LIFE Newsletter Volume 2, No. 1
April 2008

Note from the Editor

Welcome to the Spring 2008 edition of the LIFE newsletter; and the first to use our new corporate design. In the past months, we have worked together with the design agency, Adler & Schmidt, on a new LIFE logo and a corporate design for LIFE. We are very happy to introduce LIFE's new face to you today!

The current LIFE Newsletter issue opens with an article by Eric Turkheimer (UVa) on quasi-causal twin studies of human behavior. In the article, Turkheimer and his colleagues strongly criticize the usual efforts of twin research to measure the heritability of traits, with the expectation of revealing the genetic nature of psychological phenomena.

Michael Eid (Free University Berlin) turns our attention to structural equation models being applied to multitrait-multimethod data analysis and the difficulty of choosing the right model for a specific application. Eid and his colleagues are currently developing a general psychometric theory to define multitrait-multimethod models for different types of measurement designs and different types of change.

John R. Nesselroade was kind enough to continue our series of interviews with LIFE faculty members. He tells us about his "back door" entry into development, meaning the mixture of inclination and historical accident that made him a developmentalist. Asked about future research directions to better understand human development, he suggests a closer integration of within- and between-person variation as a key direction in which to head.

LIFE fellow Daniel Caro Vasquez (Humboldt-Unviersity Berlin) shares his experiences at a LIFE research stay abroad. From October to December 2007, he stayed at the University of Michigan and closely collaborated with Kai S. Cortina on data from the Michigan Study of Adolescent and Adult Life Transitions (MSALT). Beyond this, he encountered fruitful exchanges with LIFE faculty members and fellows as well as visiting researchers from Canada and Belgium.

Kristin Flegal (UM) reports about her impressions of the first LIFE Academy she participated in as a LIFE fellow, the Fall Academy 2007 in Ann Arbor. She was particularly impressed by the collegial atmosphere and the fruitful exchange between researchers from different parts of the world, with their diverse research backgrounds and perspectives.

In the news section you can find information on new fellows and faculty members, conferences, awards/grants received by LIFE members, completed dissertations, and careers steps of LIFE fellows and alumni.

Imke Kruse

A Better Way to Use Twins for Developmental Research

Eric Turkheimer, Professor of Clinical Psychology,
Department of Psychology, University of Virginia, ent3c@virginia.edu.

By now most psychological researchers are accustomed to twin studies and know the basics: You measure a trait of interest in identical (MZ, for monozygotic; sharing all their genes) and fraternal (DZ, for dizygotic; sharing a random half their genes, like ordinary siblings) twins, compute the MZ and DZ twin correlations, and compare them. With a few assumptions and a little modeling, higher similarity of identical pairs is used to compute the *heritability* of the trait, along with two components of the environment, *shared* and *non-shared*. The shared environment represents the portion of the familial environment that serves to make children reared together similar, while the non-shared environment is the portion of the environment that makes them different. All three terms are commonly standardized so they range from zero to one and sum to unity. But what does heritability mean? As a statistical matter, there are some complications, but overall the math is straightforward. Heritability is the proportion of the total variance of the trait accounted for (in the usual sense of analysis of variance) by genetic similarity. It is an effect size, expressed like a familiar R^2 value.

The assumptions underlying twin studies and the calculation of heritability are a classic debate topic that I will not rehash here. For the modeling underlying twin studies to make sense, one has to assume that the environments experienced by identical twins raised in the same family are no more similar than those experienced by fraternal twins, that genes and environments are independent and additive (again, in the usual ANOVA sense) in the determination of the trait, and that parents mate randomly with respect to the trait. Like all statistical assumptions, at the end of the day none of these are true, but a century of research has shown that in a basic sense they don't matter much. It's like the assumption of homogeneity of variance in an independent group *t*-test. We violate the assumption all the time, but most of the time the consequences aren't very great, and even if they are, more advanced methods have been developed to compensate.

But back to the question: What does heritability mean? Almost everyone who has ever thought about heritability has reached a commonsense intuition about it: One way or another, heritability has to be some kind of index of how *genetic* a trait is. That intuition explains why so many thousands of heritability coefficients have been calculated over the years. Once the twin registries have been assembled, it's easy and fun, like having a genoscope you can point at one trait after another to take a reading of how genetic things are. Height? Very genetic. Intelligence? Pretty genetic. Schizophrenia? That looks pretty genetic too. Personality? Yep, that too. And over multiple studies and traits the heritabilities go up and down, providing the basis for nearly infinite Talmudic revisions of the grand theories of the heritability of things, perfect grist for the wheels of social science.

Unfortunately, that fundamental intuition is wrong. Heritability isn't an index of how genetic a trait is. A great deal of time has been wasted in the effort of measuring the heritability of traits in the false expectation that somehow the genetic nature of psychological phenomena would be revealed. There are many reasons for making this strong statement, but the most important of them harkens back to the description of heritability as an effect size. An effect size of the R^2 family is a standardized estimate of the proportion of the variance in one variable that is reduced when another variable is held constant statistically. In this case it is an estimate of how much the variance of a trait would be reduced if everyone were genetically identical. With a moment's thought you can see that the answer to the question of how much variance would be reduced if everyone was genetically identical depends crucially on how genetically different everyone was in the first place; that is, it depends on the genetic variances.

And variances of this kind are local, as opposed to universal, properties of traits. Every social scientist knows that the amount of variance in y that is accounted for by x depends on the variance of x. If someone were to ask, *"How much of the variance in graduate school performance is accounted for by GRE scores?"* everyone would know that the answer is, *"It depends."* More variance would be accounted for in unselective programs for which a wide range of abilities were admitted than in a selective program that only admitted people with very high scores. So you couldn't use the proportion of grad school performance variance accounted for by GRE scores as an index of how GRE-dependent the graduate school is, because the answer depends as much on contingent local variances as it does on the nature of the graduate program.

This reasoning is Analysis of Variance 101, known at least in principle to anyone who has ever analyzed the results of a study that did not include random assignment to groups. But somehow it is routinely forgotten in the world of twin studies. Over and over again, behavior geneticists, as well as their critics, have started out with an admonition that heritabilities do not apply to traits outside the context of a particular sample, and then two paragraphs later reverted to speculation about whether the heritability of intelligence is .7 or .3 or .5. *The heritability of intelligence isn't anything!* There is no meaningful answer to that question, and responsible social scientists should refrain from speculating about it. And since heritability of intelligence is not equal to any particular value, the number produced in any particular analysis cannot possibly tell us anything about how genetic intelligence is. It was a silly question anyway.

So what now? If there is no point in estimating heritabilities, is there a point to behavior genetics generally? Fortunately, the answer is yes. Understanding the real point of behavior genetics will require backing up and starting over.

What is the effect of parental divorce on children? This classic question in developmental psychology resists easy answers. The obvious research design is to identify a sample of divorced parents with children, pair it with a sample of non-divorced parents with children, and compare the children. If there is a difference between the groups of children, there is your effect of divorce. But of course this research strategy is deeply flawed. The clearest way to state what is the matter with it is to observe that it is not possible to randomly assign families to divorce status. If it were possible, we could infer that the *only* difference between the two groups of families was their marital status, and infer that any differences between the children were uniquely attributable to that difference. That is why random assignment is the bedrock of experimental science.

In the absence of random assignment we must somehow account for the fact that divorced and non-divorced families differ in countless ways other than their marital status. Poverty is a risk factor for divorce, as are alcoholism and depression. Personality and education and ethnicity are all correlated with marital status. Any of these confounds could explain why children in divorced families are different than children in non-divorced families. The standard social scientific methodology to deal with this is statistical. We measure as many of the potential confounds as we can and include them in our prediction models, in the hope that the effect of interest will hold up when all of the potential confounds are "controlled." But it is impossible to list, let alone measure, all of the potential confounds, and the theory underlying this kind of statistical control is notoriously shaky. That is why, in our view, the social science of uncontrollable phenomena like marital disruption has made as little progress as it has: It is impossible to achieve experimental control, and the statistical fixes don't work very well.

Suppose you have a large sample of twin parents (n.b., not twin children, but grown twins with children of their own). Among these twins you will be able to find the occasional pair for which one member has divorced and the other has not. Suppose the children of the divorced twin are getting into trouble in school. Is that the effect of the divorce? Now at least you have an interesting control group: What are the children of the non-divorced twin doing? If they are acting out just as much as the children of the divorced twin, it doesn't seem likely that divorce per se is the decisive causal factor; on the other hand, if they are doing better, then perhaps there is reason to expect that the marital status of the divorced family is causing the problems in the children.

Our lab has spent the last five years unpacking the details of this kind of twin design and applying it to a variety of developmental problems. The design works because identical twins "control for" confounding background variables by literally, as opposed to statistically, holding them constant. Two crucial classes of potential confounds are controlled. The first is what we call a genetic confound. That is, if a genetic propensity to be aggressive makes parents more likely to get divorced, and those same genes when passed to the children make them more likely to be aggressive on the playground, then one will observe an association between divorce and playground aggressiveness that will not really be a causal consequence of divorce. If the parents had experienced a last-minute reconciliation and saved the marriage, the kids would have the same genes and be acting out in the same way. But in identical twin parents who share all their genes, none of the differences between the children can arise from differences in the genes of their twin parent, so if the children do differ, we can (almost, see following) rule out a genetic explanation of the association.

The second class of confounds we can rule out are shared environmental. The same logic obtains. Suppose poor families are more likely to be divorced than well-off families, and children raised in poor families are more likely to be delinquent. As before, we will observe an association between divorce and delinquency that doesn't have any causal relationship to divorce. But twin parents share their family history of poverty, so if the children of the divorced twin are more likely to be delinquent than the children of the non-divorced twin, the parental poverty isn't a plausible alternative explanation, so our confidence in the hypothesis that divorce per se is causing the delinquency is increased.

Hopefully the reader has already thought of some objections. Paradoxically, one of the satisfactions of exploring this kind of twin design has been coming to understand its limitations as well. On the bottom line, no amount of twin analysis reproduces the experimental clarity of random assignment to groups. This is not an obstacle that can be completely overcome. Two limitations in particular are important to consider. The first is the possibility of non-shared environmental confounds. Perhaps it is the case that alcoholism contributes to the likelihood of divorce, and in addition alcoholism promotes bad parenting practices that contribute to negative outcomes in children. Once again, we would observe an association between divorce and child outcomes, and once again it would not really be the effects of the divorce itself: It's the drinking. But in this case, the identical twins wouldn't help. If the divorced twin was a drinker and the other not, then the drinking would induce a difference in their children, and the twin design wouldn't be able to hold it constant.

The other major shortcoming is really a special case of the first, but as it is the most glaring it is worth mentioning separately. Unfortunately for us hardworking behavior geneticists, twin parents can't have kids all by themselves, and twins don't often marry other twins. Spouses are a weak link in the twin-parent design, because they introduce both genes and environment that cannot be controlled by identical and fraternal twins. Usually, the best you can do is to revert to the old methodology, measure as many variables as you can in the spouses, include them in the model and control for them statistically.

Being a non-experimental social scientist is an exercise in humility. The best scientific tool available—in some sense, the only real scientific tool—is not available to us because we do not have experimental control over our phenomena. In our lab, we prefer to take that limitation seriously, and make a habit of never saying that we have established that one phenomenon causes another. Instead, we prefer to say that we have established *quasi-causation*, by which we mean that we have observed an association between two developmental variables and exposed them to the toughest *quasi-experimental* tests that the twin design will allow.

Once you start thinking this way, you realize that most of traditional human developmental psychology is waiting to be redone using the twin design. We (and by we I am referring to myself, my colleague Robert Emery, and our students) have, of course, undertaken extensive studies of marital status, under the direction of Brian D'Onofrio, now at the University of Indiana. Paige Harden (Harden et al., 2007) has studied the effects of marital discord, as opposed to dissolution. Stacey Lynch (Lynch et al., 2006) has looked at the consequences of parental punishment practices. Jane Mendle (Mendle et al., 2006) has examined the consequences of step-parenting, and Jennifer Hill (Hill, Mendle, Harden, Turkheimer, & Emery, in press) has studied adolescent peer groups.

Throughout all of these studies, we have found one particularly encouraging contrast with the old heritability-based twin studies of years past. Those twin studies had a discouraging habit of all coming out the same way. Again and again, twin studies would show that one trait or another was fairly heritable (a heritability of .5 is always a good guess) with not much variance attributable to shared environment, and the substantial remaining portion attributable to the non-shared environment. In contrast, our quasi-causal studies have revealed a rich variability of outcomes, with some purported effects explained away by genetic or environmental confounds, while others hold up to the rigors of the quasi-causal testing. The results of the studies are neither simple nor conclusive, but they are a more adequate reflection of the environmental and genetic developmental processes that underlie complex human behavior.

References

Harden, K. P., Turkheimer, E., Emery, R. E., D'Onofrio, B. M., Slutske, W. S., Heath, A. C., & Martin, N. G. (2007). Marital conflict and conduct problems in children of twins. *Child Development, 78,* 1-18.

Hill, J. E., Mendle, J. E., Harden, K. P., Turkheimer, E., & Emery, R. E. (in press). The relationship between adolescent externalizing and affiliation with externalizing peers: A behavior genetic approach. *Journal of Abnormal Child Psychology.*

Lynch, S. K., Turkheimer, E., D'Onofrio, B. M., Mendle, J., Emery, R. E, Slutske, W. S., & Martin, N. G. (2006). A genetically informed study of the association between harsh punishment and offspring behavior problems. *Family Psychology, 20,* 190-198.

Mendle, J., Turkheimer, E., D'Onofrio, B. M., Lynch, S. K., Emery, R. E., Slutske, W. S., & Martin, N. G. (2006). Family structure and age at menarche: A children-of-twins approach. *Developmental Psychology, 42,* 533-542.

Structural Equation Modeling of Multitrait-Multimethod Data: New Developments

Michael Eid, Professor Methods and Evaluation, Department of Educational Science & Psychology, Free University Berlin, eid@zedat.fu-berlin.de

Multitrait-multimethod (MTMM) analysis is the most important methodological development for the analysis of the convergent and discriminant validity of psychological measures. Campbell and Fiske's (1959) groundbreaking article on "Convergent and Discriminant Validation by the Multitrait-Multimethod Matrix" is the most-cited article ever published in *Psychological Bulletin.* This high impact suggests that multimethod research programs are generally preferred to single method approaches in almost all areas of psychological research (Eid & Diener, 2006). The great importance of multimethod research programs led to the development of many methodological approaches for analyzing MTMM data (for an overview, see Dumenci, 2000; Eid & Diener, 2006). Over the years, structural equation models (SEM) have become the most widely applied statistical models for MTMM data analysis. SEM models allow for a separation of trait, method, and error components, as well as for empirical tests of the underlying model assumptions. Moreover, trait and method factors can be linked to other latent constructs, allowing for the analysis of construct validity and criterion validity in one single model. Analyzing MTMM data with structural equation models has had a long tradition in psychology and has resulted in many different models. Widaman (1985), for example, developed a taxonomy of 16 SEM for MTMM data by crossing four different types of trait structures (no trait factor, general trait factor, orthogonal trait factors, oblique trait factors) with four different types of method structures (no method factor, general method factor, orthogonal method factors, oblique method factors). In addition to these models, several other SEM-based approaches have been developed. I would guess that in the meantime there are up to 30 different SEM for multimethod data.

Although there has been extensive work on SEM models for MTMM data over the last years, most applied researchers are still uncertain as to which model should be chosen for a specific application. In my practical work as a methodological consultant, I often experience that researchers apply all available MTMM models in order to find the model that fits the data best. But, as more models are tested, the likelihood that improper solutions, convergence problems, and poor model fits will emerge increases. Moreover, researchers may be uncertain as to how the results obtained from SEM models for MTMM data should be properly interpreted. Hence, this data-driven strategy can lead to frustration and the abandonment of SEM approaches to MTMM data altogether. One major problem is that many SEM for MTMM data have not been developed based on sound psychometric theory but were designed according to some reasonable assumptions or some aesthetic features such as symmetry.

Researchers can avoid such problems if they carefully consider the underlying MTMM measurement design, and if they apply models that are appropriate for the measurement design being considered. Each measurement design has serious implications for the development of an appropriate model. These implications, however, often do not become obvious until one tries to define the models as stochastic measurement models. That means one has to define the random experiment that characterizes the measurement design, the set of possible outcomes for which the observed and latent variables have to be defined, and so on.

One major current topic in our research group is to develop a general psychometric theory for the definition of MTMM models for different types of measurement designs. One important aspect of the measurement design is the type of method being considered. Currently we examine three types of method structures: (1) interchangeable methods, (2) structurally different methods, and (3) the combination of both methods. For instance, if we consider different raters as different methods, an example of interchangeable methods (raters) is the assessment of an employee by several clients who are randomly drawn from all of the employee's clients or the ratings of an individual's behavior by friends who are randomly selected from a peer group. The situation is different however if one would obtain, for example, self-evaluations, client reports, and supervisor ratings of employees. These three raters are structurally different because they are not randomly chosen from a common set of equivalent raters, and have different access to the target's behavior. An example of the combination of interchangeable and structurally different methods is the assessment of an employee by several clients (interchangeable raters) and by her- or himself. The clients and the target are structurally different methods.

Considering these differences between methods has important consequences for the definition of an MTMM model. In the classical general MTMM model, the so-called Correlated-Trait-Correlated-Method Model (Marsh, 1989), an observed variable (measuring a trait with a specific method) is decomposed into a trait factor, a method factor, and an error variable. Moreover, the trait factors can be correlated, and the method factors can be correlated. Our psychometric work, however, shows that this general model is not reasonable in most cases – in addition to the many other problems this model shows (Eid, Lischetzke, & Nussbeck, 2006; Eid, Nussbeck, Geiser, Cole, Gollwitzer, & Lischetzke, 2008). According to our approach, a model with as many trait factors as traits and as many method factors as methods is only reasonable in the case of interchangeable methods (e.g., raters) but only if the method factors are uncorrelated. For structurally different methods, a model with as many method factors as methods considered is not reasonable. Instead, a model with one method factor less than methods is a reasonable and interpretable model (Eid, 2000; Eid, Lischetzke, Nussbeck, & Trierweiler, 2003; Geiser, Eid, & Nussbeck, 2008).

Currently, we are extending this approach to longitudinal modeling and are developing MTMM models for different types of change (Courvoisier, Nussbeck, Eid, Geiser, & Cole, 2007; Geiser, Eid, Nussbeck, Courvosier, & Cole, 2008a,b). As different types of methods require different types of models, different types of change (e.g., variability vs. developmental changes) require different types of models as well. As a consequence, a system of longitudinal MTMM models that are appropriate for different types of methods and different types of change would fill an important gap in the world of latent variable modeling. This system allows choosing the most appropriate model for analyzing the convergent and discriminate validity of change.

References

Campbell, D. T., & Fiske, D. W. (1959). Convergent and discriminant validation by the multitrait-multimethod matrix. *Psychological Bulletin,* 56, 81-105.

Courvoisier, D., Nussbeck, F., Eid, M., Geiser, C., & Cole, D. (2008). Analyzing the convergent and discriminant validity of states and traits: Development and applications of multimethod latent state-trait models. Manuscript submitted for publication

Dumenci, L. (2000). Multitrait-multimethod analysis. In S. D. Brown & H. E. A. Tinsley (Eds.), *Handbook of applied multivariate statistics and mathematical modeling* (pp.583 611). San Diego, CA, US: Academic Press.

Eid, M. (2000). A multitrait-multimethod model with minimal assumptions. *Psychometrika,* 65, 241-261.

Eid, M., & Diener, E. (2006). *Handbook of multimethod measurement in psychology*. Washington, DC: American Psychological Association.

Eid, M., Lischetzke, T., & Nussbeck, F. W. (2006). Structural equation models for multitrait-multimethod data. In M. Eid & E. Diener (Eds.), *Handbook of multimethod measurement in psychology* (pp.283-299). Washington, DC: American Psychological Association.

Eid, M., Lischetzke, T., Nussbeck, F. W., & Trierweiler, L. I. (2003). Separating trait effects from trait-specific method effects in multitrait-multimethod models: A multiple-indicator CT-C(M-1) model. *Psychological Methods*, 8, 38-60.

Eid, M., Nussbeck, F. W., Geiser, C., Cole, D. A., Gollwitzer, M., & Lischetzke, T. (2008). Structural equation modeling of multitrait-multimethod data: Different models for different types of methods. Manuscript submitted for publication.

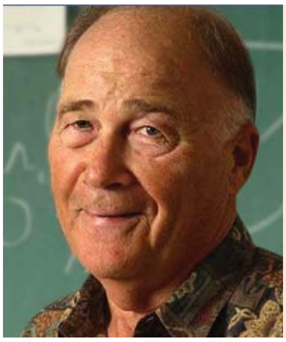
Geiser, C., Eid, M., & Nussbeck, F. W. (2008). On the meaning of the latent variables in the CT-C(M–1) model: A comment on Maydeu-Olivares & Coffman (2006). *Psychological Methods,* 13, 49-57.

Geiser, C., Eid, M., Nussbeck, F. W., Courvoisier, D. S., & Cole, D. A. (2008a). Analyzing latent change in longitudinal multitrait-multimethod studies: New models and application to depression and anxiety assessment in children. Manuscript in preparation.

Geiser, C., Eid, M., Nussbeck, F. W., Courvoisier, D. S., & Cole D. A. (2008b). Analyzing the convergent and discriminant validity in longitudinal studies: Confirmatory factor analysis of multitrait-multimethod-multioccasion data. Manuscript submitted for publication.

Marsh, H. W. (1989). Confirmatory factor analysis of multitrait-multimethod data: Many problems and a few solutions. *Applied Psychological Measurment,* 13, 335-361.

Widaman, K.-F. (1985). Hierarchically nested covariance structure models for multitrait-multimethod data. *Applied Psychological Measurement,* 9, 1-26.



10 Questions.

John R. Nesselroade, Professor of Qunatiative Psychology,
Department of Psychology, University of Virginia, jrn8z@virginia.edu

1. How did you get involved in the study of development?

Both by inclination and by historical accident. All of my PhD work was with Raymond B. Cattell at the University of Illinois so, technically, personality was my major area. Even as a graduate student, however, I was very interested in the measurement of change, especially the methodological issues associated with it. When I took my first post-PhD position--Assistant Professor, West Virginia University--I was part of the personality-social program in the Psychology Department. When the senior personality professor left to take another job, K. Warner Schaie, the Department Head, decided to scuttle the personality-social program and gave me the choice of joining the clinical, the experimental, or the developmental program. By that time, I was well into my long collaboration with Paul Baltes so the choice was easy--I became a developmentalist.

2. Could you name a few books or articles that have profoundly influenced your own thinking about development?

Because of my "back door" approach to development, I wasn't directly influenced by what others would regard to be the "classics" of the developmental literature. As I look back on my 40 years of being a "developmentalist" I would have to say that the sources I think have influenced me most include the life-span series out of the West Virginia conferences (especially the first three), *Problems in Measuring Change* edited by Chester W. Harris, the first edition of *The Handbook of Multivariate Experimental Psychology* edited by Raymond B. Cattell, and the late Jack Wohlwill's book--*The Study of Behavioral Development*. Subsequently, I have also been influenced by the writings of many other developmentalists.

3. What are, according to you, two main issues that the field is or should be wrestling with?

Well, there are many more than two, I believe but to keep this manageable, I will mention only two of my favorites. First, how to move the field from its current reliance on essentially static, equilibrium-oriented concepts and models to more dynamic conceptions is one of the greatest challenges facing developmentalists, I believe. Second is how to keep the field moving in that direction in the face of all the inertia that dominates it.

4. What research topics have so far been neglected or have not received enough attention?

I am in favor of much more avidly pursuing systems-theoretical approaches. In many cases, this means refining or even drastically altering the kinds of questions we ask and try to answer. We have been leaning on the same old "tired" paradigms for a long time, both substantively and methodologically, and rather than adding on still another bell or whistle to what we already have, I'm for actively pursuing novel approaches to measurement, design, modeling and analysis and, most important of all, the questions being asked and the theories from which they are derived.

5. You have, over the years, often pushed new methods for studying development. Can you tell us about some of your current innovations?

For the last few years, I have been working at a problem of measurement: How to represent the same construct with different indicators from one individual to another. Tied up with this is the idiographic/nomothetic debate and the relationship of intra-individual to inter-individual variability.

**6. You have been involved in many longitudinal studies, both intensive studies of a few individuals and large scale studies

of many individuals. What types of studies do you see on the horizon, or as most fruitful for further understanding of development?

For my money, better understanding and closer integration of within- and between-person-variation is a key direction in which to head. Molenaar, for example, who so easily moves across disciplines, has brought the concept of ergodicity into discussions of developmental phenomena. In so doing, he has given us an important way to conceptualize many of the commonalities and disparities. Much more needs to be done in this regard, however.

7. What are some of your current and future research plans?

My professional career is now more than 40 years old and starting to wind down. Before it ends I would like very much to follow up some of the work I have been doing in the last few years involving alternative definitions of invariance and approaches to multivariate measurement. I am also deeply interested in efforts to emphasize the individual as the unit of analysis while still doing rigorous, quantitative research.

8. What do you like most in being a professor of developmental psychology?

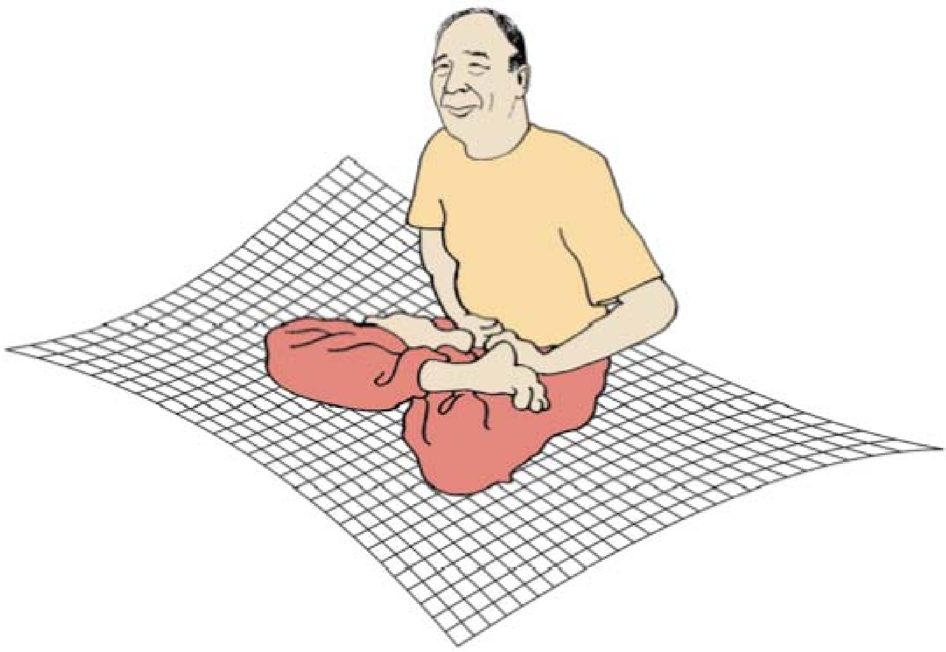
Two aspects come to mind. One is legitimately working on problems associated with the study of change with which, as I mentioned earlier, I have had a continuing love affair since I was a graduate student. The second is working with bright young PhD students and Post-docs, of whom I have had a generous share of the exceptional ones for 40 years. My former students are now in several different countries and doing very well. It makes me very proud to have had a part in shaping their careers.

9. What do you get out of LIFE?

Participating in LIFE has been reinforcing in several respects. One is the satisfaction of being involved in a program that I believe contributes greatly to the quality and quantity of developmental science production. It is also rewarding to affiliate with many of the "best and brightest" currently working in developmental science.

10. What is your favorite part of LIFE?

Referring to my answer to the previous question, I think it is the frequent affiliation with junior and senior developmental science colleagues occasioned by the academies. They are productive, enlightening, and fun!



A LIFE Fellow's Exchange from Berlin to Ann Arbor

Daniel Caro Vasquez, LIFE Fellow since 2006
Humboldt-University Berlin, cavasqud@student.hu-berlin.de

International collaborations and exchanges are a core component of the LIFE program. As Dr. Lindenberger has suggested, there are no geographical boundaries for research and thus he encourages fellows to participate in LIFE's Research Stay Abroad program. This particular view of research plus Jutta Matta's exchange experience at the University of Michigan (see LIFE Newsletter No. 1) inspired me to seek a research visit at a partner university in the US. Now back in Berlin after two months at the University of Michigan (UM), I am very glad to have benefited from such an enriching educational opportunity.

During my stay at the UM, I collaborated with Dr. Cortina in the analysis of the Michigan Study of Adolescent and Adult Life Transitions (MSALT). I had contacted Dr. Cortina on this collaboration, among the faculty members in the US, because he had moderated my talk in the LIFE Spring Academy 2007 and seemed to share my research interests. His reaction was very positive. Aside from being an extremely kind person, Dr. Cortina's supervision was a key to my research; I could not have made a better decision.

For those considering a research stay abroad with LIFE, the period after the LIFE Academy is perhaps most suited. Also, from my experience, you should organize your visit, that is, prepare the required documentation (e.g., letter of invitation, J-1 visa) and suggest potential collaborations, at least three months ahead of time. As to the length of the visit, two months seemed a reasonable period for a secondary data analysis project such as mine. Indeed, in this period I prepared the data, obtained preliminary results, and got feedback.

Needless to say, this would have not been possible without the logistical organization of my hosting institution, the Institute for Social Research (ISR) at UM, in providing me with the data and infrastructure. The ISR has a relatively high flow of visiting researchers and certainly a solid network facilitating their work. During my stay, I had a chance to meet three of their visiting researchers from Canada and Belgium. In conjunction with some LIFE fellows and Dr. Eccles, they kindly offered valuable feedback to a presentation I gave on the MSALT analyses during one of their LIFE meetings.

I also had the opportunity to discuss possible collaborations with Isabelle Archambault, a visiting researcher from the University of Montreal. We share an interest in the Canadian context, which in my case emerged during my Master's degree in Canada. Our conversations evolved into a collaborative project on a Québécois study of schools in disadvantaged areas. We plan to work on it mostly as a long-distance project but will also meet during her visit to Berlin for the International Congress of Psychology in July 2008.

During my time in North America, I also had a chance to present at the Conference of Life Course Transitions of Children and Youth in Halifax, Canada, an event hosted by the University of Dalhousie and the Atlantic Research Data Centre. This was a very rewarding experience. I presented a Canadian longitudinal study on the SES achievement gap from childhood to adolescence to an audience of policy makers and researchers from Statistics Canada and other institutions who kindly provided me with feedback and raised interesting questions. After my talk and during dinner, I had the opportunity to discuss our research interests with some of them separately. These conversations were particularly inspiring as most of them had a research focus on the life course.

Fortunately, I also had the opportunity to enjoy the city of Ann Arbor and do some traveling. Ann Arbor has a lot to offer. For instance, its cultural life, international restaurants, and bookshops exceed what one would normally expect for a university city with a population of around 120,000 people. Curiously, its population barely exceeds the capacity of the football stadium (110,000), which is the largest in the US. Yes, football is very big in Ann Arbor and you can only imagine how the streets look during a game.

For those who like outdoor activities, Ann Arbor has beautiful views. I very much enjoyed running next to the river and in the woods. There are several running trails hosting runners. Certainly the views of the foliage during the fall and the fresh and pure air the forest exhales throughout the city are something to be thankful for. Luckily, the city is small enough not to miss its amazing landscapes. And, if you are serious about running, you may also want to join one of their running teams.

Overall, visiting the UM was a very enriching experience. I have organized a project with the MSALT data and already obtained critical feedback on preliminary analyses. I engaged in a collaborative project with Isabelle Archambault and presented some of my research in Canada. I even had the privilege of attending a talk by Dr. James R. Flynn on the racial IQ gap. I also enjoyed the city of Ann Arbor, with its progressive lifestyle and beautiful landscapes. As I was told prior to my trip there, it is indeed a cute little city.

Reflections on My First LIFE Academy

Kristin Flegal, LIFE Fellow since 2007
University of Michigan, kflegal@umich.edu

Having joined the LIFE program at the University of Michigan earlier in the year, the first academy I attended was the one hosted by my home institution last October. Excited for my first opportunity to interact with LIFE fellows and faculty from other sites, and a bit nervous to be presenting a poster on preliminary findings from my own research for the first time, I wasn't sure what to expect. What I found was a community of collegial and dedicated scientists, offering a rare opportunity to integrate perspectives from other parts of the world as well as other parts of behavioral science into my graduate education.

Meeting LIFE fellows from other sites and learning about their research was the overall highlight of my first academy. At each meal break I encountered a new mix of friendly tablemates, it wasn't long before I had been introduced to most of the visiting students, all of whom warmly welcomed me to the program and were curious to hear about what I was studying. In these informal exchanges, as well as during the daily research presentations and discussions, I was struck by the genuine interest that fellows showed in one another's research, and also by how generously the faculty shared their insights and suggestions.

The interdisciplinary focus of the LIFE program was one of the reasons I wanted to apply in the first place, but I gained a better appreciation of the real sense of community within the program once I saw firsthand how students and faculty from different institutions were coming together and exchanging ideas.

My first LIFE academy acquainted me with knowledgeable and accomplished researchers whose interests matched my own, but also invited me to learn about topics under the overarching theme of lifespan development that I would very rarely be exposed to at ordinary conferences in my field. Discussions of educational, social, and genetic influences on the life course, to name just a few, meaningfully inform the large research questions that I want to ask, and they add a unique perspective to my graduate education that I am grateful to have available as a fellow in the LIFE program. One part of the academy that I especially enjoyed was simply finding out more about the research that the other Michigan fellows are currently conducting, outside of my area of cognitive psychology. While all along I had been looking forward to meeting the students from other sites who were previously known to me only by their LIFE website profiles, an unanticipated bonus of the experience I had in October was getting to better know the students from my own institution.

Something else that changed was my outlook on the large variety of research aims coexisting in the LIFE program. Recently at Michigan there have been few enough fellows spread widely enough across disciplines that at times there is little overlap in what we study, so that maintaining a feeling of cohesiveness and shared knowledge is not always easy. At the academy, determining the right balance between common research interests and breadth of discipline coverage was a recurring topic in casual conversation (and was directly addressed during the fellows' last-day lunch session), and yet any prior concerns that I had were subdued by the commonalities I saw emerging as individuals who might feel isolated to some extent at their home institutions were coming together. My own research interests, for example, include cognitive aging and functional neuroimaging, interests I have in common with only a few other Michigan fellows, but I was thrilled to meet fellows from other sites for the first time who are actively investigating similar issues. The message I took away is that if you are studying any facet of lifespan development, then somewhere in the LIFE program, even if not at your institution, someone else is working on a related research question. Far from giving rise to segregated factions of experimenters, instead it seems to me that this situation naturally presents opportunities for across-site collaborations that are such a benefit of the program.

Lastly, as a fellow at the institution that was hosting the Fall Academy, I had an inside view of the considerable behind-the-scenes efforts that the local students (not to mention the tireless local program coordinator, Deanna!) invested to organize activities for their guests. I was impressed by the creativity and commitment of the Michigan fellows to ensure that visitors from Charlottesville and Berlin would have the chance to see more of the Ann Arbor area than the inside of the conference hotel! From what the Michigan fellows who are LIFE veterans had to say, they clearly felt it was the least they could do after the tremendous hospitality they had been shown at past academies. Witnessing this planning process, and later the camaraderie among fellows who had first met one or more academies ago, really demonstrated for me the sense of community and relationship-building that seems to be a core strength of the LIFE program. I value this kind of cooperative spirit, particularly in academic settings, and an all-around positive experience at my first academy has left me looking forward to further involvement in those still to come.

LIFE News

- Three new LIFE fellows in Michigan: Julie Maslowsky, Leah Kokinakis, and Jerel P. Calzo. Julie is a graduate student in the Developmental Psychology area and primarily works with John Schulenberg, Christopher Monk, and Daniel Keating. Leah is a graduate student in the Personality and Social Contexts area of Psychology and primarily works with Robert Sellers and Jacquelynne Eccles. Jerel is a graduate student in the Developmental Psychology area and participates in the Development, Psychopathology, and Mental Health program. His advisors are Dr. L. Monique Ward and Dr. John Schulenberg.

- Lars Penke, LIFE alumnus from the Humboldt-University Berlin, took up a postdoc position at the University of Edinburgh in February 2008. He works with Ian Deary in the "Disconnected Mind" project, which aims to determine the risk factors in healthy mental aging, and to define the changes taking place in the brain during mental decline.

- Berlin LIFE alumni Tobias Bothe-Hutschenreuter (HU), Bettina von Helversen (MPIB), Dana Kotter-Grühn (MPIB), Jutta Mata (MPIB), Yee Lee Shing (MPIB), and Yi-Miau Tsai (MPIB) most successfully defended their doctoral theses at one of the Berlin Universities.

- Robin S. Edelstein, Assistant Professor at the University of Michigan's Department of Psychology, recently joined the LIFE Faculty. Her research interests relate to the areas of attachment and close relationships, emotional memory and cognition, and psychology and law.

- Three new LIFE fellows in Berlin: Gizem Hülür, Maja Wiest, and Ralf Wölfer. Gizem is a graduate student at the Humboldt-University working with Oliver Wilhelm; Maja Wiest is working with Clemens Tesch-Römer at the German Centre of Gerontology; and Ralf Wölfer is part of the Applied Developmental Psychology Unit at the Free University headed by Herbert Scheithauer.

- A LIFE symposium on "LIFE: A dynamic inter-play between neurobiological predisposition and environmental influences" has been accepted for the International Congress of Psychology that will take place in Berlin in July 2008. People involved are: Karen Bartling and Irene Nagel (Chairs); Paige Harden, Daniel Caro Vasquez, Tabea Reuter, Fanny Jimenez (Participants); Ulman Lindenberger, Jacqui Smith (Discussants)

- Karen Bartling (LIFE fellow, Berlin) received a DFG travel grant for the International Conference on Infant Studies 2008 in Vancouver/Canada.

- Annette Brose (LIFE fellow, Berlin) received a research cooperation grant from the German Science Foundation (DFG). She is currently staying at Pennsylvania State University, where she is closely collaborating with David Almeida.

LIFE Newsletter

Editor
Imke Kruse, Max Planck Institute for Human Development | kruse@mpib-berlin.mpg.de

Collaboration on this issue: Amy Michèle, Max Planck Institute for Human Development | michele@mpib-berlin.mpg.de

Aim of the newsletter
The LIFE newsletter encourages collaboration and interaction among people within the LIFE program. It provides an information platform where fellows, alumni, and faculty members learn more about each other's research, identify colleagues with similar interests and possible projects for collaboration.

Contributions
Please send contributions, suggestions, and input to the editor.

Publishing information
The LIFE newsletter is published three times a year as a PDF document and send to LIFE members only.

Editorial office
Max Planck Institute for Human Development | Königin-Luise-Straße 5 | 14195 Berlin | Germany