ДОКУМЕНТЫ И ПУБЛИКАЦИИ

«Я был рад и ошеломлён находкой сокровищницы материалов по российской генетике» Письма Марка Адамса Ф.Г. Добржанскому

(Публикация, предисловие и комментарий М.Б. Конашева[1] с согласия и при содействии М.Б. Адамса)

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Предмет публикации — письма Марка Бойера Адамса Феодосию Григорьевичу Добржанскому по истории российской генетики и эволюционной биологии, а также два ответных письма Ф.Г. Добржанского. Марк Адамс — историк биологии, заслуженный профессор Университета Пенсильвании, США, член-корреспондент немецкой Академии наук (Леопольдина). В настоящее время он на пенсии. Ф.Г. Добржанский (25 января 1900 г. — 18 декабря 1975 г.) был знаменитым русско-американским генетиком, эволюционным биологом, мыслителем и эволюционным гуманистом. Он был одной из центральных фигур в области эволюционной биологии в первой половине XX века и сыграл ключевую роль в «эволюционном синтезе» и формировании «синтетической» или современной теории эволюции в 1930–1940 гг. Добржанский родился в небольшом городе Немиров на юго-западе Российской империи, выехал в лабораторию Томаса Ханта Моргана в Соединённых Штатах в декабре 1927 г. и в 1931 г. принял решение не возвращаться в СССР. Главной темой писем, написанных в 1970–1975 гг., являются встречи Адамса с Добржанским, включая несколько интервью. Основной целью этих интервью были различные страницы и аспекты развития российской генетики, главным образом, в 1920-х гг., вклад и личности самых важных российских биологов того времени, в первую очередь Ю.А. Филипченко, Н.К. Кольцова, С.С. Четверикова, Н.И. Вавилова и А.С. Серебровского, и также библиотека Добржанского и его отношения с его бывшими российскими коллегами в тот период, когда Добржанский жил и проводил свои исследования в Советском Союзе. Письмо, датированное 11 ноября 1975 г., сопровождается Приложением, включающим исследовательский проект Марка Б. Адамса под названием «Исследования по истории российской биологии 1870–1930 годов», дополнительную информацию о предполагаемом исследовании, и краткую биографическую справку — комплект всех документов, необходимых для подачи заявки на получение гранта IREX'а на проведение исследований по данной теме.

Ключевые слова: российская (советская) генетика, Марк Б. Адамс, Ф.Г. Добржанский, «синтетическая теория эволюции», эволюционная генетика, история науки.

Марк Боейр Адамс — американский историк биологии, всю свою научную жизнь являвшийся профессором в Пенсильванском университете, в Филадельфии. В настоящее время он на пенсии. Его основные научные интересы лежали в области истории науки, преимущественно истории биологии и в основном таких её составных частей, как генетика, популяционная генетика, Ч. Дарвин, дарвинизм и эволюционная теория, морфология, экспериментальная биология. При этом его главные исследования как историка науки и его главные работы посвящены истории российской и советской науки, прежде всего генетики, сравнительному анализу истории евгеники в разных странах, в первую очередь в СССР, США и Германии. Но и его интерес к таким предметам, как соотношение науки и религии, науки и литературы, науки и научной фантастики, в той или иной степени также всегда был связан с его главными исследованиями.

Естественно поэтому, что ещё в самом начале научной карьеры, когда ему представилась возможность вступить в переписку и взять интервью у известного русско-американского генетика, эволюционного биолога, мыслителя и эволюционного гуманиста Феодосия Григорьевича Добржанского, он тут же ею воспользовался[1]. Главной темой писем М.Б. Адамса к Ф.Г. Добржанскому, написанных в 1970–1975 гг., являются его встречи с Добржанским, включая несколько интервью. Основной целью этих интервью были различные страницы и аспекты развития российской генетики, главным образом, в 1920-х гг., вклад и личности самых важных с точки зрения М.Б. Адамса российских биологов того времени, в первую очередь Ю.А. Филипченко, Н.К. Кольцова, С.С. Четверикова, Н.И. Вавилова и А.С. Серебровского, и также личная библиотека Ф.Г. Добржанского и его отношения с бывшими российскими коллегами в тот период, когда Ф.Г. Добржанский жил и проводил свои исследования в Советском Союзе. Письмо, датированное 11 ноября 1975 г., сопровождается Приложением, включающим исследовательский проект М.Б. Адамса под названием «Исследования по истории российской биологии 1870–1930 годов», дополнительную информацию о предполагаемом исследовании, и краткую биографическую справку — комплект всех документов, необходимых для подачи заявки на получение грант IREX'а на проведение исследований по данной теме[2]. В подборку писем включены также два ответных письма Ф.Г. Добржанского, сохранившиеся в виде машинописных копий.

Публикация осуществляется по рукописным и машинописным подлинникам, а также копиям, хранящимся в Библиотеке Американского философского общества (American Philosophical Society Library. B: D65 Th. Dobzhansky Papers). Письма

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[1] Ф.Г. Добржанский (25 января 1900–18 декабря 1975) был одной из центральных фигур в области эволюционной биологии в первой половине XX века и сыграл ключевую роль в «эволюционном синтезе» и формировании «синтетической или современной теории эволюции» в 1930–1940 гг. Добржанский родился в небольшом городе Немиров на юго-западе Российской империи, выехал в лабораторию Томаса Ханта Моргана в Соединённых Штатах в декабре 1927 г., и в 1931 г. принял решение не возвращаться в СССР. См. подробнее: *Конашев М.Б.* Ровесник генетики, ровесник века: Ф.Г. Добржанский (1900–1975) // Деятели русской науки XIX–XX веков. Вып. IV. СПб.: Нестор-История, 2008. С. 193–228.

[2] Одним из следствий получения М.Б. Адамсом этого гранта стала его вторая поездка в СССР в 1977 г., положившая начало многолетнему сотрудничеству с коллегами, важной вехой которого стал международный симпозиум «Феодосий Добржанский и эволюционный синтез», посвящённый 90-летию Ф.Г. Добржанского и прошедший в сентябре 1990 г. в Ленинграде. См. подробнее: *Конашев М.Б., Кременцов Н.Л.* Симпозиум, который несколько лет назад был бы невозможен // Вопросы истории естествознания и техники. 1991. № 2. С. 158–160.

расположены в хронологическим порядке. Для унификации каждое письмо имеет заголовок, куда вошли число, месяц, год и место написания письма. Даты и другие элементы, отсутствующие и установленные при подготовке публикации, даются в квадратных скобках. Также в квадратных скобках даются дописанные части сокращённых слов, кроме общепринятых.

Археографическая обработка текстов проводилась в соответствии с правилами публикации документальных источников. Публикация осуществлялась по правилам современной орфографии и пунктуации; в некоторых случаях сохранены особенности авторских написаний отдельных слов и терминов.

Данная публикация стала возможной благодаря исследованию в Библиотеке Американского философского общества, поддержанному Библиотекой Американского философского общества (2015–2016 Library Resident Research Fellowship at the American Philosophical Society).

Mark B. Adams — Th. Dobzhansky

№ 1
October 21, 1970, Philadelphia

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October 21, 1970

Professor Theodosius Dobzhansky
Rockefeller University
New York, New York

Dear Professor Dobzhansky:

As a graduate student at Harvard who studied history of biology under Everett Mendelsohn, evolutionary theory with Ernst Mayr, genetics with Meselson[3], and as a personal friend of June[4] and Stephen Toulmin[5], I have of course had the pleasure of reading many of your published works. In my researches on the history of population genetics, it would indeed have been impossible to have overlooked your writings, since to do so would have been to ignore perhaps the major figure in the field. It is with a certain amount of embarrassment that I send you my two articles on

[3] Мэтью Мезельсон (Matthew Meselson, b. 1930) — американский генетик. С 1960 г. профессор Гарвардского университета, активный сторонник запрета биологического и химического оружия.

[4] Джун Гудфилд (June Goodfield, b. 1927) — английский историк науки, учёный и писатель.

[5] Стивен Эделстон Тулмин (Stephen Edelston Toulmin, 1922–2009) — английский философ науки. В 1972 г. Опубликовал работу «Человеческое понимание», в которой показал, что процесс развития науки носит эволюционный характер.

Russian population genetics 1920–1935[6], since it seems fairly clear to me that you know so much more about the subject than I do that they contain nothing you do not already know. Nonetheless I send them anyway, partly as a way of introducing myself and partly in the hope that you can set me straight on any misstatements or misinterpretations that the essays still contain.

As a brief reading of the two essays will make clear, I have barely scratched the surface, and since I feel that there is a great deal of importance to be elucidated in the experience of Russian biology, 1890–1930, I am now attempting to probe the development of Russian genetics somewhat more deeply. I have gotten ahold of the eugenics/genetics publications edited by Filipchenko from 1921 through the early thirties, and my present research strategy would be to center on Filipchenko, Koltsov, Chetverikov, and Serebrovskii, and groups of students and colleagues who studied with them. I should also like to get ahold of more precise information about training in Russian universities in evolutionary theory, biometry, genetics, variation, etc, from about 1890 through 1928. I would also like to be able to get ahold of relevant intellectual "flows" — for example, the impact of Johannsen's books on Russian biologists, the specific channels whereby the impact of the Morgan school was felt by the Russian biologists, the significance of Muller's various trips particularly in the early twenties, and so forth. I have gotten ahold of the Russian book, Klassiki Sovetskoi genetiki[7], Vavilov's Selected Works (two volumes)[8], Reznik's Popular Biography of Vavilov[9], Filipchenko's Variabilität und variation[10], and Polynin's Prorok v svoem otechestve[11], and a biography of Koltsov[12]. In addition, I have been following the publication of a number of works by and on Vavilov, Serebrovskii, et al., in Genetika, and to a certain extent in the Biuleten' Moskovskogo obshchestva ispytatelei prirody. The picture developing in my head, however, on the basis of the above sources, seems rather static and incomplete, lacking the dimension of community dynamics and interaction which someone who knew the people involved could appreciate better than I.

You are of course exceedingly busy, but I would greatly appreciate any information which you could provide me on the subject of early twentieth century Russian genetics, either in the form of sources which I could use, collections of correspondence which might be made available to me, or personal recollections. In particular, if your schedule permits I should be delighted to meet with you and discuss the matters personally, in the form of a taped interview, an off-the-record personal conversation, or any other form that you deem most appropriate. I would appreciate hearing from you as to whether such a thing might be arranged.

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[6] *Adams M. B.* The founding of population genetics: Contribution of the Chetverikov school, 1924–1934 // Journal of the History of Biology. 1968. Vol. 1. № 1. P. 23–40; *Idem*. Toward a Synthesis: Population Concepts in Russian Evolutionary Thought, 1925–1935 // Journal of the History of Biology. 1970. Vol. 3. № 1. P. 107–129.

[7] Классики советской генетики. 1920–1940: Сб. статей / Отв. ред. П.М. Жуковский. Л.: Наука, 1968. 539 с.

[8] *Вавилов Н.И.* Избранные произведения: в 2 т. / ред. и коммент. Ф.Х. Бахтеева; Статья П.М. Жуковского. [Т.] 1–2. Л.: Наука, 1967. Т. 2.

[9] *Резник С.Е.* Николай Вавилов. М.: Молодая гвардия, 1968. 336 с. (Сер. «Жизнь замечательных людей»)

[10] *Philiptschenko Jur.* Variabilität und Variation: Mit 4 Textabb. Berlin: Borntraeger, 1927. [8], 101 s.

[11] *Полынин В.М.* Пророк в своем отечестве: [О Н.К. Кольцове]. М.: Сов. Россия, 1969. 127 с.

[12] После книги В.М. Полынина первая биография о Н.К. Кольцове была опубликована пять лет спустя: *Астауров Б.Л., Рокицкий П.Ф.* Николай Константинович Кольцов. М.: Наука, 1975. 168 с. Биографические статьи о Н.К. Кольцове, приуроченные к 100-летию со дня его рождения, были опубликованы два года спустя: *Канаев И.И.* Николай Константинович Кольцов (к 100-летию со дня рождения) // Цитология. 1972. Т. 14. № 9. С. 1201–1203; *Сидоров Б.Н.* Николай Константинович Кольцов // Генетика. 1972. Т. 8. № 8. С. 170–172.

Having been a participant in the Asilomar Conference on Philosophy of Biology several years ago[13], I became interested in the various dimensions of the problem of reduction and reductionism in biological explanation. There are a number of dimensions to the problem, as I see it: an epistemological dimension (concerning the nature of our biological knowledge and its logical and epistemological relationship to other forms of knowledge); an historical dimension (at what points in the history of biology, and within what disciplines and sub-disciplines have reductionism discussions occurred, under what intellectual and disciplinary circumstances and what was their intellectual and disciplinary outcome?); and a sociological dimension (how has the occurrence of reductionist debates differed from country to country and period to period, to what extent are these occurrences related to different forms of training, traditions, and ideological commitments?). It seems to me these matters are of obvious interest when we examine, for instance, biological mechanism in the seventeenth century, or the German reductionism of Du Bois-Reymond[14] and colleagues in 1848, or the current discussions of the last two decades of DNA and the biological code. In the course of studying the behavior of the Soviet scientific community toward Lysenko since Stalin's death, I happened upon a rather interesting debate occuring within the Soviet scientific community centering on reductionism in biological explanation. There were a great number of participants, including Engel'gardt[15], Frank[16], Dubinin[17], Oparin[18], Sisakian[19], Semenov[20] and

[13] Конференция по объяснению в биологии, прошедшая в Асиломарском конференц-центре в Калифорнии (Conference on Explanation in Biology held at Asilomar State Park, Monterey, California, in June 1968).

[14] Эмиль Генрих Дюбуа-Реймон (Du Bois-Reymond; 1818–1896) — немецкий физиолог, швейцарец по происхождению, философ, иностранный член-корреспондент Петербургской АН (1892). Основоположник электрофизиологии, автор молекулярной теории биопотенциалов, представитель механистического материализма.

[15] Энгельгардт Владимир Александрович (1894–1984) — советский биохимик, специалист в области молекулярной биологии. Академик Академии наук СССР, академик АМН СССР. Герой Социалистического Труда. В 1959–1984 гг. — директор Института радиационной и физико-химической биологии АН СССР (с 1964 года — Институт молекулярной биологии АН СССР), в организации которого участвовал.

[16] Глеб Михайлович Франк (1904–1976) — советский биофизик. Сын М.Л. Франка, брат И.М. Франка, племянник С.Л. Франка и Л.В. Зака. В 1943–1952 гг. заведующий Лабораторией биофизики изотопов и излучений АН СССР, на базе которой в 1952 г. в Москве был создан Институт биологической физики АН СССР. С 1957 г. директор этого института.

[17] Дубинин Николай Петрович (1907–1998) — советский генетик. В 1957–1959 гг. директор Института цитологии и генетики, в 1966–1981 гг. — директор Института общей генетики АН СССР. В 1981–1997 гг. зав. лабораторией мутагенеза того же института.

[18] Опарин Александр Иванович (1894–1980) — советский биолог и биохимик, создавший теорию возникновения жизни на Земле из абиотических компонентов; академик АН СССР (1946; член-корреспондент с 1939), Герой Социалистического Труда (1969), первый президент, а затем почётный член организованного в 1970 г. Международного научного общества по изучению возникновения жизни (International Society for the Study of the Origin of Life). В 1942–1960 гг. заведовал кафедрой биохимии растений МГУ, где читал курсы лекций по общей биохимии, технической биохимии, спецкурсы по энзимологии и по проблеме происхождения жизни.

[19] Сисакян Норайр Мартиросович (1907–1966) — советский биохимик, академик АН СССР (1960), академик АН Армянской ССР (1965). Основные труды по изучению закономерностей действия ферментов в процессе обмена веществ, биохимии засухоустойчивости растений, технической биохимии, космической биологии.

[20] Семёнов Николай Николаевич (1896–1986) — советский физико-химик, один из основоположников химической физики, академик АН СССР (1932), единственный советский лау-

Nesmeianov[21], as well as such philosophers as Frolov[22], Kedrov[23], and Kremianskii[24]. In my view, this particular discussion, which was one of the central issues in Soviet philosophy for much of a decade, can only be fully appreciated when all three of the above dimensions of the discussion are understood.

I am presently preparing a study of this debate, and I would be delighted to send you a copy when it is completed.

With best wishes,

Yours truly,
Best regards,
Mark B. Adams
Assistant Professor

№ 2
November 16, 1970, New York

November 16, 1970

Dr. Mark B. Adams
Department of History and Philosophy of Science
University of Pennsylvania
Philadelphia, 19104, Pennsylvania

Dear Dr. Adams:

I shall be delighted to have a "bull session" with you, and reminisce about evolutionary biology in Russia, 1920–1927, a period well known and remembered. Reminiscing is, of course, one of the few pleasures left in old age.

My colleague Ayala and myself are very concerned about the problem of reductionism in biology, since neither of us was invited to Asilomar[25], your references to discussions there sound to me rather cryptic. I am also unfamiliar with the discussion in Russia to which you refer, and would be glad to learn about it.

реат Нобелевской премии по химии (1956; совместно с Сирилом Хиншвудом). Дважды Герой Социалистического Труда (1966, 1976).

[21] Несмеянов Александр Николаевич (1899–1980) — советский химик-органик, организатор советской науки. Президент Академии наук СССР в 1951–1961 гг., ректор Московского университета, академик АН СССР (1943). Дважды Герой Социалистического Труда (1969, 1979).

[22] Фролов Иван Тимофеевич (1929–1999) — советский и российский философ, партийный деятель, в 1990–1991 гг. — член Политбюро ЦК КПСС, в 1989–1990 гг. — секретарь ЦК КПСС, в 1989–1991 гг. — главный редактор газеты «Правда», академик АН СССР (1987), академик РАН (1991).

[23] Кедров Бонифатий Михайлович (1903–1985) — советский философ, логик, химик, историк и методолог науки, психолог, популяризатор науки, специалист в области материалистической диалектики и философских вопросов естествознания, академик АН СССР (1966).

[24] Кремянский Виктор Израилевич (1909–1986) — советский философ, специалист по методологическим проблемам биологии, кибернетики и теории информации. С 1963 г. — в Институте философии АН СССР.

[25] Конференция по объяснению в биологии, прошедшая в Асиломарском конференц-центре в Калифорнии (Conference on Explanation in Biology held at Asilomar State Park, Monterey, California, in June 1968).

Nov. 25 — Dec. 1 I hope to be in England, and Dec. 14 — early January in South America. We could meet early in December or in January. My laboratory phone number is 360–1556; it should be possible to set a mutually satisfactory date.

Sincerely
Theodosius Dobzhansky

TD: mp

№ 3
June 3, 1974, Philadelphia

UNIVERSITY OF PENNSYLVANIA
PHILADELPHIA 19174

Department of History and
Sociology of Science
Edgar Fahs Smith Hall D 6
215–594–8400

June 3, 1974

Professor Theodosius Dobzhansky
Department of Genetics
University of California, Davis
Davis, California 95616

Dear Professor Dobzhansky:

It was a great pleasure and honor to meet and chat with you at the American Academy of Arts and Sciences conference on the "Synthetic Theory of Evolution"[26]. You will recall that I mentionned to you a book I was writing on genetics and the Soviet scientific community, 1948–1965. It will be a rewrite of my doctoral dissertation[27], and has been accepted for publication by Chicago University Press[28]. I have taken the liberty of sending you a copy of this work airmail special delivery. I realize you are very busy and will be leaving the 16th of June for the summer. On the other hand, if you could find time to read the MS and criticize it before you leave for the summer, I would be very much in your debt. I have agreed to deliver the final version to the Press by July 15, 1974. You obviously know a great deal more about my subject — firsthand — than just about anyone else in the West, which makes you my ideal critic. I should very much like to have your corrections, criticisms, and responses to my interpretations before publishing the book. Specific comments about specific passages would be especially useful to me in revising the MS. General comments

[26] Конференция «Эволюционный синтез: перспективы объединения биологии», состоявшая из двух симпозиумов, прошедших 23–25 мая и 11–12 октября 1974 г., была организована Комитетом по современной истории науки и техники Американской Академии искусств и наук (The Committee on the Recent History of Science and Technology of the American Academy of Arts and Sciences) за счёт гранта Национального научного фонда (National Science Foundation). См.: The Evolutionary synthesis: perspectives on the unification of biology / ed. by Ernst Mayr and W.B. Provine. Cambridge, Mass.: Harvard University Press, 1980. P. ix–xi.

[27] *Adams M.B.* Genetics and the Soviet Scientific Community, 1948–1965. (Unpublished Ph.D. dissertation, Harvard University). Ann Arbor: University Microfilms, 1972.

[28] *Adams M.B.* In the Name of Science: Genetics and the Soviet Scientific Community, 1948–1965. Chicago: Chicago University Press, 1977. 320 p. Книга была принята к публикации в издательстве, но так и не была опубликована.

about length, organization, and general tone of treatment would also of course be enormously valuable to me. Let me tell you in advance that I plan to completely rewrite chapter one. I want to create an introductory chapter which will tell more about Russian biology and genetics, the role of the Russians in the "synthesis", and the setting for Lysenko's rise; general comments on the situation before 1948 and the various interpretations found in Western literature; and my methodological approach. Any suggestions as to what kinds of things I should be sure to cover in the first chapter would also be extraordinarily helpful.

My next major research undertaking will concern the development of evolutionary biology and genetics in Russia and the Soviet Union, ca. 1885–1930. I'm especially interested in Russian traditions and approaches and their genesis, the role of naturalists, their relationship with experimentalism and how the latter developed, the attempts at synthesis in 20s and 30s, and the eugenics movement. Perhaps you may have reprints or access to articles by Serebrovsky and others on eugenics. Your wealth of personal experience in the Russian experience in the teens and twenties seems to me the historian's dream. I realize that to a fine scientist with a cutting mind (well trained in the use of Occam's razor) much of this story might appear to be gossip or irrelevant or better left unsaid. As an historian, however, I consider it vital that such perspective and detailed knowledge be recorded for posterity. Obviously, you might wish to have some say as to what knowledge can be published under what circumstances. Subject to this constraint, do you think it would be possible for you to set aside two or three days to recall out loud some of your experiences and to enunciate your perspective on these events? What I have in mind is a lengthy taped interview. I would be happy to come to California any time at your convenience to conduct such an interview. Perhaps one session, followed by several months of follow-up reading, research, and thought on my part, followed by a second interview is a format that would make good sense. Is this feasible from your point of view? Please do let me know. If you could free up the time before June l8th, I could come out for three days now; or perhaps the fall would be a better time for you. Please do let me know whether you are interested and what would be the best time for you.

Let me conclude by remarking that I found your paper at the conference[29] terribly useful — both in confirming my own working suspicions, and in adding new information and perspectives. I look forward to hearing from you about the MS and interviews.

Best regards,
Mark B. Adams
Bers Professor of the History and Sociology of Science

P.S. I was looking forward to Sunday dinner with you, but Ernst Mayr[30] informed me on the phone on Sunday that you and he were having dinner together because you wanted to discuss some things, so I felt it better not to intrude.

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[29] *Dobzhansky Th*. The Birth of the Genetic Theory of Evolution in the Soviet Union in the 1920s // The Evolutionary synthesis: Perspectives on the unification of biology / eds. Mayr E. and Provine W.B. Cambridge, Mass.: Harvard University Press, 1980. P. 229–242.

[30] Эрнст Вальтер Майр (Mayr Ernst Walter; 1904–2005) — немецкий и американский зоолог, систематик, эволюционист, один из создателей «синтетической теории эволюции», член Национальной академии наук США (1954).

№ 4
July 21, 1974, Philadelphia

July 21, 1974

Dear Doby,

I want to give you my heartfelt thanks for a splendid and productive week. There is inevitably a feeling of intrusion that any historian feels when he comes into a great man's world for any length of time, and I hope you will forgive any disorder or disharmony I may have introduced into Mather, a *Sanctum Sanctorum* of the most spiritual sort. And yet I never felt like an intruder: you gave me the best of all possible welcomes, that of being one of the crew, sharing the work and feeling very much at ease and at home and with friends. I also want thank you for the regular and business-like way the interviews went, and the time you graciously set aside for them from work and contemplation. I am happy to report that I have listened again to the tapes and that the sound is fine. If I became a bit testy in Davis, or appeared quick or distracted, I hope you will understand that it stemmed from frustration that a splendid week was drawing to an end, and a sense that there was so much more to discuss and explore with you, had I been somehow more adequate to the task.

As it was, you gave me a week that always will always mean a great deal to me. Papers, texts, publications, even correspondence are the métier of the historian and they are a familiar and comfortable source for me. But being with the man himself lent a whole human dimension, a living thread to the tapestry of my studies that will make me a better historian, I hope.

I plan to be studying Russian biology ca. 1860–1930 for a long while to come, and at Mather the teens and twenties gained a new presence and vividness that have already sent me back to my texts with renewed vigor. I am touched and appreciative. And being taught how the Drosophila collecting is done by the master himself is something I shall long remember. I will be sending copies of pictures when they come back from developing.

On the whole I am very pleased with the ways the interviews went. I hope you were. Let me assure you that the interview was more organized and thought through than it may have appeared. I simply did not wish to lose the spontaneity by setting forth the agenda too mechanically. Let me also assure you that my understanding of spoken Russian is better than my ability to speak it: you must have been dismayed by the plethora of simple errors which I hear myself making on the tapes. Finally, rest assured that I understood what you were saying at virtually every point: if a subsequent question appeared to be based on a misconception of some earlier comment, it was usually because I was trying to get you to elaborate and spell out the point at greater length — a nasty trick, perhaps, but an interviewer's stock in trade in his struggle to record the living word for history. I am still keeping very much in mind my pledge to not use anything on the tape without your permission.

You may recall that at Mather I asked several favors of you which you were kind enough to agree to. Let me just set them forth in a numbered way to remind you if you can find a few moments between Mexico[31] and Montpelier[32]:

1. A letter to Living History project giving me permission to have access to your 1961 (?) interview[33] — to them with copy to me or vice versa, whatever you think appropriate.

[31] Ф.Г. Добржанский был в Мексике с 13 по 17 марта 1974 г. на II съезде национального мексиканского общества генетиков (II Reunion Nacional de la Sociedad Mexicana de genetica).

[32] Ф.Г. Добржанский был во Франции, в т.ч. в Монпелье с 3 сентября 1974 по 10 октября 1974 г., затем совершил поездку в Испанию, и вновь был во Франции с 17 октября по 27 ноября 1974 г.

[33] The Reminiscences of Theodosius Dobzhansky. Part I. Columbia University, Oral History Research Office. New York, 1962. 639 p.

2. Your personal correspondence with Филипченко[34] and Керкис[35] especially, and anything appropriate from Серебровский and others[36] we discussed if you are willing — for copying.
3. Copies of photographs relevant to our discussions. As we discussed, our department has equipment for "photographing" photographs — or you could make copies, whichever is most convenient. The first nine in the Land book[37] seem directly relevant — plus several others that I recall from memory: your parents (earlier and later); and several group portraits, e. g. at Peterhof[38]; class portraits; the ones of Филипченко and Кушакевич[39].
4. Copies of publications which are rare or difficult to find; especially:
   A. С. Четвериков, «Волны жизни» (1905)[40].
   B. The "early" Добржанский papers in Russian[41].
   C. The offprint files you showed me of Филипченко and Серебровский.

Let me explore one other matter. In Davis, Jeff (a splendid and worthy Добржанский student![42]) showed me your "library". As he may have reported to you, I was delighted and aghast at the treasure trove of materials on Russian genetics — some of which I had not been able to locate, even in the Soviet Union! Two caches especially caught my eye — the collection of Lysenkoite materials from the 1930s-1950s (for example, Корпускулярная генетика by Фейгенсон[43]); and, more importantly, at the bottom of one of the shelves on the left toward the center, a series of first editions of Филипченко works (for example, 2 complete copies of the two-volume Частная генетика[44], and 5 or 6 other works). Having searched in Washington, Boston, Philadelphia, and New York for these works unsuccessfully, I had been trying unsuccessfully to get xeroxes made (or microfilms) in the Soviet Union. These works are of some importance to me since, after editing the Lysenko book, I was planning to launch a study of Филипченко. Would it be possible, do you suppose, for me to make the copies myself or have you make them — perhaps the first would be easier. And in the case of xeroxing, I would personally make the copies to insure proper treatment of the originals.

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[34] Максимум возможного (Переписка Ф.Г. Добржанского с отечественными биологами: 1920–1970 гг.). Ч. 1: Переписка Ф.Г. Добржанского с отечественными биологами: 1920–1930-е гг. / ред.-сост. М.Б. Конашев. СПб.: Нестор-История, 2014. С. 7–252, 253–396.

[35] Максимум возможного (Переписка Ф.Г. Добржанского с отечественными биологами: 1920–1970 гг.). Ч. 2. Переписка 1950–1970-х годов. СПб.: Нестор-История, 2018. (В печати.)

[36] Там же.

[37] *Land B.* Evolution of a Scientist: The Two Worlds of Th. Dobzhansky. New York: Thomas Y. Crowell Company, 1973. 263 p.

[38] Фотографии, сделанные в Петергофском естественно-научном институте (ПЕНИ) в Старом Петергофе, под Ленинградом, во второй половине 1920-х гг., где Ю.А. Филипченко летние месяцы, как правило, проводил, работая в лаборатории экспериментальной зоологии и генетики ПЕНИ. Лаборатория была основана Ю.А. Филипченко в 1920 г.

[39] Кушакевич Сергей Ефимович (1873–1920), зоолог. С 1915 г. проф. Киевского университета, с 1919 г. директор Днепровской биологической станции в Староселье, под Киевом.

[40] *Четвериков С.С.* Волны жизни: (Из лепидоптерологических наблюдений за лето 1903 г.) // Дневник Зоологического отделения Имп. общества любителей естествознания, антропологии и этнографии. 1905. Т. 3. № 6. С. 106–111.

[41] См.: Theodosius Dobzhansky: Career Summary and Bibliography // Evolutionary biology. 1976. Vol. 9. P. 413–416.

[42] Джеффри Р. Пауэлл (Jeff R. Powell) — американский генетик, ученик Ф.Г. Добржанского.

[43] *Фейгинсон Н.И.* Корпускулярная генетика: Критический обзор. М.: Сельхозиздат, 1963. 544 с.

[44] *Филипченко Ю.А.* Частная генетика: Часть 1: Растения. Л.: книгоизд-во «Сеятель» Е.В. Высоцкого, 1927. 239 с.; Ч. 2. Животные. Л., 1928. 279 с.

Believe me, I do not make these requests lightly; I realize the materials I wish to borrow briefly are invaluable and irreplaceable — and they would be treated with great care and would not leave my hands, even to be copied. Let me add that obviously, I would expect to pay all mailing or other costs, whether incurred here or at Davis. Finally, let me urge you to feel free to refuse any of the materials for whatever reason, and I will understand perfectly.

But, enough of my future research plans. The main reason I am writing this letter (at 7:00 a. m., since I am now attempting to emulate the earlier rising that I became enamored of at Mather) is to thank you and let you know how much that week meant to me — (please forgive any Barbara Land-like lapses into sentimentality!) — and, as it happens, to thank you for the best birthday gift ever! I turned 30 on July 3 while we were at Mather, and do think that it was the most enjoyable and splendid birthday I have had.

Warmest regards,
Mark Adams

P.S. — It just occurred to me that I never did do my Ford[45] imitation for you. The next time I see you, remind me to get soused so that I can get the manner just right for you.

№ 5
August 22, 1974, Philadelphia

UNIVERSITY OF PENNSYLVANIA
PHILADELPHIA 19174

Department of History and
Sociology of Science
Edgar Fahs Smith Hall D 6
215–594–8400

August 22, 1974

Dear Doby:

Your letter has made my decade! Your offer of the Russian materials from your library goes well beyond my wildest hopes — and I have real difficulty finding the words to express how much this will mean to me and to my work. As you know, for some time I have been wanting to do some fairly definitive work on Russian genetics in the twenties. The difficulty has always been getting to the primary materials — between the difficulty of travel and the Soviet bureaucracy I had almost given up hope. And now your letter changes all that! I can see the kind of work I will be doing for the next five or six years quite clearly now: a monograph on Филипченко; one on the Russian eugenics movement; one on the Кольцов institute; perhaps one on the Серебровский group; and one or several on the great Добржанский himself!

I would be delighted to come and pack up the "treasures" and would like very much for you to be there so that you can know what I am taking and to decide what you may wish to keep handy. The delay until December poses no problem whatever for me. If you will let me know what would be a good time after your return from France, I will be happy to go to California, rent a car, come up and pack them for shipment. The best times for me are from December 22nd, 1974 through January

[45] Эдмунд Б. Форд (E.B. Ford, 1901–1988) — английский генетик, друг Ф.Г. Добржанского. Марк Б. Адамс встретился с ним на конференции «Эволюционный синтез: перспективы объединения биологии» в 1974 г.

12th or 15th, 1975. If you will have some time, perhaps I could have a brief follow-up interview based on questions that arise from the Land Oral History interview[46] and the Mather tapes.

I was very sorry to learn of your traumatic encounter with the Mexican underworld in Mexico city. What a hassle! I do not envy you the nitty-gritty work of replacing all your документы, but I trust that your forthcoming French sojourn puts a more positive complexion on that troublesome enterprise.

I trust you will have a pleasant and productive stay in France and I look forward to seeing you at year's end.

Best regards —
Mark

P.S. Many thanks for the letter to the Oral History Project.

№ 6
April 26, 1975, Philadelphia

UNIVERSITY OF PENNSYLVANIA
PHILADELPHIA 19174

Department of History and
Sociology of Science
Edgar Fahs Smith Hall D 6
215–594–8400

April 26, 1975

Dear Doby,

Many thanks for your letter several months ago! I had planned to come out during Spring Vacation, but caught a nasty cold and decided to put off the trip.

Chicago University press has been patiently awaiting the manuscript's final version, but as yet I have not finished it. I am trying to take into account the comments you were so kind to offer as well as those of others, but the main task is to write the first chapter to put the whole thing in perspective. As usual in such enterprises, since it will be my first book, I am trying to get everything just right. I am told the Spanish have a saying, "the perfect is enemy of the good" — and I suspect it applies here: there may be something to suppressing manfully one's perfectionist tendencies and just getting it out in good form. (After all, if nature only takes 9 months, why should I have to take 2 years?) My courses this term have also demanded most of my time: its on the history of biology (esp[ecially] evolution & inheritance) and I rewrite the lectures every year.

But at long last, the term has come to an end: I must merely grade the papers and fill in the sheets. The first chapter of the book will be done, together with the revisions, by July 15, 1975. And after some eight years of discontinuous effort I can go on to a new research interest. (I am already getting excited!). This will be Russian/Soviet biology ca. 1890–1935. My first line of attack will be to look at the 'teens and 'twenties in minute detail, and this should take me a couple of years. I want to find out about all the figures who were working actively in the twenties, focusing on Филипченко, Вавилов, Кольцов, Четвериков, Серебровский and their contemporaries. Obviously I will be concerned with their younger colleagues and the éminences grises

[46] The Reminiscences of Theodosius Dobzhansky. Part I. Columbia University, Oral History Research Office. New York, 1962. 639 p.

as well — I want to read virtually all that was written during the period, get some sense of the "total population" of biologists during the time, and to study the development of thinking on the nexus of questions that agitated biologists there then: questions of evolutionary mechanisms & regularities, inheritance of acquired characteristics, connections between development & heredity, the introduction (via Филипченко) of "Western" (Morgan school et al.) genetics, developments in population genetics, interests in eugenics, agriculture and other "links" with other fields that interested or seemed relevant to biologists then.

Lest this appear hopelessly Rankean[47], I do have some foci: key institutions, and how they developed (or were founded) through this tumultuous period; key figures and their intellectual & organizational impact; and aggregate figures on courses, students, publications, etc. Unlike my work on post-Stalinist Soviet biology, I do not intend to keep this stuff in the closet for a decade or two: I want to pace myself by getting out articles along the way, and I have some idea of which and when.

After the book, I want to get out a quick article on dialectical materialism and Soviet science (1948–1970). You had read my MS as indicating support for Loren Graham's[48] approach — and so had Michael Lerner[49]. Since I am not a supporter of Graham's[50] view, this indicates to me that my writing on this point was not sufficiently clear. And rather than clutter the book MS with clarifications, I feel I should get out an article which states my views clearly.

Then I had promised an article for Mayr's volumes[51]. At the fall conference, I had presented a paper on "Severtsev & Shmalhausen: Morphology and the Synthetic Theory"[52]. This got to be somewhat far afield from population genetics (until mid-late Shmalhausen). But I have copies of most of their major works and I gave an interesting & focussed (I think) paper and I want to follow it up. These two projects will take about two weeks each, since I've done all the work and research on them.

Then on to the "new" research I've been collecting on Филипченко, Кольцов, et al. for a number of years, and now I'll get a chance to really get into it. I think the ultimate goal will be an effort of booklength, but I will begin by putting together two articles: "Soviet Eugenics"[53] and "Filipchenko"[54]. I think that our "current" perspective on eugenics is colored by our current views & realizations, so that we tend to ignore, or misinterpret, what "eugenics" would have meant to, e. g., Soviet biologists in the Twenties.

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[47] Производное от имени Leopold von Ranke (Леопольд фон Ранке, 1795–1886) — немецкого историка, основателя исторической школы, отдававшего приоритет исследованию архивных источников и анализу исторических документов. В данном случае имеется в виду подход к истории как простому составлению хроники с включением как можно большего числа точных фактических деталей о прошлом без какой-либо попытки оценить или интерпретировать их значение.

[48] Лорен Р. Грэм (Loren R. Graham; b. 1933) — известный американский историк науки, специалист по истории российской и советской науки.

[49] Исидор (Михаил) Михайлович Лернер (Michael (Isidore) M. Lerner; 1910–1977) — американский генетик русского происхождения.

[50] Вероятно, имеется в виду книга: *Graham L.* Science and Philosophy in the Soviet Union, Alfred Knopf, 1972. 584 p.

[51] The Evolutionary synthesis: perspectives on the unification of biology / E. Mayr, W.B. Provine (eds.) Cambridge, Mass.: Harvard University Press, 1980. 487 p.

[52] *Adams M.B.* Severtsov and Schmalhausen: Russian Morphology and the Evolutionary Synthesis // The Evolutionary synthesis: perspectives on the unification of biology / E. Mayr, W.B. Provine (eds.) Cambridge, Mass.: Harvard University Press, 1980. P. 193–225.

[53] *Adams M.B.* The politics of human heredity in the USSR, 1920–1940 // Genome. 1989. Vol. 31. № 2. P. 879–884.

[54] *Adams M.B.* Filipchenko [Philipchenko], Iurii Aleksandrovich // Dictionary of Scientific Biography. 1990. Vol. 17. Suppl. II. P. 297–303.

You can understand my delight, appreciation, and gratitude at your offer of your relevant "Russian" library. It will be my primary source of study for at least the next decade! It literally makes possible for me the kind of studies which would be very difficult to do without it, even (especially?) in the Soviet Union. I am something of a "Bulldog" for accuracy & thoroughness in the research stage…

I am pretty much at your disposal for the visit, beginning about May 8th. The best time for me would be mid-June, but I know your schedule is much more demanding than mine. Ideally, I should like to come out for 4 or 5 days. I should like to pack up and mail out the materials, but especially I look forward to the chance to exchange views & information with you, to follow up the earlier interviews formally and informally (I still remember Борис![55]). I certainly don't want to impose in any way on you. I would be delighted to stay at your house, but I would find no trouble whatever finding other accommodations. Will I have any difficulty getting mailing envelopes (padded) in Davis? I'll come with mailing stickers, so it will just be a question of inserting, stapling, sticking, and mailing (I think this will be the safest way — boxes getting wrecked fairly easily).

So much is relevant that it might be best simply to lay out what I'd like, from which you can decide what you wish to give me. Simply stated, it falls into 4 categories:

(1) Russian language books (Lysenkoist, works of Filipchenko, etc.). I think I saw perhaps 80 or 90 that would be useful.

(2) Runs of Russian biology journals, esp. «Журнал экспериментальной биологии», «Биологический журнал», and a couple others that I saw (esp. 1910s through say 1945, & even more recent).

(3) Correspondence with Russian biologists (as complete as you wish to make it) — needless-to-say I will use great discretion & follow your advice completely on what may be quoted vs. paraphrased vs. treated privately for my own background.

(4) Your own personal journals, papers, etc., especially from pre-1927, but also through the 40s.

This is a lot — so feel free to include or cut out whatever you want! Hoping to hear from you. & I really look forward to renewing & deepening our relationship.

Best regards,
Mark

№ 7
June 4, 1975, Philadelphia

June 4, 1975

Dear Doby,

As you can see, I arrived safely in Albequerque[56] and my friend Michael Krausz[57] and I are soaking up sun, swimming, talking, and planning a 3-day trip up to Taos[58], Los Alamos[59], and Choco Canyon[60]. All in all it should round out a splendid trip in a very stimulating way.

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[55] Согласно М. Адамсу, имеется в виду любимая опера Ф.Г. Добржанского «Борис Годунов», которую они слушали вместе вечером в доме Ф.Г. Добржанского.

[56] Альбукерке — город на юго-западе США, штат Нью-Мексико.

[57] Михаил Крауз (Michael Krausz, b. 1942) — американский философ швейцарского происхождения, художник и дирижер, проф. философии в колледже Брин Мор (Bryn Mawr College).

[58] Таос — город на юго-западе США, административный центр округа Таос штата Нью-Мексико, центр индейской культуры.

[59] Лос-А́ламос — населённый пункт и округ в штате Нью-Мексико, США.

[60] Каньоне Чако — расположен на северо-западе штата Нью-Мексико, между городами Альбукерке и Фарминтон.

I wouldn't feel right leaving, however, without first writing to thank you. Needless-to-say, all the materials which you most graciously gave me are going to keep me busy for a couple of years. Indeed, I can hardly wait to get back and finish xeroxing[61], organizing, and studying them. It is hard to express what it means to me to be launched on a project full of interest and importance, fearing a lack of materials — and to unexpectedly gain access to a literal treasure trove of just the sort of thing I need — the historian's "meal". I am very, very grateful — and very very anxious to get started — and very very excited.

As the work gets going — possibly in late summer — I will want to be in touch with you. I will undoubtedly need some help in deciphering the xeroxed letters. I may also ask you to check to see if you have some other reprints by relevant authors — we never did get a chance to go over those files in enough detail, I fear. Also, I will inquire about copying photographs and see if some simple & safe arrangement for making copies can be worked out. For the book to be based on those materials, I hope it would be alright to include some of these photos — they will add some life & sense of actuality to the narrative.

Most memorable for me, however, was the chance to spend time with you and renew our friendship. It has meant and means a great deal to me. You were, as always, the perfect host & a thorough delight and stimulating interlocutor. I always leave our meetings slightly "high" from the experience!

Again, I trust and hope that I did not get too much under foot — and that you have subsequently found time to make the MS Addition. Also I'm delighted that I was able to find that cache of letters — it is so nice to be able to do something for you for a change!

Fond regards,
Mark

№ 8
July 7, 1975, Philadelphia

UNIVERSITY OF PENNSYLVANIA
PHILADELPHIA 19174

Prof. Mark Adams[62]
Department of History and
Sociology of Science
Edgar Fahs Smith Hall D 6
215–594–8400

July 7, 1975

Dear Doby —

Thanks for your note of June 8th. I trust you got my letter from Albequerque. You were right — Sante Fe and Taos were far more interesting places. I returned here June 19th in the evening but only stayed a few hours. Just after entering my apartment, I got a call from my brother saying that my father had died suddenly of a massive myocardial infarction two days before, on the 17th. I left that same evening for Florida to put affairs in order, comfort my brother and mother, attend memorial services, and so forth. I derive some comfort knowing that he was never in any pain — from all

[61] Производное от названия компании Xerox (Xerox Corporation), которая первой стала массово производить аппараты для копирования.

[62] Вписано синими чернилами. Весь остальной текст письма написан черными чернилами.

accounts, it was a matter of seconds — and that he had spent the last two years having the time of his life. Still, the sense of loss creeps up on me from time to time, and it will be months before the whole thing has sunk in and been dealt with. Ah well, it was inevitable sometime.

All the materials arrived in very good shape. The batch of letters (originals) are being microfilmed — I took them to the Associate Director of the library, and explained the situation to him, and he guaranteed utmost care with the originals and a first-class job.

I will be getting them back to you in about a week or two. Again, I apologize for the delay but considering circumstances it is the best I could do. I have unpacked, read through, and organized the books and journals — great, great stuff! In fact I'll be using it in preparing the new first chapter of the book. I hope you won't mind me thanking you in the preface!

My schedule is first to finish the book; then to write up two articles for the Mayr volumes (Четвериков group and Северцов/Шмальгаузен)[63]. I will also be getting sabbatical year after this coming one (1976–1977) and will be applying for a grant to work on the Russian materials. May I give your name as a reference? Few appreciate the value and interest of such a study as much as you do.

Let me thank you for another splendid week! It would be hard for me to describe to you how much these times spent with you mean for me. I know from personal experience that such encounters are always harder for the host than the guest and I hope once again that I did not get too much "under foot". Do express my appreciation to Francisco[64] and Mitzi[65] for having me to their feast and to Mike Andress for his various labors on my behalf.

In a few days I will send you my notes on your Morgan paper for Mayr[66] — it arrived in a batch of stuff. For the moment — many thanks. Hope to see you at Mather or Davis (or Philly[67] or Boston) before too long.

Mark

№ 9

October 19, 1975, Philadelphia

October 19, 1975

Dear Doby,

I was talking to Ernst[68] on the phone last Wednesday and he mentionned that you had written him a note mentionning an attack of meningitis which had caused you to be in a coma for a day or so[69]. Since then, I have tried to call you off and on but having received no answer, I had gotten

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[63] *Adams M. B.* Sergei Chetverikov, the Kol'tsov Institute, and the Evolutionary Synthesis // The Evolutionary synthesis: perspectives on the unification of biology / E. Mayr, W.B. Provine (eds.) Cambridge, Mass.: Harvard University Press, 1980. P. 242–278*; Idem*. Severtsov and Schmanlhausen: Russian Morphology and the Evolutionary Synthesis // The Evolutionary synthesis: perspectives on the unification of biology / E. Mayr, W.B. Provine (eds.) Cambridge, Mass.: Harvard University Press, 1980. P. 193–225.

[64] Франциско Аяла (Francisco Ayala), американский генетик.

[65] Митци Аяла (Mitzi Ayala), жена Франциско Аяла.

[66] *Dobzhansky Th*. Morgan and His School in the 1930s // The Evolutionary synthesis: perspectives on the unification of biology / E. Mayr, W.B. Provine (eds.) Cambridge, Mass.: Harvard University Press, 1980. P. 445–452.

[67] Сокращенное название города Филадельфия.

[68] Эрнст Майр (Ernst Mayr), американский биолог.

[69] 12 сентября 1975 г., утром, Ф. Аяла, после того как Ф.Г. Добржанский не отвечал на его телефонные звонки, поехал к нему домой и нашёл его в кровати без сознания и вызвал машину скорой помощи, которая доставила Ф.Г. Добржанского в госпиталь. Сутки или больше он находился в реанимации. А 16 сентября уже сделал запись о происшедшем на английском и русском в своем дневнике.

a little concerned. Finally I called Francisco[70] today and he set my mind at ease. He tells me you had a close brush with death but have emerged as the same old Doby, only more so. Ernst also mentionned that you are back to work, writing articles of the usual lucidity, at the usual rate. I should have known that an indomitable spirit such as yours would bounce back! It is hard putting such things in words, but as you may have gathered, during the short period of our acquaintance, not only has my considerable respect for you deepened, but I have come to feel a great affection for you. I feel greatly relieved to know that you have resumed your hectic pace.

Let me relieve some guilt feelings by finally commenting on the brief piece you wrote on Morgan for Mayr's volumes[71]. Rest assured that if I had had any substantive criticism to make, you would have heard from me much sooner. As usual, I am awestruck by the absolute mastery over English style which you manifest in everything you write: you really do put such native speakers as myself to shame. Everything is so gracefully put, so lucid, that there is really very little to be said. The piece that emerges is one that covers the gamut of Morgan's personality, style, philosophy, and thought, always with balance and in proportion. You are, as historian, a "natural". If I were asked to make any suggestions, they might be that you are too good as an historian: the article is full of balance and perspective. Personally, I would have enjoyed reading more personal reminiscences: all the little episodes involving Morgan and his lab that you can recall from personal experience. Your article could not have been a better secondary source. While its value as secondary material would have been threatened by too much personal aside and intrusion, still perhaps its value as a primary source would have been increased. As you see, a minor comment. I'm not sure you'll wish to make any changes as a result.

Your productivity puts me to shame! Since I have seen you last, I have received the articles and reviews you sent me, and have read them with great interest. I wish I had a comparable number to send you, but, alas, have not. Rest assured that as and when my writings appear in print, you will receive copies for your criticism, perusal, and amusement. Also — forgive my typing the letter. It is less personal, or may seem so, than a handwritten letter. On the other hand, I am a fairly good typist, my typing is more legible than my handwriting, and for me it is an equally personal communication: indeed, I can be somewhat more conversational, since I type much more quickly than I write in longhand.

Francisco mentionned that you had been concerned about some missing articles by Timofeeff-Ressovsky, especially in German. Let me set your mind at ease in one respect: when we were going through your reprint files, I had mentionned T-R; you responded, "do you consider him a Russian"? I indicated that I did, so you gave me your entire reprint collection of his works. If you would like them back, I would be happy to make xeroxes for myself and send you the originals. Just let me know.

I hope I may intrude with two items of business. First, I am applying to do research on the history of Russian biology in the Soviet Union for next year, when I have my leave coming up. I am applying to IREX (International Research and Exchanges Board) and the National Academy of Sciences (just to cover myself in case I don't get one). In addition, I am applying for a 2-year grant for the National Science Foundation. My basic concerns will be the development of Russian biology from roughly 1870 through 1930. I will have to be careful how I phrase the matter: my most immediate concerns are the developments in the twenties — Philipchenko, Kol'tsov, Serebrovsky, Chetverikov, Vavilov, Dobrzhansky et.al. But I'm not sure the Soviets will cotton to my prying into their archives, especially during the post-Revolutionary years. So how I state the issue will require careful thought.

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[70] Имеется в виду Ф. Аяла.

[71] *Dobzhansky Th.* Morgan and His School in the 1930s // The Evolutionary synthesis: perspectives on the unification of biology / E. Mayr, W.B. Provine (eds.) Cambridge, Mass.: Harvard University Press, 1980. P. 445–452.

Would you be willing to write recommendations (or one letter thrice sent) for my applications? I would greatly appreciate it, since you know my interests and the value of exploring these topics as much as anyone. I also appreciate how busy you are with your scientific work. If these recommendations would pose any problems for you, or would intrude into your busy schedule too much, please let me know. I quite understand, and regret having to impose upon you in this way.

Second: I will be visiting California around Christmas time. As I wrote you, my father died June 17 (shortly after I last saw you). As you know, my brother and his family live in California, and we are gathering there (my mother is coming from Florida) for the holidays. Will you be in Davis? Can you be visited? Would you mind my staying a night or two? I would like to have a look, and maybe do some xeroxing, from Genetika (you may find it hard to believe, but no Philadelphia library takes it!) but of course my main reason for coming would be to visit you. Again, let me know whether this fits your schedule. I'll be in California from roughly December 20 through January 5 or so: I do hope you'll have some time.

I've been asked to write the biographical article on Nikolai Ivanovich Vavilov for the Dictionary of Scientific Biography[72]. Needless-to-say, your various books and reprints are coming in very handy indeed.

Please give my very best regards to those of your friends I was privileged to meet on my last visit.

With warmest personal regards,
Mark

№ 10
October 21, 1975, Davis

UNIVERSITY OF CALIFORNIA, DAVIS

COLLEGE OF AGRICULTURAL AND
ENVIRONMENTAL SCIENCES
AGRICULTURAL EXPERIMENT STATION
DEPARTMENT OF GENETICS

DAVIS, CALIFORNIA 95616

October 21, 1975

Prof. Mark Adams
Department of History and Sociology of Science
Edgar Fans Smith Hall D 6 University of Pennsylvania
Philadelphia, Penn. 19174

Dear Mark:

Time is coming when I must think about sending my correspondence, etc. to the library of the Philosophical Society, as promised almost 10 years ago. I have already forgotten the name of the Librarian who at one time even said that he may come to New York to pack these materials (surely Davis is too far, and this kind of help is not really necessary). You probably are well acquainted with people in charge of the geneticists archives, so, please, give me your and their instructions and advice.

I have preserved only "selected" correspondence older than the move to Davis (1971), but whatever there is may perhaps be of interest. Among the "selected" materials are the Russian let-

[72] *Adams M.B.* Vavilov, Nikolai Ivanovich // Dictionary of Scientific Biography. 1990. Vol. 15. Suppl. I. P. 505–513.

ters with which you are familiar. Also have a lot of diaries, chiefly of my trips in the world at large (very seldom make entries while living "at home"). Finally, the transcripts of the Oral History — do they want these? Also, do they want the laboratory notebooks for the last 10 or so years?

In September I had a pretty serious illness, but at present am fairly well recovered, except for deafness which necessitates a "hearing aid". As expected, the hearing aid is only a weak "ersatz" for what natural selection has wrought. Anyway, it is joy to be working in the lab, looking at flies and chromosomes, reading and writing papers.

How is your book (or books)? What are other personal and general news?

Cordially
Doby
Theodosius Dobzhansky

TD: ct

№ 11
November 11, 1975, Philadelphia

UNIVERSITY OF PENNSYLVANIA
PHILADELPHIA 19174

Department of History and
Sociology of Science
Edgar Fahs Smith Hall D 6
215–594–8400

November 11, 1975

Dear Doby:

I have taken the liberty of enclosing the recommendation form for the National Academy of Sciences/Soviet Academy exchange, together with a seven-page statement of my proposed research project. That statement was written for IREX: I will condense it to the one-page description required by the NAS, but I thought a full description of what I plan to do would be more useful to you in writing the letter — the proposed project is the same, regardless of which exchange program I may go on. I do appreciate your

–2–

writing these letters. I realize such letters are probably something of a nuisance, and I am most grateful. In the near future, I will be sending you my application to the NSF[73] — so you may wish to save copies of your letter to NAS[74] in case you wish to use the same wording.

I should be in California from about December 18, 1975 to about January 3rd or 4th — so I will hope to see you before Christmas, as you suggested. I'll write you about more precise dates for a visit in a few weeks.

Again, many thanks.

Best regards,
Mark

[73] Сокращение от National Science Foundation.
[74] Сокращение от National Academy of Sciences.

IREX
Research Proposal

Mark Boyer Adams
USSR/ACLS-Academy

Studies in the History of Russian Biology 1870–1930

The modern evolutionary synthesis which emerged 1920–1945 owed much to the work of Soviet biologists. Perhaps their main contribution was the creation of experimental population genetics, the "hub" of the synthesis and a field developed almost totally by Soviets during the first decade and a half (1925–1940) of its existence.

Sergei Chetverikov's 1926 theoretical paper stands together with those of R.A. Fisher, S. Wright, and J.B.S. Haldane as the earliest which integrates Darwinian natural selection theory and Mendelian genetics into a single coherent view of the evolutionary process — this after two decades of theoretical antagonism between experimental geneticists and naturalists. The term "gene pool" (genofond) was first used in its modern sense by another Soviet geneticist, A.S. Serebrovsky, in 1925. "Genetic drift", sometimes known in the West as the "Sewall Wright effect", was independently discovered by D.D. Romashov and N.P. Dubinin in 1930. Finally, the first genetic analysis of natural Drosophila populations were undertaken in 1925–1926 by a number of younger workers, including N.V. Timofeev-Resovsky. In two studies, I have explored the nature of the contributions of Chetverikov and his group.

The remarkable Soviet contributions in the 1920s to genetics and evolutionary theory can be chronicled and documented, but how are they to be explained? The question becomes more difficult when we realize that these contributions emerged from a country which had just undergone a World War, two revolutions, and a bloody civil war. Considering the fact that Russia had no "tradition" in genetics — the first Russian university course in the subject opened on the eve of the war, in 1913 — how could the Soviet Union develop one of the world's most active and productive centers of genetics research in the 1920s?

Answering this question will involve three interrelated levels of analysis: (1) the intellectual history of Chetverikov, Serebrovsky, Filipchenko, and their co-workers (ca. 1905–1929); (2) the institutional history of the Institute of Experimental Biology and related institutions (1916–1930); (3) a prosopographical and statistical analysis of the growth of the Russian biological community (1870–1930). I have already undertaken research on these three levels: a year in the Soviet Union would allow me to gain information necessary to complete the evolving picture.

Intellectual History. Recent Soviet publications suggest that experimental population genetics emerged out of the Chetverikov laboratory at Kol'tsov's Institute of Experimental Biology (Moscow) in the mid-1920s. H.J. Muller's visit in 1922 marks the beginning of laboratory Drosophila work; genetic analyses of natural populations begin in 1925; Chetverikov's theoretical paper appears in 1926; finally, his discussion group (Dros-so-or) on "Evolution and Genetics" meets from 1924 through 1929, when the group disperses. As yet, however, the genesis and development of Chetverikov's evolutionary views are not clear.

An examination of Chetverikov's lecture notes for his courses on Entomology, Theoretical Systematics, and Genetics might clarify the development of his views on natural variation, species, natural selection, and the variety of evolutionary theories and mechanisms currently in vogue. An examination of his other papers and those related to his laboratory might clarify the importance of foreign work and discussions with colleagues, as well as permitting us to watch the development of his 1926 ideas through various drafts. A number of the members of the Chetverikov group are still alive: interviews with P.F. Rokitsky and N.V. Timofeev-Resovsky might help to clarify which papers were discussed in the laboratory and give us some sense of how the group's ideas evolved.

One of the characteristics which distinguish Chetverikov from other contemporary Soviet and Western geneticists was his background: trained as a butterfly taxonomist, he came relatively late to genetics both in terms of his career (he was in his forties) and the development of genetics

(20 years after the re-discovery of Mendel). Mayr and Dobzhansky have suggested that his naturalist background played a key role in his synthetic view. To clarify the significance of Chetverikov's naturalist background, it will be useful to compare him with lurii A. Filipchenko, Russia's leading geneticist in the 1920s, who had been teaching a genetics course at Petersburg (later Leningrad) University since 1913. Filipchenko had the experimentalist background more usual in geneticists, and his views on evolution up until his death in 1930 ranged from agnostic to orthogenic. By examining Filipchenko's papers in the Archives of the Academy of Sciences (Leningrad), I hope to gain some perspective on the ways he perceived the relationship between genetics and evolution and on his reactions to the work of the Chetverikov group.

Finally, Aleksandr Serebrovsky's conceptualization of the gene pool (genofond) in 1925 and his manifold studies of poultry populations would suggest that he was yet a second and highly original Soviet creator of population genetics. On the other hand, we know that he was a colleague of Chetverikov and participated in his discussion group. Did his ideas pre-date this exposure, develop conjointly with it, or did he get them from Chetverikov? Again, an examination of the papers of Serebrovsky (1916–1930) would help to clarify the development of his ideas.

Institutional History. All of the Soviet founders of experimental population genetics worked in the 1920s at Kol'tsov's Institute of Experimental Biology and its affiliated research stations. Yet Kol'tsov himself wrote almost nothing on evolutionary questions: his background was in experimental morphology and he is remembered for early suggestive papers (1928, 1930) on the chemical structure of the gene. Like many who shared his experiences at the Naples Station, he viewed traditional branches of biology as "unscientific" in comparison with new experimental approaches. While this group did much to develop cytology, embryology, and genetics, they tended to be leading "non-adherents" of evolution by natural selection, in contrast to naturalists for whom evolutionary questions were of primary concern. Why then would Kol'tsov pick a butterfly taxonomist to head a genetics division of his experimental institute?

I suspect that the answer may have to do with his conception of the organization of research, and with the potential he saw for "population genetics" to become an "experimentalist" approach to evolutionary questions. Like other scientific research institutions of the 1920s (e. g. Ioffe's Physico-Technical Institute in Leningrad, or Cal Tech), Kol'tsov's was built around an "experimentalist", highly interdisciplinary conception of research: its various divisions studied physico-chemical biology, developmental mechanics, ecology, animal behavior, and the genetics of Drosophila, poultry, cattle, guinea pigs, and man. An examination of Kol'tsov's papers would help to clarify the evolution of his conception of biological research and its effective organization; the models, if any, which he used in planning the institute; and his administrative style — the degree and nature of his personal involvement in its lines of research.

While Kol'tsov saw the primary task of his institute as "pure research", he also understood its important practical implications for agriculture and medicine. His views on the social utility of such biological research, and his public articulation of them, may help to explain the extraordinary material support provided by a number of government ministries, notably NARKOMZDRAV. A study of the administration of the institute 1917–1930 would help to clarify the factors which came together to create one of the world's most remarkable biological research environments in the 1920s.

The Development of the Russian Biological Community. Many of those who were active in Soviet biology of the 1920s had studied before the Revolution, a number before the turn-of-the-century. The post-revolutionary developments clearly have roots in Russian traditions of evolutionary and experimental biology which extend back to the 1860s. In an earlier unpublished study, I have demonstrated that unlike other disciplines and forms of publication, the number of Russian scientific and technical periodicals grew exponentially from roughly 1870 through at least 1930. This "exponential take-off" corresponds with large increases in those receiving academic train-

ing in the sciences in Russia, with the formation of increasing numbers of learned societies and popular science journals.

The biological sciences were an integral part of this pattern of growth. While in earlier decades Russia's leading biologists were generally "foreign imports" (e. g. Roulier or Karl Ernst von Baer), the last third of the 19th century produced a host of "home grown" talent (e. g. V.O. Kovalevsky, A.O. Kovalevsky, I.I. Mechnikov, I.M. Sechenov, K.A. Timiriazev, K. Kessler, M.A. Menzbier, N.A. Severtsov, I.P. Pavlov). In order to understand the intellectual traditions in Russian biology, it is important to realize that the beginnings of its rapid development are concurrent with the introduction into Russia of both Darwinism and German reductionist physiology (1860s). Recent studies have made clear that the Russian reception of Darwinism was perhaps the most positive in any country. Thanks to recent works by A. Vucinich and J.A. Rogers, we are beginning to get some sense of the forms of Russian biology in the late 19th century, but thus far our knowledge is limited primarily to intellectual biographies of only the most outstanding figures. What we most need now is some sense of the development of the Russian biological community and its activities as a whole.

While in the Soviet Union, I hope to continue to collect data for a collective biography of Russian biologists which I began some five years ago, concentrating on disciplines most relevant to evolution and genetics. By collecting and comparing data on all Russians trained in the biological sciences (at home and abroad) 1860–1917, we may clarify characteristics of the group as a whole: class origins and recruitment patterns; sources of European scientific influence; institutional loci of training and research; subsequent publication and employment patterns; the development of research orientations, specialization and discipline formation; the development of journals, learned societies, and other forms of professional activity; and norms of methodology and explanation. Such data will give us insight into the factors whose interaction brought about Russia's scientific "take-off" and will provide a basis for comparison with other periods and countries. In addition, by clarifying the macrostructure of Russian biological activity during the period, we will be better able to evaluate its microstructure.

Let me give three examples. We know that the de-emphasis of Darwin's views on intraspecific competition in favor of "cooperation" was not limited to Russian social thinkers and popularizers: it was articulated in 1869 by Karl Kessler, distinguished ichthyologist and Rector of St. Petersburg University, and would be picked up by Petr Kropotkin (1908) — and T. D. Lysenko (1948). How widely was this view held among late 19th century biologists, and how did it influence their thinking and research? The growth of the Russian biological community was reflected not only in its Darwinian orientation: it would produce two of the more impressive non-Darwinian evolutionary theorists — Korzhinskii (1899) and Leo Berg (1920). How many variant views of evolution existed, apart from or under the Darwinian orientation and what was their following and disciplinary orientation?

Finally, Kol'tsov was to argue in the period 1910–1930 that there had been very little "experimental" biological work in Russia, aside from his institute: was his an accurate description of the Russian biological establishment, or a programmatic statement designed to garner support for his enterprise? Such questions can only be answered on the basis of a systematic study of the Russian biological community as a whole, 1870–1920.

Lest the research project outlined above appear too broad to be do-able, I should point out what has perhaps already become obvious: I have completed considerable research on all phases of the project. My past and forthcoming publications all touch on aspects of It. Recently, my researches have been greatly inhanced by Theodosius Dobzhansky's generous gift of his library of Russian biological books and periodicals, together with access to his Russian correspondence from the 1920s. Even so, there are many gaps, and I hesitate to call such research finished until I have explored the accessibility of Soviet archival materials on Kol'tsov, Filipchenko, Serebrovsky, Chetverikov, and the Institute of Experimental Biology. The Lenin Library will have most of the materials necessary for the prosopographical study: I checked this out on my trip to Moscow in 1971.

Thus, I am prepared to have a productive research trip, the exact character of which will inevitably depend on the availability of archival materials.

USSR/ACLS-Academy

SPECIFIC RESEARCH INFORMATION

Primary Institutional Affiliation:

Institut istorii estestvoznaniia i tekhniki, AN SSSR
(Director, S.R. Mikulinsky)

Moscow:

Sector on the History of the Biosciences (L.J. Bliakher) and other sectors concerned with history of Russian science and naukovedenie. (6 months)

Leningrad:

Sector on History and Theory of Evolutionary Concepts (K.M. Zavadsky) and others which concern institutional history and naukovedenie. (4 months)

Secondary Affiliations (introductions and access):

Vsesoiuznoe obshchestvo genetikov i selektsionerov, AN SSSR.
Institut biologii razvitiia, ANSSSR (esp. A.I. Gaisinovich)
Institut obshchei genetiki, ANSSSR (N.P. Dubinin)
Institut tsitologii i genetiki, Sib. otd. (SO) AN SSSR (D.K. Beliaev) (1 month)
Moskovskoe obshchestvo ispytatelei prirody.
G.M. Dobrov's institute, Kiev (AN Ukr. SSR) (1 month)
Institut molekuliar'noi biologii, ANSSSR (V.A. Engel'gardt)

Interviews:

N.V. Timofeev-Resovsky	A.I. Oparin	V.V. Sakharov
N.N. Medvedev	A.S. Spirin	V. Babkov
A.A. Kanaev	G.M. Dobrov	A.A. Malinovsky
P.F. Rokitsky	A.I. Gaisinovich	V.P. Efroimson
V.A. Engel'gardt	E.S. Smirnov	
N.N. Semenov	V. Polynin	
P.L. Kapitsa	N.P. Dubinin	

Libraries and Archives:

Archives of the Academy of Sciences, Leningrad
Lenin Library
Archives of the Moscow Society of Naturalists
Archives of the Congresses of Naturalists & Physicians
Archives of Moscow University
Archives of the Ministry of Public Health

Requested Visits:

I am requesting 6 months in Moscow, 4 months in Leningrad. In addition, I would like to visit the Akademgorodok (see above), Dobrov s Institute in Kiev (see above), and spend a couple of weeks at the Science City in Pushchino.

USSR/ACLS-Academy

On Previous Contacts:

While on a two-month trip to the Soviet Union (August–October, 1971) I attended the International Congress of the History of Science held that year in Moscow. Since I was one of the few Westerners there who knew Russian, I became acquainted with most of the Soviet historians of science in my field. In addition, I was able to interview a number of geneticists and other scientists (e.g. N.P. Dubinin, A.I. Oparin, B.L. Astaurov). I was also able to renew previous contacts made when I assisted I.B. Cohen in running a joint Soviet-American conference on the history of science, sponsored by the American Academy of Arts and Sciences in the late 1960s.

During my six weeks in Moscow, I spent a good deal of time with members of the Sector on the History of the Biosciences of IIET and got to know 10 or 15 fairly well. In addition, I had the pleasure of meeting A.I. Gaisinovich and speaking with him on several occasions. While I was in Leningrad, I was invited to give an impromptu lecture before the Sector on the History and Theory of Evolutionary Concepts. I also was able to spend considerable time with 6 or 7 members of the Sector. These exposures no doubt led to the invitation to write an article for the Soviet journal Iz istorii biologii, which unfortunately I was unable to accept at the time for personal reasons.

As a result of these contacts, I frequently receive off-prints and cards from the Soviet Union. B.L. Astaurov, perhaps my closest contact, has unfortunately passed away (May, 1974). He was an Academician, Director of the Institute of Developmental Biology, and President of the Vavilov All-Union Society of Geneticists and Selectionists. His presidential address before its second session had the following kind words: in discussing how little of Soviet genetics work has received historical study in the West, he commented:

> …it remains a fact that the published translation, and recognition of the pioneering significance, of S.S. Chetverikov's 1926 work on evolutionary genetics and the explication of the work of Chetverikov's school in the special historical investigations of Adams (1968, 1970) constitute the single, gratifying exception /edinichnym i otradnym iskliucheniem/.
>
> (B.L. Astaurov, "Genetika i problemy individual'nogo razvitiia", Ontogenez, Vol. 3 (1972), № 6, p. 556.)

My work has been referred to elsewhere in Soviet publications, e.g. by N.G. Rubailova ("Novyi zhurnal po istorii biologii", Iz istorii biologii, № 3, Moscow: Nauka, 1971) and K.M. Zavadsky: (Razvitie evoliutsionnoi teorii posle Darvina 1859–1920-e gody, Leningrad: Nauka, 1973).

Thus I have extensive contacts within the Academy, and in particular with the Institute of the History of Science and Technology, where I am requesting primary affiliation.

IREX 1, p. l
Addendum.

Mark Boyer Adams
USSR/ACLS-Academy

11. Travel abroad: date, duration, and purpose of previous travel or residence in in any foreign country.

Date	Country	Purpose	Duration
1964	USSR	Summer Language Study Program University of Michigan (July/August)	3 weeks
1968	Ecuador Chile Argentina Brazil	Retracing Darwin's voyage; making film and film loops; research on Darwin's travels in South America.	4½ months (May/September)
1970	Israel	Presenting paper at Van Leer Conference on Science and Values, Jerusalem.	2 weeks (September)

1971	USSR	Attendance at International Congress on the History of Science and Technology. Library research, interviews	2 months (August/October)
		(tourist status; NSF support).	
	England	Presented paper at Van Leer Conference, Cambridge University.	1 week (September)

CURRICULUM VITAE: Mark Boyer Adams

Current status:

Julian S. and Janice C. Bers Assistant Professor of Social Studies (1973–197)
Undergraduate Chairman, Department of the History and Sociology of Science, Faculty of Arts and Sciences, University of Pennsylvania

Degrees:

Ph.D.	Harvard University 1973	History of Science
A.M.	Harvard University 1969	History of Science
A.B. Magna cum laude	Harvard College 1966	History and Science

Previous Appointments:

Lecturer in History and Sociology of Science, University of Pennsylvania (1970–1973)

Teaching Experience:

H&SS. 201.	"Biology and society" Taught: Spring 1971, 72, 73, 74 Enrollments: 30, 40, 50, 60
H&SS 210.	"Science and Literature" Taught: Fall 1970, Spring 1971, Fall 1972, 73, 74 Enrollments: 20, 35, 45, 50, 64
CTS	"Science Fiction" Taught: Spring 1972 Enrollment: 15
H&SS 510.	"Seminar in Science and Literature" Taught: Fall 1972, Spring 1974 Enrollments: 10, 5
H&SS 535.	"Biology in the Last 100 Years" Taught: Spring 1972, 73, 74 Enrollments: 3, 12, 12
H&SS 560.	"Science in Russian Culture" Taught: Fall 1970, Fall 1974 Enrollments: 10, 7
H&SS 570.	"Nature and Nurture" Taught: Fall 1973 Enrollment: 15

Direction of Undergraduate Senior Theses:
1974. Daniel Todes, "Vladimir Onufrevich Kovalevskii" (195 pp.)
1974. Joseph Maline, "Edward Drinker Cope" (140 pp.)

Doctoral Student Supervision:
Molly Noonan, "Psychic Research and Professional Psychology in the 19th Century". (January, 1976)

Administrative and Related Experience Departmental:
Departmental:
Undergraduate Chairman (1972–)

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Administrative and Related Experience — Continued
University:
Member, Student Affairs Committee (1973–74)
Member, Provost's Committee on Human Biology Major; member of its two working subcommittees.

Professional:
Chairman and Organizer, 10th Annual Joint Atlantic Seminar on the History of Biology (70 participants, held at University of Pennsylvania, March 15–16, 1974).

Residential:
Founder, Van Pelt College House (1971)
Resident Faculty, Van Pelt College House (1971–)
Chairman, Master Search Committee, Van Pelt College House (1974)
Variously in charge of: Guest Suite; Speakers Program; Sherry Hours; Student and Staff Admissions.

Research and Scholarly Activity:
Books:
Genetics and the Soviet Scientific Community (tentative title)
Accepted for publication by Chicago University Press; to be published 1976.

Articles:
"Towards a Synthesis: Population Concepts in Russian Biological Thought, 1925–1935", Journal of the History of Biology, Vol. 3, No. 1, 1970.
"The Founding of Population Genetics: Contributions of the Chetverikov School, 1924–1934", Journal of the History of Biology, Vol. 1, No. 1, 1968.
"Aleksandr Onufrevich Kovalevski", Dictionary of Scientific Biography, Vol. VII, pp. 474–477.

Book Reviews:
"David Joravsky, The Lysenko Affair", Isis, Vol. 62, No. U, 1971.

"Alexander Vucinich, Science in Russian Culture, 1861–1917", Canadian Historical Review, Vol. 53, № 3, 1972.

Invited Lectures, Conference Papers

"The Founding of the Akademgorodok: A Case Study in the Interaction of Soviet Science and Politics" (December, 1974) (L'Institut d'histoire et sociopolitiques des sciences, Montreal).

"What Can Be Learned by Studying Science Fiction Historically?" (December, 1974) (L'Institut d'histoire et sociopolitiques des sciences, Montreal).

"Severtsov, Schmalhausen, and Russian Evolutionary Morphology" (October, 1974) (American Academy of Arts and Sciences, Second Workshop revolutionary Synthesis).

Invited Lectures, Conference Papers — Continued

"Russian Population Genetics and the Synthetic Theory of Evolution" (May, 1974). American Academy of Arts and Sciences, First Workshop on the Evolutionary Synthesis.

"Developments in 20th Century Soviet Biochemistry" (October, 1973) (American Academy of Arts and Sciences, Conference on the History of Bioenergetics).

"Evolution Theory and Its Social Implications: A Historical Survey" (September, 1973) (American Academy of Arts and Sciences, Conference on Biology and Social Theory).

"An Evolutionary Model for the History of Science" (1973) (Department of the History of Science, Harvard University).

"The Rise of Soviet Molecular Genetics" (1973) (University of Minnesota).

"Lysenko and the Emergence of Soviet Molecular Biology" (1972) (American Association for the Advancement of Science, Conference on the History of Genetics).

"Vazhnost' i znachenie sovetskikh issledovanii v populatsionnoi genetike" (September 1971) (Leningrad Division, Institute of the History Science and Technology, Soviet Academy of Sciences).

"Dialectical Materialism and Soviet Biology", (August, 1971) (Van Leer Foundation. Conference on Science and Values, Cambridge university, U.K.).

№ 12

November 25, 1975, Philadelphia

UNIVERSITY OF PENNSYLVANIA
PHILADELPHIA 19174

Department of History and
Sociology of Science
Edgar Fahs Smith Hall D 6
215–594–8400

November 25, 1975

Dear Doby:

Many thanks for the recommendation to the NAS[75] — I do appreciate it, and your kind comments about my description. "Prosopography" is a term meaning "collective biography of a 'natural' group of people", e. g. members of a society or profession, in order to find common characteristics and degree of diversity in e. g. class origins, education, social or political views or connections, etc.

[75] Сокращение от National Academy of Sciences.

The papers of Serebrovsky, Filipchenko, Kolt'sov are preserved in the Архивы АН СССР and elsewhere — there are published lists. As to Chetverikov, no published фонд number has appeared, but Астауров[76] in his various works refers to copies of things in the Архивы АН. How "private" they are, how illuminating, how much they say — answers will have to await inspection! I am fairly sure that I won't be allowed to see anything that is political post-1917.

I am sorry to hear about your health. Frankly, I am not sure my[77] health is up to a Mexican trip, it must be a disappointment — but at least your pocket won't be picked at the airport! I will be arriving in Tracy on roughly December 17, 1975 and am returning here January 3, 1976. It will be good to see you again! Aside from the Holidays proper (December 24, 25, January 1) I will be at your disposal as to time! I thought maybe 4 days would be nice if it weren't too inconvenient.

Hopefully by then I will have finished a draft of a couple of articles, which I would be delighted to get your reactions to: one on Северцов and Шмальгаузен[78], another on the Kol'tsov Institute[79] — based in part on your materials (for which I am needless-to-say most grateful!) I've contacted my friend Dr. Whitfield Bell, Librarian of the APS[80] — he should be writing to you. The plan at the moment is that I will help sort through your papers etc. with you, and Bell, who will be in San Francisco in February, will come up and make packing arrangements (he has more experience at sending & protecting valuable papers than anyone I know). I will also soon be sending you recommendations for NSF.

See you in a few weeks!
Mark

"I Was Delighted and Aghast at the Treasure Trove of Materials on Russian Genetics"

Mark Adams's letters to Th. Dobzhansky

(Publication, preface, and commentary by *Mikhail B. Konashev*[1] with the permission and assistance of *Mark B. Adams*)

[1] St. Petersburg branch of the Institute for History of Science and Technology named after S.I. Vavilov, Russian Academy of Sciences, Saint-Petersburg, Russia; mbkonashev@mail.ru

The publication includes Mark B. Adams's letters to Theodosius Dobzhansky regarding the history of Russian genetics and evolutionary biology. Mark B. Adams is a historian of biology, and Emeritus Associate Professor at the University of Pennsylvania, USA. Th. Dobzhansky (January 25, 1900 — December 18, 1975) was a prominent Russian-American geneticist, evolutionary biologist, thinker, and evolutionary humanist. He was one of the central figures in the field of evolutionary biology in the first half of the XXth century, and played a key role in the "evolutionary synthesis" of the 1930s-1940s and in shaping

[76] Борис Львович Астауров (1904–1974) — советский биолог, цитогенетик, эмбриолог-экспериментатор, академик АН СССР (1966).

[77] Подчеркнуто М. Адамсом двойной чертой.

[78] *Adams M.B.* Severtsov and Schmalhausen: Russian Morphology and the Evolutionary Synthesis The Evolutionary synthesis: perspectives on the unification of biology / E. Mayr, W.B. Provine (eds.) Cambridge, Mass.: Harvard University Press, 1980. P. 193–225.

[79] *Adams M.B.* Science, Ideology, and Structure: The Koltsov's Institute, 1900–1970 Social Contextt of Soviet Science / L. Lubrano, S. Gross Solomon (eds.) Boulder: Westview Press, 1980. P. 173–204.

[80] Сокращение от American Philosophical Society.

the “synthetic” or modern theory of evolution. Dobzhansky was born in a small town Nemirov in the South-West of the Russian Empire, went to Thomas Hunt Morgans’s lab in the United States in December, 1927, and in 1931 he decided not to return to the USSR. The main theme of the letters written in 1970–1975 is Adams’s meetings with Th. Dobzhansky, including several recorded interviews. The key goal of these interviews was to illuminate various aspects of the development of Russian genetics, mainly during the 1920s, including contributions and personalities of the most important Russian biologists, notably Yu.A. Filipchenko, N.K. Koltsov, S.S. Chetverikov, N.I. Vavilov and A.S. Serebrovsky, as well as Dobzhansky’s library and his relations with Russian colleagues during the time of Dobzhansky’s life and research in the Soviet Union. A letter, dated November 11, 1975, also includes Adams’s Research Proposal “Studies in the History of Russian Biology 1870–1930”, Specific Research Information, Addendum, and Curriculum Vitae for his application for an IREX grant.

Keywords: Russian (Soviet) genetics, Mark B. Adams, Th. Dobzhansky, “synthetic theory of evolution”, evolutionary genetics, history of science.