

Interview with Töres Theorell

SJWEH Suppl 2008;(6):7–14

Töres Theorell (TT) talks with his colleague, Aleksander Perski (AP), about his life as a scientist and ideas behind the demand–control model.



AP: We have a possibility to give a bit of a background behind the idea of the demand–control model, how it came about, what led to it, and what you thought when you were working with these issues. I think this will be an opportunity for younger scientists to see how interesting ideas develop.

TT: Where do we start?

AP: I think we should start, very shortly, with your background. You were born in Stockholm?

TT: Yes. Two older brothers, both of them physicians. My father was a biochemist and my mother was a professional musician.

AP: So your father was not a physician, he was a chemist?

TT: He was a physician, but very soon during his education at the Karolinska Institute he became convinced that he should be doing biochemical science; so he spent a couple of years in Berlin in the mid-1930s, which was of course very

AP: It was the center of science

TT: Yes, exactly, and also the center of Nazi developments. So it was really tricky to be there, and very strange. He, of course, was appalled by the Nazi development, but his tutor there was a very famous Nobel prize winner, and he, despite the fact that he was a Jew, was untouched by the system and survived.

AP: What was his name?

TT: Warburg, Otto Warburg. It was during this particular period that my father also made his discovery, which led to the Nobel prize in 1955.

AP: And your mother was a concert pianist?

TT: Yes, she was a concert pianist, and also a teacher of the harpsichord at the College of Music here in Stockholm. She was also the harpsichordist in the Philharmonic Orchestra. So music has been a part of my life. When I fell asleep at night, I could often hear my mother practice difficult parts of a piece of music, and often my parents played some sonata by a popular composer at the time, and these are, you know, like imprints on me; so it is part of my whole security system.

AP: The house was also filled with scientists, coming and going, and visiting students.

TT: Well, in a way, yes. Of course, my father had very long workhours, and I remember always waiting for him to come home for dinner. He was always late, but we were still having dinner together. So we had to wait until he came. That was a very common scene. And my parents were also very often away at dinners. They had extremely active social lives. They traveled a lot also. The traveling was different from today; for example, when they went to the States, when I was small, they had to go by boat. They were away for long periods.

AP: Did you go to your father's laboratory and spend time there?

TT: Oh yes. I went to my father's laboratory several times, and also, you know, in a way, this was part of our social life as well, because the collaborators were often from abroad. For instance, there were a lot of American, French, Belgian, British, Japanese, and Italian people. You know, from all over the world, and twice a year there were gatherings. One was Christmas, when there was a celebration with all the staff and I remember myself singing solo. The echo in the marble hall. Typical Christmas songs, *Det strålar en stjärna* (*A Star Is Beaming*). And during the summer, they invited all the staff, which means everybody—also the janitors, for instance—to the family's place in the archipelago, there was a big group of people coming out. My triumph once, when I was something like 11 years old and everybody was sitting at the pier at the water and I was using my casting rod, was this pike, 6 kilograms, jumping in front of everybody

AP: Moments of triumph. I am always wondering that you are one of the very few people whom I know who has absolutely no prejudice against other nationalities or

occupations. Everybody has small prejudice or thoughts about this, but you are one of the few who never show any sign of it, everybody seems to be a person. Can we trace this to this fully international house with all these people?

TT: Yes, I really think that this is quite important. You know, my father and mother. They, of course, were, as you would say today, politically conservative and voted for the right party, but, at the same time, they were often speaking at the dinner table about the terrible Nazis, and how terrible it was to, you know, be mean to Jews and to other people. So they were very clear from that point of view. They would never allow any thoughts pointing negatively at different nationalities. Our family also had exotic ancestors. I have a red Indian among my ancestors... and a lot of our friends, of course, came from all over the world.

AP: The shape of your nose—this is from the Indian?

TT: We believe so.

AP: It looks like you had some strong forces in your family. One is science and another is music. Then, as a young person, did you have to choose between these when you thought about your future carrier?

TT: Yes, but I don't think that choice was so difficult really, because I realized that studying medicine gives a person very diverse opportunities, and this is exactly what I am telling my 19-year-old son. There is such a diversity that you can go into all sorts of activities if you start with this basic training. But the other thing is that music was extremely important and was legitimized in my family since we all played an instrument. I was playing the violin. It was not only associated with good feelings, there was also an element of irritation and stress that I did not like particularly. Then I found my own music, which was singing. That was something I did dream about. During that period, I was singing in a student musical performance at a real theater (Södra Teatern) and had some other singing experiences as a soloist.

AP: I have a feeling that you have never actually chosen, you have just done both things, more or less?

TT: In a way. That's true.

AP: Medical studies in Stockholm?

TT: Yes, I started in 1960, and I became a licensed physician in 1967.

AP: Did you do some research during your studies?

TT: Not during my studies, but, immediately after becoming licensed, I started working in the department of medicine at the Serafimer Hospital, and my boss there was Gunnar Biörck, who was a well known figure at the time in all of Sweden. He had been the pupil of my father in biochemistry. He was convinced that I should

be doing biochemistry. So he assigned a laboratory assistant to me to do great science.

AP: In general medicine or cardiology already?

TT: No, I was in general medicine, mainly cardiology clinically. But my biochemical work was supposed to be about oxygen transportation, which is relevant to cardiology. I soon found that this was impossible. I could not be a good biochemist if I intended at the same time to be a good clinician. So I thought I really had to do something more clinical in science. Gunnar Biörck wisely understood this. I announced that I was interested in psychosomatic medicine, which he also was interested in. Then it so happened that a Californian psychiatrist, Richard Rahe, was spending 2 years at the Serafimer Hospital, and I was adopted by him—so that is where it all started.

AP: So you tried to be a biochemist, but it did not really work. Then you discovered that you were much more interested in patients, in people, in their stories, and their life stories.

TT: Yes—the first studies I did with Rahe were simply retrospective recordings using questionnaires asking people what had happened during the periods preceding their myocardial infarction.

AP: You were at a department of medicine, you had a lot of basic scientists around you, you had a home where basic science went on full speed, and you moved into a “soft” area. You were interested in people's history, how they live. Was that also a bit of a protest for you, to move into social areas, or was it simply because you had a basic interest in people?

TT: That is something I really do not know myself. But, of course, there was, at the same time, societal protest and nobody could be unaffected by it.

AP: Are we talking about 1968–1969, the Vietnam war movement?

TT: I became a licensed physician in 1967, which is of course close to 1968. Gunnar Biörck himself was extremely conservative in his political view, but, as I noted earlier, he had radical sides and attracted the most radical students and physicians at that time. My very first publication (as one of many co-authors) concerned gypsies. A group of gypsies who had come to Sweden and were examined medically. The paper was published in the *Journal of the Swedish Medical Association* (in Swedish).

AP: And what was shown in this publication?

TT: It showed that these gypsies were in a very poor medical condition. We argued for an improved reception of these immigrants.

TT: There were two elements in my doctoral thesis. One was the exploration of which life events had typically occurred before a myocardial infarction. And

then we followed patients week after week after they had suffered a myocardial infarction. We did interviews with them, recorded all the events that had happened during the past week. At the same time, we recorded their urinary catecholamine excretion every week. Blood samples were taken for the analysis of triglycerides and other lipids. Triglycerides, for instance, are very much affected by depressive episodes as they occur. Uric acid (which has an interesting relationship to energy production in the body) was clearly elevated during periods of arousal and energy mobilization.

AP: The catecholamine measurements were something new.

TT: That was a new thing. That was my first collaboration with Lennart Levi. We did experiments injecting adrenaline—quite substantial dosages repeatedly over half a day. All sorts of metabolic parameters were followed.

AP: That was cardiac patients or healthy people?

TT: No, these were just healthy people, like me and students.

AP: So you injected quite a lot of it into yourself as well?

TT: Yes. I remember that the adrenaline induced the feeling that I should be angry, but mostly there was nothing to be angry about. If somebody slammed the door, which was normally quite innocent, I “used this opportunity” to yell and took it out on that person. And then I also remember, quite interestingly, that I fell asleep in the evening, but I woke up repeatedly. So I had a very disturbed sleep despite the fact that the injections were made from the early morning until noon. A lot of metabolic effects must have lasted throughout the whole 24-hour cycle.

AP: And that was part of your dissertation?

TT: Yes. I do not think the ethics committee would have allowed us to do that now.

TT: So let’s go back to your very interesting question: “What was the driving force?” There was an element of protest when they tried to push me into my father’s discipline. I found that I did not want to do biochemistry. I wanted to do something in clinical medicine, and I was very fascinated by psychosomatic medicine. I read the work of Stewart Wolf, as well as several other old psychosomatic medicine books.

AP: And what was so fascinating about it?

TT: At the time, you know, everything was pushing towards a very objective science. So this was an era that had very little psychosomatic medicine in it. It is different today.

AP: Thanks to you and your colleagues.

TT: Yes, there were several of us. One of the colleagues I published with during these years was Ulf

Lundberg, who is nowadays a professor of biological psychology at the University of Stockholm.

AP: One of your often-cited papers from this period was published with him.

TT: Yes, he was a very good contact. He was very skilled in mathematics, and he could describe some of these phenomena in elegant ways. After him, there was Marianne Frankenhauser. And another person who was, of course, very important for me during these years was Lennart Levi.

AP: So you could use objective science for measuring subjective states?

TT: But then we came to 1978, which was an important year for me. I had become an assistant professor in 1973. I had spent a year in the United States with Stewart Wolf doing somewhat similar things. There, I analyzed the situation of myocardial infarction patients. They had been followed for many years. A group of clinicians had been doing physiological recordings, along with taking their detailed histories.

TT: One of the studies was performed on mono- and dizygotic twins who were discordant with regard to heart disease. One of the twin parents had heart disease and the other did not.

AP: So it was interesting, why?

TT: Yes, exactly. So we looked at cardiovascular reactions when talking about difficult things. Also hormonal reactions were recorded since we drew blood samples repeatedly.

AP: Were they different in their way of reacting to the world, or did they have other events in life or other lifestyles?

TT: There were similarities and dissimilarities. I remember, for instance, that the likelihood that a twin would blush when we talked about difficult things was extremely similar in the monozygotic twin pairs regardless of what life experiences they had had.

AP: So you have been in the States, and you are now an associate professor.

TT: In 1974, I also started collaborating much more with Lennart Levi. I had also done so before, during 1971–1973. So I knew him before 1974.

AP: Maybe we should say a few words about him. At this time, he was establishing stress research in Sweden. Right?

TT: That’s right. He was a very remarkable person in the sense that, long before he had even done his dissertation, he was able to start a laboratory of his own, which was a kind of institution. It was quite remarkable at the time. He made an alliance with the professor of internal medicine, Lagerlöf, at the Karolinska Hospital and with the professor of psychiatry at the same hospital, Börje Cronholm, and these two were interested in

psychosomatic medicine. One of the reasons why they believed in him could be that he had spent some time with Hans Selye in Montreal, who had a formidable worldwide reputation.

TT: But fairly early in the late 1960s, I also started more real epidemiologic studies. The first was the study of building construction workers. It included a cohort of 8000 construction workers, not all men. The idea was to have a big enough sample so that we would be able to see sufficient numbers of myocardial infarctions prospectively in this cohort and relate them to life events. We could not record life events at one point in time and then wait for 20 years. It would have been meaningless.

AP: So if you have a big enough cohort you can see all the relationships.

TT: We did that, and, you know, that was sort of an important experience for me, because then I had to even reject some of the beliefs that I had had earlier.

AP: So what was happening? You saw this big data file, you saw that life events are important but . . .

TT: Yes, that's right. Life events matter only sometimes, rather than all of the time.

AP: And it becomes very interesting. It seems as if you move to the next stage. The first stage is that personal history might be important for myocardial infarction. But then you came to the stage where life conditions themselves—work characteristics, society characteristics, the social class characteristics are very important for disease development. This is like the establishment of a new paradigm. You come from individual factors to more societal factors. The paper that was published in the *American Journal of Public Health* in 1981 became the all-time classic of your citations. It is the most cited paper you have written.

TT: Ohhhhhhhh, I didn't know that.

AP: Cited almost 600 times, and it is also the most cited paper even now.

TT: How interesting!

AP: We should look at this paper carefully. The paper is called "Job Decision Latitude, Job Demands and Cardiovascular Disease: a Prospective Study of Swedish Men". You wrote it together with Bob Karasek, Dean Baker, Frank Marxer, and also Anders Ahlbom, who is your colleague here in Sweden.

TT: I could tell you something rather interesting about that particular paper but . . ., let me just say this before I forget it, there is one logical red thread in all of this, which is the lack of control and the exertion of control in one's life. I can see that this is part of my whole scientific life in various ways, but we can go back to that later.

AP: You mean that you already saw it when you were doing your life event studies?

TT: I definitely think so, and this is part of life event research. For instance, both Johannes Siegrist and I, independently of one another, were using life-event characteristics scales, one of them based upon the issue of control.

AP: Which events are controllable, and which ones are not controllable.

TT: Back to this paper from 1981, in 1978 I had moved to the department of social medicine.

AP: Sweden is a perfect country in which to do this kind of study. You could ask questions to the whole population about the relation between health and living conditions.

TT: So then, one day in 1978, Bob Karasek came into the picture.

AP: Who invited him? He just came?

TT: No, he came because he wanted to see me. He had seen that I had published papers on psychosocial factors and the job situation. He described his model, which was an interesting synthesis of two research traditions. The first one was the stress model based upon Marianne Frankenhauser's, Lennart Levi's, Hans Selye's, Walter Cannon's, and Claud Bernard's research.

AP: And the idea was that our organism responds to increased load.

TT: Yes, if you increase load, you get more stress.

AP: One tradition is that our body responds to load, so, actually, in the popular sense, the term stress becomes a synonym for load. Too much to do, too heavy work, too hectic, too much responsibility. Right? In the 1970s, when I came to this research, the question was "If stress is load, why are there so many people who can go on, who can work very hard, and do a lot of things, and they are not stressed and will not get sick?" So something happened in the 1970s in the stress tradition, for which you came to some kind of solution. Right? The effect of load on the organism depends on the resources that the organism has for coping. And here comes your ideas about control.

TT: It was another element in this that was, I think, equally important, although it was actually similar in content, and this, of course, was the fact that stress had been associated traditionally with the upper classes.

AP: Too much to do . . .

TT: Yes, yes, and this was such an important idea that it legitimized things like high salaries for high-level executives because they were so stressed. But, during this period, there were papers popping up in the literature. One of them was by, interestingly, Aaron Antonovsky, who wrote a paper based on an epidemiologic study

showing that myocardial infarction is not an upper-class disease; it is actually quite the contrary, more common among the lower social classes.

AP: Actually, I was surprised that you cited him in your famous paper from 1981.

TT: Yes. And then, of course, Michael Marmot came along saying similar things, based upon British epidemiologic studies. That, yes in the past, maybe the upper classes have had more heart disease, but nowadays this is not true in most Western countries.

AP: But what Bob Karasek did, you said that it represented two traditions, one with stress and the other . . .

TT: . . . and the other was the alienation theory. Bob Karasek had been trained as an architect, which is interesting, because he was able to look at things three-dimensionally. He could construct graphs describing, you know, the influence of many things at once on one outcome. His beautiful graphs were one of the reasons why he caught the eye of people. And he is also a very, very able person who has brilliant ideas about lots of things. So he combined the stress research tradition with the alienation theory, and that is the basis of the demand–control model. He had been influenced by Bertil Gardell and by Marianne Frankenhauser. The reason why he was influenced by them was that he spent some time before he met me in Sweden. He came to Sweden as a Fulbright grant holder in the mid-1970s and worked at the Institute for Social Research. So he knew people like Sten Johansson and Robert Erikson. This is also the reason why he discovered me. Their institute had collaborated with me in the production of one paper.

AP: And he actually managed to write two papers before you met him, but they were not recognized as important. Something happened when you two guys came together. What?

TT: What is my role in this? He had actually started his model, but my role was to help him relate it to hard endpoint outcomes. And I had some impact on him in formulating physiological theories about why conditions of high demands and low control affect health. We published a paper in the *Journal of Human Stress* with Scott Russel. This introduced measures of anabolism and metabolism in relation to the model.

AP: This was before the 1981 paper. This paper is not so widely cited because it was published in a small journal. But in the 1981 paper you try to understand why low decision latitude may be dangerous, and you say that, if no action can be taken in the face of high load or if other desires of an individual are frustrated, then the unreleased stress may have adverse psychological and physiological consequences. This is actually the idea one can trace to Selye, because he also talked about this loading of the organs or activation that does not have release.

TT: It also relates a little bit to the work of Holger Ursin and Hege Eriksen in the new stress model, and I mean the important component that is expectancies. This also relates to other kinds of theories.

AP: Do you actually remember how your work with Bob went, and how you came to the conclusion that you might do a paper together?

TT: We had endless discussions. We really spent a lot of time together. First of all, of course, we met several times in Stockholm. But then Bob managed to get a grant from NIOSH, which made it possible for me and my family to spend 4 months in the States working with him at the University of Columbia.

AP: That was in New York?

TT: Yes. And then there were other people around—Peter Schnall, Joseph Swartz, Carl Piper.

AP: And you suddenly had a quite a lot of data and could start discovering what you should be looking for.

TT: Yes, that's right. There were both American cross-sectional data and this particular Swedish cohort. That analytical work was actually mainly done in Sweden. The paper was finished more or less already in 1979, but then to publish it was not so easy. Not so long ago, Bob Karasek sent me its rejection letter from *The Lancet*. The formulation was the following from the Chief Editor: "How could you possibly think that we could publish this?"

TT: Since then, *The Lancet* has published a lot on the demand–control model.

AP: So it was a way of thinking at the time?

TT: Yes, definitely, but we also thought a lot about relating our model to things like anabolism, metabolism, and recovery. Bob Karasek had picked up some studies by Dement about REM sleep, which was assumed, at the time, to be an important condition for brain recovery. We talked about these sorts of things.

AP: And you mean that you understood that the disturbance of recovery might be behind this negative effect of stress?

TT: Yes, we were in that field, and we talked about it in the Russel–Karasek–Theorell paper, but that, of course, was in the *Journal of Human Stress*, which was not in Medline, so nobody knew about it.

AP: There is one thing that I wonder about. I have noticed that, since I have been near you for quite some time, sometimes you have this tremendous pleasure when you discover something in data, then you are sort of high. This is like life at its best moments. Do you remember such a moment with this data set, where you suddenly saw the relationship between the high demand and low control?

TT: Oh, yes, definitely. For instance, the American data set. They had two American data sets, which were on representative samples of working Americans. One was a health and nutrition examination survey, the HANES. That was the one we worked mainly on, and it turned out that it had great importance with respect to how we combined the two dimensions demand and control. One way was to simply subtract control from demand. Then we did not see such clear findings, but it was when we made the explicit hypothesis that you should have both high demand and low control that we started seeing things. So there were ups and downs. But the other moment that I remember very vividly, which also Bob Karasek still talks about a lot, was when we found out that we could make maps of occupations, by level of demand and control. We had averages for around 190 occupations, and then we could put them on a scale, a demand scale, and, at the same time, on a decision latitude scale. So we ended up having a map of occupations. We were so enthusiastic about it that we really felt elated, and, when Bob and I went to Broadway in the evening to see a play, we could almost not see the play because we just thought about our maps.

AP: But this is your second most-cited paper, this is from 1989. This is a paper with job characteristics in relation to the prevalence of myocardial infarction in the HANES. So you were moving this stuff one level higher—job titles rather than individuals.

TT: Well, that is very logical and, of course, this was, you know, based upon the fact that the Americans did not collect data very much on the work environment in these sorts of cardiological or epidemiologic examinations. So we had to be satisfied with the second best thing, which was to relate job title to other surveys in the American population with data on demand and decision latitude in different occupations. That information could be used in an indirect way, which has been labeled the inference method, all people working in an occupation being assumed to have basically the same conditions if they are of the same age and have worked approximately for the same number of years and if they are of the same gender. Under these conditions, all carpenters are assumed to have the same decision latitude, for instance.

AP: It is fantastic that it works. Such a robust sort of finding that you do not have a bunch of people to show it, you can actually have a group of job characteristics to show this relationship.

TT: That is true, but it is interesting that the basis of the relationship has changed a little bit now because the model has been working better in its predictions for blue-collar workers than for white-collar workers in the past. The control dimension has been more important than demands in the past, but what has happened, of course, is that the number of blue-collar workers is

shrinking in the working population. One part of the decision latitude formulation, skill discretion, has improved considerably, so that almost everyone in modern worklife in Sweden find that they learn new things at work. Therefore, the role of skill discretion has changed, and, in some cases, increasing skill discretion could even approach psychological demands. Furthermore, many industrial jobs in the blue-collar group have disappeared, whereas other kinds of jobs have become more common, such as those in information technology and knowledge production. The role of decision latitude has changed in the sense that one probably has to measure it differently. At the same time, demand levels were rising throughout the whole of the 1990s in all of Europe, reaching a peak in 2003 in Swedish data. In the latest prospective cohort studies of the relationship between job strain (high demands and low decision latitude) and the risk of developing myocardial infarction, the JACE study, which is a cohort study involving several European countries, as well as a Danish study, demands have gained in importance at the expense of decision latitude [See Kivimäki et al's paper in this supplement.]

AP: And that brings up something that fascinates me in this research. The question "What is biology and what is culture?" So, when I read your papers, I have a feeling that you are actually characterizing a basic sort of relationship, the demands and their modifiers, which are culture dependent. And you and Bob actually came up with probably one of the most important modifiers, which is the control issue.

AP: How we can predict the future, how we can control the future, how we can prepare ourselves for the future will modify the biological dimensions. Then come the other guys, your colleagues, who said that not only will control modify our reactions, but also the social context will be important. Your third-most cited paper was written by you and Jeff Johnson, when you delved into the social context issue and made a model with two modifiers—control and social support. Then you have other colleagues, like Johannes Siegrist, who talks about the balance between rewards and effort. Or, if you want to go back to Antonovsky, you might talk about the meaning of life and social context. So, actually, we are getting into the model in which, on one side, we have life demands and on the other side are resources. So maybe we should start to modify the stress concept. Stress is not the demand in itself, it is a relationship between demand and the resources people have at their disposal, and you have shown, throughout the papers and research you have done, how strongly the modifiers affect our bodily reactions to demands put on us.

TT: Jeff Johnson and his wife, Hellen Hall, spent time in Stockholm and clarified the role of social support. In one of Jeff's writings, he says something very important,

which is when he talks about relationships between social support and decision latitude. He emphasizes the fact that, if a group has a strong collective cohesion, which gives good social support, then it has a much stronger possibility to take control over things—collective control.

AP: So the social support factor works through control; therefore it is not an independent factor?

TT: I did not say that. However, they are intertwined; they operate both together and collectively. Cohesiveness is perhaps one of the most important concepts in all of this. I participated last week in a conference in Linköping, where Clyde Hertzman talked a lot about his studies in Canada. He was saying that, for Aborigines in British Columbia, good cohesiveness and no animosity with whites is very important for their health.

AP: Very interesting!

TT: So cohesiveness seems to be something that is extremely basic for healthy biology.

AP: That gives us a tremendous defense against life challenges. I have two more questions that I would be interested to hear a comment on. The first is about the positive aspect of this model. It predicts that people with high demand and high control or ones with high control and low demand or high demand will be better off. Did it work?

TT: Well, that is interesting. There are, of course, studies that show that the so-called active quadrant of the model has a problem in modern societies, and you could say that maybe the model is sometimes wrong. That is one way of looking at it, but the model still seems to predict well when we analyze outcomes like long-term sick leave. Yes, people with high demand tend to take more sick leave in general than other people, but, when you confine the analysis to long-term sick leave, it is the strain quadrant that has the problem because the active quadrant is populated by people who can perhaps use short-term sick leave to recover and do things that help them avoid long-term consequences. This does not seem to be the case with the strain quadrant. But let us assume that, indeed, the active quadrant does have a problem. There could be two reasons, either that the model does not work today or that, in modern societies, demands rise and rise.

AP: There will be no room to defend oneself?

TT: Imagine a situation in which you have beautiful structures for exerting control, but you cannot use them. I mean, in Sweden, we did this historical mistake of translating the idea of improving participation and decision latitude for employees by removing middle management. That is an idiotic thing to do in many places, because the upper levels of management get frustrated and get too much to do, while the remaining workers get

frustrated because there is no one to ask when there are problems. So that could be part of the problem. It could be that the model still works even in these situations, but we do not measure the control dimension adequately.

AP: In the times we live in now, maybe there are other dimensions that are important to protect us from demands that are too high—rewards, the meaning of our activities, or just pure lifestyle factors such as enough sleep or rest.

TT: Yes.

AP: I have one more question. If you think about it, the demand–control model became very popular, it became very widely cited. But, with the profound changes in our societies, can you see any effect on your way of thinking? Did you want to change society when you got going with this kind of research?

TT: It is always very difficult to see these things when you live in them, and things never happen the way you expect them to. Particularly the time dimension may be completely different than the one you would expect. It may take much longer for an idea to be translated into practice. I think, in general, we do have a lot of impact on society.

TT: In general, I tend to think that, in Sweden, we have had this intensive debate about stress, mainly in the late 1990s and 2000. It was triggered by the fact that we had this very condensed, very restless period with all the changes occurring at once. People became interested in stress, and that, I think, has been good for Sweden. Perhaps that could even explain some of the leveling off of work-related mental health problems since 2003.

TT: One other thing. We talk about the good sides of things in the model, and one thing that we have seen is that the social dimension at work is also related to participation in cultural activities. In our new SLOSH database, we saw that people who have low decision latitude at work, also have very little cultural activity related to work.

AP: So cultural participation can be one more resource that defends us against stress. And is this a question that you have always been interested in?

TT: Yes, yes. We have just finished a couple of studies, one was work-based, and the other was based on a psychosomatic group of patients with irritable bowel syndrome. Both show that regular participation in cultural activities does have positive physiological effects.

AP: You were always interested in psychosomatics. You also even started a small research clinic at one time, where you had a group of four therapists working with severe cases of psychosomatic diseases, and various cultural activities were part of the treatment. These were quite controversial and very exciting projects that cost you a lot of time and energy.

TT: Very little money, and we still did it.

AP: It seems that this is where you are going now. Right? You would like somehow to put more emphasis on the issue of culture and its role in health. And is this in defense of culture or in defense of health?

TT: This is a good question. In defense of health, I would say, primarily.

AP: Culture does not have to be defended.

TT: No—and it is also what people point out. You should never get a society, which requires culture, to be health promotive. Then you are in a dangerous situation.

AP: One last thing. In your most-cited paper in 1981, you put forth an idea about why class differences produce differences in health. The reasoning was that the tension in the body that can be caused by stress, or the state of imbalance that stress introduces, actually may be behind the health difference in the different social classes, a conclusion that some of your colleagues reached 20 years later or 30 years later.

TT: Oh yes, Michael Marmot has summarized this in his beautiful book. The name of it is *The Social Status Syndrome*, and I think that his summary is better than anybody else's.

AP: Well, you wrote about it in this 1981 paper.

TT: Yes, but none of us really discovered this. This

reasoning had already existed in the early psychosomatic studies on life events and so forth—when you expose people to humiliation and they cannot do anything about it, it will boil inside and will eventually affect them in profound ways.

AP: Thank you for sharing your experiences with us.

Töres Theorell—his five most-cited papers

Karasek R, Baker D, Marxer F, Ahlbom A, Theorell T. Job decision latitude, job demands, and cardiovascular disease: a prospective study of Swedish men. *Am J Public Health*. 1981;71(7):694–705. [567 citations]

Karasek RA, Theorell T, Schwartz JE, Schnall PL, Pieper CF, Michela JL. Job characteristics in relation to the prevalence of myocardial infarction in the US Health Examination Survey (HES) and the Health and Nutrition Examination Survey (HANES). *Am J Public Health*. 1988;78(8):910–8. [271 citations]

Alfredsson L, Karasek R, Theorell T. Myocardial-infarction risk and psycho-social work-environment: an analysis of the male Swedish working force. *Soc Sci Med*. 1982;16(4):463–7. [180 citations]

Johnson JV, Hall EM, Theorell T. Combined effects of job strain and social isolation on cardiovascular disease morbidity and mortality in a random sample of the Swedish male working population. *Scand J Work Environ Health*. 1989;15(4):271–9. [169 citations]

Alfredsson L, Spetz CL, Theorell T. Type of occupation and near-future hospitalization for myocardial-infarction and some other diagnoses. *Int J Epidemiol*. 1985;14(3):378–88. [158 citations]