. One is the undermining incentive for the remaining lifetime employment workers to continue to invest in firm-specific human capital, and generate local knowledge and share that with managers; that incentive can get undermined if your fellow lifetime employment workers are getting fired. The second is lasting reputational effects. The few workers will think twice before join in this so-called lifetime employment.

One of the key challenges for the Japanese company is, when they face downward sort of slowdown, they need to adjust labor input, but they do not want to really jeopardize those incentives for the remaining lifetime employment workers or they do not want to amplify the negative reputational effect. What did they do? In our view they actually did something like this. They created and maintained a two-tier structure of lifetime employment workers. One is the original members of the lifetime employment system—male new graduate hires—and then the expansion members; those are the female and mid-career hires.

So, there are some studies which show us that during their high growth period a lot of those female regular workers and mid-career regular workers started to enjoy sort of lifetime employment. So, they are added to the league, so to speak. So, this is expansion teams. So, what happened is: when you downsize, the cost of breaking the implicit contract tends to be lower for those expansion members as opposed to original members. Why? Expansion members have not been investing in firm-specific human capital as much as the original members. For those reasons, they will begin with expansion members when they need to break the implicit contract. In other words, what they seem to be doing here is really to continue to honor the long-term employment contract with the core, original members; but in order to do so, they need to kind of really break the contract with these expansion members—females and mid-career hires.

Lastly, this 1998 revision of the Labor Standard Law can be viewed as a government public policy to really facilitate Japanese companies to really withdraw their kind of lifetime employment promise from this added segment of the labor force. So, the exception what they do your regular employees, your women, your mid-career—from now on I am going to give you a multi-year fixed contract, no longer an indefinite contract. So in three years, you will be fired.

That is a sort of... they are now allowed to do that. So, that is my interpretation and I have a lot of pictures here, but I will stop here. Thank you.

Prof. Kashyap: Alright. So, the discussant is Enrico Moretti from Berkeley.

Prof. Enrico Moretti, UC Berkeley, NBER, CER and IZA: Thank you. Can you hear me? Is it working?

Prof. Weinstein: Yes.

Prof. Moretti: This is an interesting paper on an important topic. It is interesting both for the specific issue that it addresses, but also for some of the broader questions that it raises, especially for people who are interested in Japan but also in other countries with rigid labor markets.

The specific question that it addresses of course is the issue of changes in the employment structure following the great recession of the 1990s. As it was pointed out, the popular perception of the Japanese employment system was highly favorable during the 1980s, and then it turned much more critical after the recession; currently the perception is much more that the system is rigid and potentially distortionary.

This generated some debate in Japan but part of the reason why the debate existed that hard facts are limited and the main contribution of this paper is to provide the first set of facts, the strict facts that can inform the debate much better. This respect to the evidence provided, I find it comprehensive and really convincing.

As I was reading the paper what strikes me about the description of the Japanese system is—although the authors present it as a unique system—how similar is to other systems that I am really more familiar with, especially European labor markets. So, the key features of the Japanese system are lifetime employment, seniority as a key determinant of wage increases, training, employee involvement in problem solving, some form of ownership of workers and profit sharing, and joint labor-management committees.

If you exclude profit sharing and the emphasis of training, they look like the Italian labor market. For example, lifetime employment is not... although there is not the implicit promise the same way that it is in Japan, a lot of Italian but also French and German firms have similar high costs of dismissal. Seniority as a key determinant of

wages: well, if you are run a regression of wages on seniority in any continental European country you will find an answer that is very high, not 1 but certainly very high.

I think part of the reason why this paper is interesting is that because it uses a very sharp and arguably very large shock to a rigid labor market and then tries to see what happens. In some sense the size of the shock is much larger than what you see in all of European countries, but the problems that rigid labor markets face are very similar in Japan. In some sense the sharp source of indication can be used to shed light on broader issues that have to do with rigid labor markets.

With perfect data and infinite amounts of time, what are the three questions that I would like to see answered in a paper like this? Well, the first question is: can a rigid labor market adapt to a macro shock? So, in other words, to what extent are rigid labor markets really rigid? The reason to think that even rigid labor markets may adapt to big shocks: first of all you may directly change the formal institution that makes the labor market rigid by relaxing some of the implicit promises or like in the Japanese case the 1997 reform of the legal framework that was supporting the lifetime employment.

But the second way that this is going to happen is you can keep fixed the institutions, but you can let market forces go around these institutions. For example, in a lot of European cases you have nationwide weight setting by industry and by occupation. So, given an industry and the level of occupation, there are contracts that say this worker should be paid this much. Well, some employers go around this by effectually promoting faster some workers; therefore even if within an industry occupation cell, you have a zero variation in earnings, you have the more productive workers are promoted across the occupation faster. Even keeping constant the institutional framework, you basically let market forces go around it. Of course, the third way in which a rigid labor market may adjust is to the informal market. I do not know whether this is an issue in Japan at all. It certainly is in continental Europe, especially in places like Italy where 25% to 30% of employment is potentially under the table.

So, in other words, the first question that I would like to see answered is how effective is the binding power of these institutions? I think on this question, this paper is very convincing. It does a very good job at describing what happened to lifetime employment in Japan as a function of the following the bad shock of the great recession.

The second and third questions, I think, are questions where the paper could think a little bit harder. Possibly, this paper or follow-up papers, we are talking it over lunch, and it sounds like this is the first step in a broader research agenda. I encourage the authors to do so, because question two and three are probably the million-dollar question, at least in my view. One is: what is the effect of this rigid labor market institution on employment and ultimately on growth? So, what are the causes of these rigidities?

The third question is: what are the benefits of these rigidities? The others mentioned the issue on investment in firm-specific human capital as a potential benefit of the rigid system, where workers stay with the same firm forever. Question one, I think, is very well answered in this present context. Question two and three: it would be nice to see more done on those questions.

So, on question one: probably the key contributions of this paper is to use a novel dataset which is the Employment Status Survey which for the first time provides worker level information on employment status of different workers with different skill sets and different seniority and gender. The ideal dataset here would be a longitudinal dataset, where you observe the worker, then you observe the shock, and then you measure how much, and what it is like, that the worker can lose his/her job as a function of the shock. They do not have longitudinal data. They have repeated cross-sections, so they are like the current population survey in the US.

Basically they create synthetic cohorts where they follow a group of workers; say observe in 1984, then they see the same group of workers in 1994, and they can measure how many workers are still with the lifetime employment contract. And what they find is it is fairly interesting. They find that for prime-age male college graduates, there is not much of an effect for most of the period. You start seeing some decline in their lifetime employment toward the end of the period. So, after 10 years of recession, this group starts seeing some effects, although it is not large.

Most of the effect is on women especially older women, especially low-skilled women, and I guess that is what they call the expansion group. So in some sense, it is a very dual labor market, and again, this is very similar to what you see in the labor market like Italy or like France where the people who are in and they are protected are not carrying much of the cost of any shock, and most of the cost is on female underemployment and the high unemployment rate. So, that was the first question.

Now, what about the second question? One that I think is the million-dollar question: how different would the experience of the Japanese economy be if the Japanese labor market was as flexible as the US labor market? So, take the Japanese economy in 1991, subject it to the same shock that it experienced, but change the labor market institution, make the labor market institution as flexible as the US labor market. What would have happened to growth and employment in those years? Now, obviously, this is a very tough question, and it might require to think about economic models, a little bit more structured, and for some structure on the data, but in some sense, this is what you really would like to know. If the counterfactual...to put the US labor market in Japan is too much, you can think about somewhere in between; think about a labor market that is not like the US one but it is a linear combination of US and Japan. But I think in some sense for policy, this is really a good question, because the next time a negative shock arises, that is what policy makers would like to know.

Finally, there is a question of the benefits of rigid labor markets. There is this argument in the paper that rigid labor markets benefit both employer and employees, and in particular point to or argue that there are two main sources of benefit of rigid labor market. One is that with lifetime employment, employees have the strong incentive in investing in firm-specific human capital and without these contracts they will be much less incentive in investing in firm-specific human capital. Second is there are negative reputation effects when workers are fired.

Now, I am saying that this is where I would like to see a little bit more analysis. It is not clear to me that you need lifetime employment to add an incentive for investing in firm-specific human capital. Presumably there is heterogeneity across firms because of their different technology and different production function, in the benefit of investment in firm-specific human capital. I do not see why you should have a homogeneous rule that forces lifetime employment on all firms, irrespective of the benefit or the cost. Presumably there is also heterogeneity across workers on how much the value lifetime employment, both across workers as well as within the lifecycle of the worker. And if there is a trade-off between job security and wages, you might have enormous benefit in efficiency of allocation of labor if you relax this constraint. So, it is not obvious to me that you need a generalized norm that is the same for all workers and all firms; especially because firms can always offer... if this was really beneficial for firms, even without the constraint they could always offer, for example, multi-year fixed contracts

where they guarantee to a subset of workers job security for the certain number of years, maybe in exchange for lower compensation.

So, I think this is probably the part where more work will be useful. I do not know whether it belongs to this paper or to a future paper, but in some sense the unique feature of the Japanese labor market makes it such that answering this question will be particularly interesting.

Finally, I am not sure I understand the second motivation for the second source of benefit of these rigidities, which is the lack of negative reputation effects, for workers who are dismissed. I think if I understand correctly, the reason is that the story is the following. Somebody gets fired, maybe because of an exogenous downturn for the firm, and their wages suffer permanently because other employers will put a stigma on that worker losing their job. Well, if losing their job is caused by some macro shock like the great recession, on average I would expect that they are not that many reputational effects, in the sense that employers will know that the workers who were fired in 1995 because of the recession were fired not because they were bad workers but because of some exogenous shock. It is a complicated issue; that there is some work by [Poroi and Tim Berkerinaudible] that shows that for the US labor market you still have wage losses years after you lose your job. So, it is not obvious exactly which way we will go, but indeed this is one great question that you guys should probably look at given your data.

So, overall I would say this is a first step in what promises to be a very interesting research agenda, and of course the first set of results is both interesting and important.

Prof. Kashyap: Thank you. Do you want to take just one or two minutes to respond and we will collect the other questions?

Prof. Kato: Sure. Let me just thank you for your very useful comments. These reputation effects I wish I have explained it more clearly and fully. I think there are two reputation effects we are talking about: the negative stigma to those workers who were fired, and some original workers, so forth, that might carry over time; the second is when the companies decides to continue this system, really investing workers and really tap into their local knowledge a lot, then they need to recruit the right kind of workers. These companies' reputation gets affected by firing. So, that would really make the system less efficient in the long run. So, that is really we are thinking, but we did not

explain it very well. What about Europe and so forth? We have this long-term research agenda, as we try to include Germany down the road. It is not even starting, but I completely agree, and I think it will be interesting to do some comparative studies. Thank you.

Prof. Kashyap: Right. We will go around the table this way. So, David, you are first.

Prof. David Weinstein, Columbia University: Thanks. I think this is really nice. I want to just follow up on one thing that Enrico mentioned and I also make a second comment. First, I think it would be really neat to try to get some handle on the impact that this has on at least unemployment in Japan.

Let me make one suggestion. There is an old paper by Gordon—I cannot remember which Gordon it is; I think it is Robert in the *Economic Journal*—that might be really relevant for this. What he does is he decomposes the variants in the wage bill in United States and in Japan into the variants of wages, the variants of hours and the variants of employment, and in that early study what he finds is that Japanese firms have a lot more wage volatility and a lot less employment volatility relative to American firms. And that would kind of give you a little bit of handle on trying to do a comparison, and if you could update that a little bit, it probably would not take you much time to compare it to the US today. Then you could say, okay, if Japanese firms cut the wage bill by “X” amount during the recession, if they have done that in the same way that American firms would have done that, how much less employment would there have been. Obviously, it is not a careful full blown econometric exercise, but I think it is a good first pass; kind of saying this is what the impact of permanent employment was on employment. So that might be one thing you might want to do.

The second thing is to link back to Kenn’s paper. One of the things that is also occurring in this market is that there is a big increase in part-time workers. What you are saying, I think, especially, is that there a lot of especially women workers who either got the sack or I do not know what, but were terminated or did not stay on their positions. That is probably got to have some implication; if a lot of those workers stay in the labor force but just switched to being part time then there are probably some implications for what that means for part-time workers. So, you could do a quick calculation, right; was the rise or the decline of these women workers largely offset or offset by a rise in part-time workers. That might be another thing that you could do that kind of links, what was happening, the rigidities in the regular employee and regular employee labor market.

That was what was happening in the part-time market. Maybe also you and Kenn can talk a little bit about these linkages.

Prof. Ulrike Schaede, UC San Diego: So, I have a few comments. I might not be entirely fair. You could just say we do not want to do this in this paper, but I just looked at changes in labor from a corporate perspective and what are companies allowed to do. The externalization of Japan's workforce is a big part of that story because we know now that 35.5% of the workforce are kind of part-timers and it used to be 15.

So, as a student of this sort of thing in recent years, I am surprised to see you end your data in 2002, which is fine for the purposes of this. I am wondering whether you want to maybe write this all in the past tense; by the time the book is published it is 2010, the government is changing the labor law yet again, and did change in 2003, which you do not mention. That is fine, I am not saying that you should mention that. But that you might want to hedge against questions like mine, which is, well, has not the 2004 revision of the labor standards law changed this yet again? So you might just want to be hedged against it and limit maybe the expectation of the reader with what precisely you are doing, so that I cannot ask mean questions like this.

The second question I would like to ask is: you treat this whole story as... there is a supply and demand side to this. The companies want to do one thing, but people are also changing in Japan. There is a big story about this and the recent white paper of the Ministry of Labor titled *Work-Life Balance*. A big reason, at least anecdotally, why tenures have been truncated is that people leave... in order to work for Goldman Sachs! No, that was last month's story. But until recently, they would leave Mitsubishi Trading Company for five times higher salary, because the net present value of that higher salary was worth more than a lifetime employment contract, which was becoming untenured.

So, there is a story about how people... You may not want to talk about this, but you might want just position your paper in a more [*inaudible*].

Prof. Takeo Hoshi, UC San Diego: My comment is similar to a second point David made, and the last point Ulrike made, and maybe actually the same thing.

But employment is a match between the worker and the job. I think Kenn's paper is focusing more on job side and this paper looks at the employment from a worker's side. But I think you could look at this change in employment practice from the job side as

well. For example, you find these mid-career female employees, they are left out from the lifetime employment recently. But is this the story of this particular workers, as you say here, or the particular jobs that these workers were more likely to be in, like disappearance of office ladies that they Kenn talked about yesterday? So, I think if it is possible to look at these things from a job's point of view, it may give us additional angle to look at the change.

Prof. Kashyap: Kenn?

Prof. Kenn Ariga, Kyoto University: Well, Takeo has told my topic almost. I like the expansion member story of the lifetime employment system, the last line of the paper, very much. As I like it so much, I would like to see if there is another dimension of distinguishing original members and expanded members. What I have in mind is something like... system engineers hired at major banks were supposed to do the integration of the computer system amongst the banks when they were merged, etc. As you know the Tokyo Mitsubishi seems to have problems in integrating the system after so many mergers, also that might mean the skill dimension of the expansion members could be somewhat more informative. Thank you.

Prof. Kashyap: Okay, let me make one other suggestion which is just to put a little structure on this point Enrico made about the employment and growth connection. It seems to me you could also try to do some sort of firm calculation that looks at the firms that were most prone to retain people and try to get some sort of normal estimate for what you would expect and then see if the firms that were unusually loyal grew differently than the ones that were less loyal. You have the names of the firms, right? No.

Prof. Kato: We do not have link data so we do not know what companies these people are working for. So, we cannot match that with the company data.

Prof. Kashyap: You cannot do anything. Okay, so there is no hope. Alright. I take that back. Alright. So, why don't you go ahead and respond.

Prof. Kato: Sure.

Prof. Kashyap: Okay. That is right.

Prof. Kato: That is great and very useful comment. Some of those comments were actually addressed somewhat in the paper which I did not cover, such as the cohort analysis. We generated probably five, six figures which really focused on the specific group, like your comment about the females and the really sharp decline in the percentage of those lifetime employment workers for females for the first five years. They were consistent with the M-shaped labor force participation rate of women. There are some supply side issues where indeed in the paper, probably not fully but some of them are already there. And 2003 and onward, we do not want to do that because really the great recession ended there that so it is really a sort of different regime, so I think that is going to be a different kind of paper.

A number of question on the part-time workers, we do have a section on the non-standard employment—a short section I must say. But why we did not pay a lot of attention to this is two-fold. One: if you look at the trend—it is actually a Figure 11 in the paper—percent of non-standard employees over 1982 to 2002, there is a lot of action for women and you all interested in women when it comes to part-time, but the increase in part time employment actually started way, way, way back in 1982. There was really no major change during this time period where we are interested in. I mean, some more interesting important underlying issues here, but we did not think that was really specific to the lost decade. So, that is the part-time issue... Yes, so, non-standard supply side. Okay, I think this is...

Prof. Kashyap: Okay. Thank you. We have actually got ahead of schedule, I do not know how that happened but we will now move to the last paper, Joe Peek.

Prof. Joe Peek, University of Kentucky: Okay. Thank you. Okay. Thanks for having me here. So, I am kind of interested... it is a very much related to the Hoshi et al paper. I am interested in the effects of bank lending.

What are the objectives? First, to have an idea of what are the effects of increased bank loans on the subsequent performance of distressed firms, I want to compare the pre-bubble period to the post-bubble period, 1980s to 1990s. And the question is: to what extent did the banks contribute to the recovery of viable, but temporarily distressed firms, so these are the firms where the distress was not really the zombie firms. These are ones where there is some hope. And if they did help, was the contribution primarily from main banks, which you can argue would have better

information about the firms and more of a commitment to the firms, or was it about the same sort of effect from increases in loans from secondary banks?

What might be the beneficial effects of increasing bank loans to these firms? Well, if you are economically viable but temporarily distressed, then it might help you overcome that problem and help you recover. It could provide the resources needed to enable the firms that are operationally distressed to undertake operational restructuring to improve their performance. At the macro level, operational restructuring will reallocate resources in the economy to more productive uses. So, it is good for the economy as a whole.

What about the bad news, the adverse effects? Well, what if instead of helping, you hurt, by allowing the zombie firms to continue to operate; by insulating them, basically you are insulating them from market forces; so they do not have to die or they do not have die officially. What about the ones that are not zombies but are temporarily distressed? Well, again if you insulate them from market forces they might not go through the pain and suffering associated with restructuring that would help them and the economy in the longer term. So you could end up with the misallocation of credit, which would reduce competition and reduce productivity.

Why might the post-bubble period be different? Well, there was a widespread deterioration in firm health. Widespread banking problems, ineffective bank supervision, which provided perverse incentives to banks in terms of their lending, and the development of the corporate bond market, which has been talked about before.

I want to skip through this because we do not have a lot of time. So instead of boring you with details about the prior literature, I will jump straight to boring you about the current paper.

For the data, the PACAP data are used for the firm balance sheet and income data, and stock price data used for book-to-market ratios, all using annual observations. We look at the 1980s and 1990s. We have the individual loan data from individual firms from each lender, and then *keiretsu* information.

We are going to identify distressed firms in exactly the same way, I think, as Hoshi et al. For financial distress, we look at the interest coverage ratio; having an interest coverage ratio less than one would be a bad year. So I am interested in “When do you enter

distress?” So you need to have a good year followed by at least two bad years. I am not picking up in the middle of this; I want to know when you enter distress. So you enter distress in that second bad year.

For operational distress, we looked at net income being less than zero through the same story. Then the question is... again we are interested in entering distress, so if you have been distressed, can you come back into the pool? So we have to have some measure “Okay you have kind of cleared up this mess, so now you are back in the pool from which you could become distressed again.” So we just say have three consecutive good years.

There are some preliminary tables that give an idea of the prevalence of financial distress and operational distress. You get similar patterns whichever one you use. The percentage of distressed firms with increased loans appears to subside somewhat at the end of the 90s, but hard to tell exactly what is going on there, and about 75% of firms have reached bottom using the individual distress measures, by the year in which they enter distress, so either in that first bad year or the second bad year, and over 90% by the second year after entering distress, so that would be four bad years. Also, I think you note that the persistence is greater in the 1990s than the 1980s in terms of being in distress.

What are some of the important issues that we thought about? Well, we got a measure of firm performance. So, one measure is return-on-assets, ROA. That is an accounting measure. The other one I thought about was well, maybe we need a market measure, because you can play accounting games, so look at the book-to-market ratio for the firms.

What about the relevant horizon? Well, that is not clear. We want to look at subsequent firm performance after entering distress, so we need to think about how long the period; and then the same thing with bank lending—how far back do you go? Do you just consider the year that you enter distress, or a couple of years before that? Because there are lags in the effects of bank lending, and also in the persistence of effects.

Another thing with loans that you have to be careful about is that an increase in loans and a decrease in loans: can you treat those the same? Certainly, with an increase in loans, that is an active decision. I applied for the loan; the lender granted me the loan. So, we each made active decisions. But if you have a decrease or no change in the loans,

it could be completely passive. Nobody did anything. We just sit there and made my payment, and so it is just the amortization of the loan. But it also could be a decrease in loan demand by the firm, or it could be “I apply for the loan, they rejected my application—they got harsh with me.” Or it could be the lender forgiving outstanding loans to the firm, which is just the opposite. They got very easy with me. There is this problem with decreases in loans, to know exactly what happened there.

In the specification for the different variables, look at the change in performance. You can see the change of ROA or the change in the book-to-market ratio. We considered two horizons: just the next year, the one-year period after entering distress, and also the two-year period after entering distress. We have some firm characteristics, main bank characteristics. We looked at the change in loans where we considered up to three years, which would be the year entering distress and the two prior years.

A set of annual dummy variables: one important thing is we do not have to worry about industry effects. We need to worry about what else is going on here. So what happens is each of these variables have been transformed by subtracting the median value for the firms in the focus firm’s industry. So I take the firm I care about, I look at the median value for these variables of all the firms in that same industry and subtract that, so that we are controlling for it. It is not a matched firm, but we are matching it to the industry to kind of pick up those industry effects. Industry dummy variables will not do that.

Let me skip to the results in table nine. There are four columns. Table nine has to do with the one-year horizon, the year after entering distress, using the ROA, and I used the minus for the book-to-marketratio so that we can interpret a positive coefficient as good or an improvement.

What is important here? Well, what we noticed—and again we are looking at the three sub-periods; looking for 1980s, the first half of the 1990s and the second half of the 1990s—so, what we find is bank health has a positive effect on the one-year change in ROA for the last period, the 1996 to 2000 period, and note that that last period is probably when the healthier main banks had less of an incentive to evergreen loans, that makes sense—that maybe when they increased loans, that helped. They were not busy increasing loans to deadbeats.

The DLNMB3Y\_POS, that is the change in loans by the main bank, looking at that three-year horizon, and here I have split positive and negative because of my concerns

that increases in loans may have a different meaning than decreases in loans. So, we get the positive effect for the ROA in the 1980s. That is sort of a standard thing that we think about. But we do not get it later on. We do not get it in the 1990s, and this is consistent with again the Hoshi et al paper where they are saying “Gee, it is main bank effects that seem to have deteriorated as we move on into the 1990s.” We do get this negative effect for the book-to-market ratio in the 1991-1995 subperiod, the first half of the 1990s, and that would be consistent with the ever greening of loans. If your main bank is giving increased loans, well, that is a good signal of decreasing performance by a market measure.

What if you have a decrease in loans? And so the positive coefficient means that the more you decrease loans, the more it hurts performance. You are seeing that for the main banks in the 1991-1995 period, although it is only at 10% level. So, that says a larger reduction of main bank loans is associated with worse firm performance.

The other thing that is kind of interesting is these are SB or secondary bank loans. So, if over that three-year period, you see a decline in secondary bank loans has a negative effect, which says the greater the decrease in loans by secondary banks, the better you are doing. And so, one story might be the consolidation that if I am a troubled firm, maybe what I want to do is get rid of the guys on the edge, so I have got my borrowing concentrated, so it is easier to negotiate in case I need help or a bailout.

Another story—I was talking to Takeo Hoshi Wednesday night at that reception—and he was saying “Well, maybe what you are picking up here is... the secondary banks are doing loan forgiveness. So, they were just saying ‘Hey, we are not really attached to this; we are out.’” So, that is a possibility. Maybe that was just the beer talking, or the beer listening, I do not know. But it seemed reasonable to me at the time.

Table 10 is just the same thing, except if you look at the two-year horizon for firm performance, you get similar types of things; positive effect for the two-year ROA in the 80s, you get the negative effect, but only the 10% level, on the book-to-market measure. You get the effect of... the increase in loans by secondary banks, that helps in the 1996 to 2000 period; maybe that is the story of secondary banks willing to go along with increasing loans for the main bank, because they do not have the same sort of obligation to the firm as might be the case for the main banks. And then again, we get this negative effect again for secondary bank lending.

If you look at the operational distress measures, you get very similar results. While increased main bank loans to distressed firms are associated with improved performance, as measured by the ROA during the 1980s, no such evidence is apparent for the 1990s. So it is there in the 1980s, but it does not seem to be there in the 1990s, things seem to have changed. Again, it is that declining importance of main banks, I think.

However, for the book-to-market measure, increased bank loans during this first part of the 1990s were associated with the deterioration in firm performance.

For decreases in loans, decreases in the main bank loans are associated with deteriorating firm performance in the 1991-1995 subperiod. Decreases in secondary bank loans are associated with improved firm performance in the 1980s and the first half of the 1990s. There is one thing, now that I have these results, I do not seem to get very much on the book-to-market measure for firm performance; I mean, partially that could be because what we are really trying to do is explain future stock prices in a sense. It is not quite that, but it is going in that direction. So that may be why we really are not getting much in the way of any effects on that measure. Thanks.

Prof. Kashyap: Our discussant is Jim Wilcox.

Prof. Jim Wilcox, UC Berkeley: I want to thank Anil and company for giving me the chance to discuss Joe's paper. Joe asked, what is really an important question here. That is: did the increase in lending by the Japanese banks actually help the distressed firms? In particular, he is asking whether the effects on either ROA or on book-to-market ratios were significant, and were there any differences before and after the bubble period, as in before and after 1990? Did it make any difference if the lending came or was restricted by either main banks or secondary banks? Did it make any difference whether we were talking about loans increasing or loans decreasing. I concur this is an important topic for that particular country, at that particular time; and as usual, it is a really careful execution of his chosen topic.

There is a very good and useful review of the prior literature, as is to be expected, I think. There is very long and sensible lists of controls. It seems like a pretty sensible sample to be using, and the right kind of time periods to be looking at.

What does he find? He finds, first of all, that this increase in lending seems to have raised ROA in the 1980s, and that is within the realm of results we have seen before for

the 1980s. However, this is really not much of a detectable effect by the time we are into the 1990s. So, there are pretty stark difference here between the result he was seeking in the 1980s and the 1990s. In particular, one of the things that is notable is: reducing loans seems to help raised ROA, at least until the 1990s. I think this does raise the issue—particularly because of the paper we saw earlier today about the role that lending would play in restructuring here, in particular—a lot of the kinds of restructuring that we have heard discussed, and that Joe mentioned, may require a very little in the way of loans. After all if you are going to shed a division, if you are going to fire your people, if you are going to reduce your salaries, those are not restructurings particularly that require more in the way of bank loans. If you need to modernize your equipment, if you want to expand into other markets, if you want to bring new products to market, that might require more credit. But a lot of the restructuring that we think about often involves some kind of a downsizing of prices or quantities, and may require very little on the way of loans. In some ways you might wonder if banks shutting off firms actually led to more restructuring.

One of the features that shows up is: there is no effect of *keiretsu* on ROA in any of these tables, as far as I can see. It may well be that the firms that are part of *keiretsu* basically behave differently than those that were not. And so it may will be that the *keiretsu* effects and some of the other variables really are more multiplicative effect. It really ought to be just in our completely separate subsample, perhaps.

Let us take a quick look at what data is used, and what we think we can learn from that, and why we might want to do things a little bit differently, and where we are going to need to be careful. First of all, remember, we are talking about traded firms. In general, these are large firms, and we know that they have issued stock in the public market, that is what makes them traded. Of all the firms that issue bonds, they are going to be in this club also. So, they are not, for example, the vast majority of the smaller firms that really are so bank dependent. Now, of course, in a bank-centric economy like Japan even an awful lot of the very largest firms really rely heavily on the banks; nonetheless, of course, we do have larger firms here. This is a sample of only distressed firms. The only way you get into this sample is to be distressed to begin with. So, we are not talking about the sample that is comparing the healthy with the distressed. We are just looking at the distressed.

One issue that would be nice if we could say something about, and though the paper talks about it I do not really quite see how we are going to do anything about it, is to

distinguish between zombies and what are sometimes referred to in the paper as “viable firms.” We have got “distressed firms,” some of which I guess we eventually learn are zombies, and some of which may recover. But in the data, it is pretty difficult to see how we are going to distinguish between the zombies and the viable firms here. We have got a criterion for deciding that they are distressed, but other than that we do not know really whether they are already dead or on the way to recovery here.

To look at the data, Joe is going to look at the ROAs or the book-to-market ratios, the two performance measures. In both cases what we are going to do, you got to make some kind of adjustment presumably for the fact of the conditions in the industry. For example, you might have a chemical firm and you would like to do something like de-median the data by the average value in the chemical industry. In particular years it is the median, which is fine, but empirical work is messy sometimes. Here is the kind of industries we are talking about. For example, take services. Services, it seems to me, runs a pretty wide gamut, and so you could be thinking of services being financial firms, it could be consulting firms. It could be all kinds of service firms. Another is chemicals; it must be some kinds of these vast differences between some kinds of portions of the chemical industry. Construction: right off the bat, it might be the difference between commercial and residential. Wholesale or take retail is one industry. If you are a car dealer, that is a lot different than being, for example, a pharmacy, but they are all retailers. I do not see how you are going to do an awful lot about that, but we do have to remember that we are just de-median-ing in this case. De-median-ing in hereby industry amounts.

And then there is going to be measurement error. We are using measures like ROA and book-to-market. In ROA, there are probably only two sources of measurement error there: return and on assets. And in book-to-market, the market may be accurately measured but I really doubt that book equity is going to be really an accurately measured variable here. So, we are going to have to just live with the fact that there is a lot of measurement error here. Fortunately, I supposed it is in the dependent variable which we confirmed ourselves as being perhaps not biasing our estimates, but nevertheless we have got some, I think. But what can we do?

What about these measures? We need some kind of a performance measure. You want something that is going to scale and have been already de-median-ed so to speak, but you want something that takes account of the size of the firm, perhaps. So, using an ROA or using book-to-market is a very sensible place to start, but let us just remember a

little bit here about what this ratio is. Suppose, for example, you got a loan that actually increased your assets—the denominator—and actually increased your profits and left your ROA unchanged. That is a recipe for a zero coefficient, many of which we find in this paper. But if you need the ROA unchanged, that may well have been a perfectly sensible productive profitable loan for the borrower and for the bank. The same thing is true with a loan; for example, it might have equal or at least offsetting effects or at least compensating effects on a book value and on a market value of a firm. If I somehow make them a loan and it raises both the book and the market value, perhaps proportionally book-to-market might not go anyplace, but that does not really say that there was no effect. And so, the question really here is in fact, did these loans lead to higher profits or higher market values? This is going to be a little tough to sort out. I am not exactly sure how to do it. I do worry that we could have effects that do not really change the book-to-market value, but in fact though it did not change the ratio, they were perfectly sensible loans to make.

It might be useful, given the coefficients we do have, just to get some idea of how big these effects might be. Trying to use sort of a back of the envelope calculation based on the coefficients in the paper, it looks to me if I understand correctly that what this... like one sigma change in loans, one standard deviation move in a loan variable seems to have changed ROA by 1/12 of its sigma. So, it looks like this effect might be worth thinking about, it is worth considering. But it might only be changing it by accounting for, say, roughly 8% of the movement of ROA here. So, whether that is a big move or not I leave it for you to decide. I think it was a good idea to try to separate the loan increases from the loan decreases, but the results we saw this morning pointed out that in fact, with a lot of restructuring it may well be that not only will loan declines be endogenous, but they might be very endogenous. It may well be that decisions to restructure are actually leading to these loan declines, which in some cases—in some of the strongest results in the paper—then lead to improved performance later on. It may just be that this is the national follow-on not only to the earlier paper, but to the earlier set of results, in that regard.

How long should we expect these effects to last? Joe looks at horizons of both one year after the loan is made and two years after the loan is made. You would hope that if you are doing a restructuring and you are really taking on a loan to undertake some rather fundamental change here, the effects are to be quite long lasting. How long is not altogether clear, but if we are really talking about restructuring in the sense of moving in the different product markets, by product or by location, if we are going to have a

completely revamped production process or something, presumably, these effects really ought to be really quite long lasting. That fact that they do not show up in the first year does not strike me as being particularly worrisome, but eventually they ought to show up and they might show up for a very long time.

Remember also that we are only looking at distressed firms. I think one issue to think about is whether we want to assess the effects of these loans as these firms became distressed. We are looking at the firms that are distressed. Is this a test that is really going to pick out what we are looking for? One possibility is to take the firms we have already got, and just look and see when they were getting the loans during the period while they were going in the distress. Did they have similar sorts of effects? And we might also just see, to what extent would this test show us, for example... did loans in the 1980s to non-distressed firms helped them on their way or not? That is, if we really think part of the difficulty deep into the 1990s is zombie lending—from zombie banks, perhaps, to zombie firms—what about when we had healthy banks making loans to healthy firms? Would we expect... during the healthy period, would we actually find the sorts of significant positive effects on ROA or book-to-market ratios that are really being hypothesized here?

It is easy to do the first assessment; you have already got the firms. Doing the second assessment is going to be a lot more difficult, because in the sample we do not have the healthy firms, we only have the distressed firms at this point. So, that is going to take a lot more work, and maybe more than worth doing.

Let me then try to just expand the scope here. One question then is: presumably the reason we care about this is because of the implication that this might have affected GDP. That is, do we think that these distressed firm loans, loans to these distressed firms, were they just the zombies that then led to firms that should have died continuing to walk and perhaps distort competition as Takeo and Anil have written about? Or, do we think somehow that it was going to reduce the credit that was available to other firms? If you really have banks that are under severe capital constraints, perhaps one dollar loan made toward a distressed firm might take away a dollar from a healthy firm. That is another possibility.

In the case of the United States at the moment, the argument apparently is being made that making this loan now will make GDP higher than it would be otherwise—that is, at

least for the next year... The argument would be that if someone makes a loan to a distressed firm then ultimately GDP will be higher at least over some short-run period.

So, then the issue is: do we think that ultimately in the case of Japan—let say before 2000 or in the case of United States after 2000—would such loans actually tend to raise GDP or not? Might not be the case that if you are in a sufficiently deeper recession, if you have sufficiently idle resources, might well GDP rise? It could be one possibility. Might it be the case, if somehow the financial system just is not working correctly and there is a very large decline in the aggregate supply of credit, might that well mean that in the case of a distressed loan, it might keep enough firms operating and avoiding debt losses associated with disruptions and so on, that GDP is higher? That is a possibility. But, I think all of us both in the Federal Reserve and over at the Fiscal Branch—harder to separate those these days—however at the Fiscal Branch of the government, they certainly are worried about, even if it does to help GDP in the short run, it still raises the issue about moral hazard and whether we can have our cake and eat it too. That is, is it possible to raise GDP in the short run with such a loan, and then somehow take care of moral hazard later? I do not know. I would refer to this as the Augustinian approach. You may remember St. Augustine said, “Lord, grant me chastity and purity, but not yet.” And I think that is sort of where we are with this loan. Thanks.

Prof. Kashyap: That is karaoke. Okay. So, now, well, do you want to respond particularly to that, Joe?

Prof. Peek: Okay. Thanks Jim. No, he has got some good points there, as usual. I might just mention a couple of things. One, near the end there, he is talking about... what about the horizon? So, if you just start thinking about these performance measures, we are looking up to two years later, but the further you go into the future, the bigger the disconnect, because you are using measures at the time of distress, so after three or four years an awful lot could have happened in between, so my guess is that you might not get much of any relationship—just too much else going on.

And then in terms of what about non-distressed versus distressed firms, I do not exactly compare them, but I indirectly compare them or partially do a comparison because, for example, the ROA of my distressed firm is compared to the median in the industry; presumably, in most industries, at least the median is a non-distressed firm. And so we are saying “Okay, if I increase... how did your ROA increase or decrease relative to

your median firm?” So in that sense, that was an attempt to try to do that comparison, but it is not a matched firm comparison.

Prof. Kashyap: Alright. We will start with Mark and then...

Prof. Weinstein: This way.

Prof. Mark Spiegel: Thanks. I enjoyed the paper. My comment follows up a little bit on some of the things Jim said. I think, particularly with respect to the endogeneity issues about what is driving the main bank lending behavior, I think that main banks can differ from other banks in more than one dimension, and that makes me a little uncertain in what we should expect to see in terms of your specifications. For example, if we think of a main bank as a bank that has superior information and is making a loan to a firm because it views that firm as a more viable entity, that might be correlated with a positive coefficient as you suggested, and you look for on a data and you found for the 1980s. But if a main bank differs from another bank in terms of its willingness to be the sort of agent that is going to lend to the firm when no one else is willing to lend to that firm, in particular when that firm is least viable, then we may indeed not see a positive relationship between main bank lending and the performance of the firm, because it is exactly the main bank serving that function—of the last guy that is willing to give that firm credit—that is leading to that increase in main bank lending.

Prof. Hoshi: My comment is probably the same as what Mark mentioned. People often talk about when a company got into trouble in the 1990s and recently, the non-main banks start to leave but the main bank is stuck with the company. Where your finding seems to be consistent with the story is that for 1991 to 1995, you found the main bank lending increase leads to lowering the market-to-book ratio. Here, instead of increase in main bank lending, if you just throw in the ratio of the main bank to a total change in the loan share of the main bank, and if you find actually a strong result, that may be interesting.

Prof. Weinstein: Just one question. It was not clear maybe in the paper... how you deal with bankruptcies in firms that run from distress and then die? A measurement that could bias things? The other thing I thought might be interesting to look at is something kind of simple, which is conditional on you being in distress; right, what was your situation five or 10 years later, and how would that vary across time, so you could kind of look at a fairly long lag or long lead time after the incident of distress, to see some

sort of long-term restructuring. Your data only goes to 2000; why is that? That is the third question. Why cannot you get...?

Prof. Peek: I am limited by the individual loan data, so that I have not got all these, I cannot go past.

Prof. Weinstein: Why does the loan data only go to 2003?

Prof. Peek: Because when I had purchased it, that is what I had and I did not have any money to purchase more.

Prof. Weinstein: Is DBJ or what was it...?

Prof. Joe Peek: No. It is a Nikkei NEEDS loan data.

Prof. Weinstein: I see.

Prof. Joe Peek: Yes, or it does not, yes.

Prof. Weinstein: Takao's data also stops in 2002, so that is the end of history.

Prof. Hoshi: [*inaudible*] can be easier to deal with them.

Prof. Peek: But there is another issue there too because as you move on into the 2000s, a number of different individual banks shrink a lot. So, you get all this concentration, and so you do not have all these individual banks in the same way that you did. You have got to worry a lot about the dramatic consolidation.

Prof. Schaede: The editors, I just have a question of clarification for this. The topic of all of these is to explore the 1990s. So, this is not necessarily a shortcoming, is it? I mean, it would be more interesting to compare the 2000s, but do you have a position on that?

Prof. Kashyap: The end of the bubble period was deemed... I do not know when. This project started in 2005, so by 2005 the cabinet office was confident, despite my objections at the time. I remember being at the meeting where this was balanced, and saying "Do not count your chickens," but GDP growth turned positive in 2003 and 2004.

So, I think it is reasonable that the date of the end is about 2002 or 2003. I think that would be kind of a conventional dating.

I was going to say something, Joe, on the timing. I think at least when you do the zombie calculations the way we do it, they do not really look like they pick up until about 1993 or 1994. So, the timing on the ever greening does not line up with what I would have guessed. I would have thought that you would find more of it from say 1994 to 2000 than you would have found 1991 to 1995. I mean, in 1991 and 1992 the economy was not doing so well, but the banks were nowhere near insolvent at that time. They were still sitting on huge capital gains and land and everything else. So, that was a little surprising.

The second thing is: I wonder whether you can explore the zombie conjecture a little bit just by taking advantage of the fact that manufacturing really looks very different than everybody else. I do not know the right way to split up all the other industries, but I think almost all the calculations suggest that the manufacturing guys—maybe because they had to compete internationally, I am not sure what the right explanation is—but almost all the kinds of calculations that have tried to look for zombies suggest manufacturing is very different. So maybe you could do manufacturing all the way through, versus a change in some of the other industries, and look a little bit more specifically at that. Ultimately we should tie your paper with Ulrike's and Takao's, just because I think you have got a very nice pair of papers that really points to changes in the way the restructuring worked, that certainly was one of the conjectures that started this project that could have been important. So we will need to closely coordinate your papers in next generation.

Do you want to give me any reactions? Okay. Thanks.

Prof. Peek: A couple of things: as Mark pointed out, this is sort of difficult to figure out... I am lending because I have private information, I got better information about you as a main bank, so I am going to go with winners. But then you would think that you would see all these positive coefficients. Then there is this other thing about... I am stuck with you and so I have got to do the best I can here, and I am going to try to help you or prolong your life. Again, that has to do with perverse incentives that Eric and I have written about. Takao's point about looking at the change in the main bank total loans: we do have as a ratio, not the change in ratio, there is the ratio of main bank total loans which is in there as a control variable but not the change. And another thing as a

substitute that I have looked at and will go back to is we have constructed a Herfindahl index of all the lenders to try to get out how concentrated it was. One thing that might be going on is—and some other stuff I have kind of messed with a little bit but I have not fully developed—is there are reasons why you might concentrate your lenders, so that you do not have to negotiate with the whole lot of guys.

It is possible that maybe something like a change in that might... I am not sure of what horizon. But are you increasing the concentration of the banks from which you borrow? Maybe something about... Takao's point about this change in main bank loans in terms of getting an increasing share, to get at that same thing.

David's question about bankruptcies, how do we deal with it? We do not deal with that explicitly. There are not that many bankruptcies of listed firms in Japan anyway. What we do is I am looking at... the point that you hit "distressed," it is possible for you to keep going down. So, we will have a real serious problem, I think, if we said "Let us look at your improvement from your bottom point," because then everybody would improve, and if you did not have a bottom point, you get thrown out, and that would be the bankruptcy guys.

So, I do not know that is the major problem in the sense of one, a very few bankruptcies—surprisingly so—and second, we do have everybody that hit distressed, you could keep going down for quite a while after that, even if you got loans or did not.

The question about looking five or 10 years later is sort of this thing about this horizon. But you also have a problem; if you are interested in five and 10 years later for the firms—and perhaps the most interesting period would be the last half of the 1990s—that is difficult to do right now, in terms of grabbing your data. I do have later data in terms of ROA for example, and the detailed variables, so I can look at that; it was the loan data that prevented the other. PACAP is very slow about getting new rounds of data out.

Anil's point: the stuff that Ulrike and I did, we find a lot of really weird or perverse behavior was in the last part of the 1990s. One thing to do is go back there and maybe think about the split... the time split might be a little different. Maybe instead of looking at the last five years, look at the last seven or eight years, either grouping 1991, 1992 with the 1980s, or leaving them out, and just seeing what happens there. Then the manufacturing, that is easy to do. Also, Jim brought up the thing about "Maybe you have got all these retail firms." Earlier someone made a comment in one of the other

papers along those lines, that yes, there is retail but there are other kinds of retail. So, there may be some differences there.

I tried not to aggregate... the PACAP has all the industries, and I tried not to aggregate in more than I had to just to make sure that the pool of same industry firms... that I had a big enough pool that I did not worry about grabbing the median out of there, as in being some weird firm. But I tried to leave quite a few industries in there. But it is easy enough to run it separately for manufacturing, because that is a lot of firms, and also to see if maybe manufacturing did not change in the same way through the 1980s and 1990s. Thanks.

Prof. Kashyap: Okay. Looks like we are coming to the end. Let me just reiterate; you will be hearing from the editors about your papers shortly after the beginning of the New Year, and then you have about two months to try to make a final push. If you have any questions, please let us know.

Let us close by thanking Mark Spiegel for hosting us in San Francisco. In fact, this was an incredibly bad time for them, since the other meeting is next week. Again, I do not know if John is here. But anyway, thanks to all the staff at the Fed.