



Hunt Institute for Botanical Documentation  
5th Floor, Hunt Library  
Carnegie Mellon University  
4909 Frew Street  
Pittsburgh, PA 15213-3890  
Telephone: 412-268-2434  
Email: [huntinst@andrew.cmu.edu](mailto:huntinst@andrew.cmu.edu)  
Web site: [www.huntbotanical.org](http://www.huntbotanical.org)

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*About the Institute*

The Hunt Institute for Botanical Documentation, a research division of Carnegie Mellon University, specializes in the history of botany and all aspects of plant science and serves the international scientific community through research and documentation. To this end, the Institute acquires and maintains authoritative collections of books, plant images, manuscripts, portraits and data files, and provides publications and other modes of information service. The Institute meets the reference needs of botanists, biologists, historians, conservationists, librarians, bibliographers and the public at large, especially those concerned with any aspect of the North American flora.

Hunt Institute was dedicated in 1961 as the Rachel McMasters Miller Hunt Botanical Library, an international center for bibliographical research and service in the interests of botany and horticulture, as well as a center for the study of all aspects of the history of the plant sciences. By 1971 the Library's activities had so diversified that the name was changed to Hunt Institute for Botanical Documentation. Growth in collections and research projects led to the establishment of four programmatic departments: Archives, Art, Bibliography and the Library.

Dr. Milorad Hladjevović  
Sr.: Scientific Attache

Dear Dr. Sušnik

Dr. Schmertz, Director of Office of International Activities in Smithsonian asked me to transmit to you an information of a very delicate nature.

The matter in question is the project "Cytotaxonomy of Yugoslave Flora". One year's work of Dr. Löve on this project has been evaluated with a bad, insufficient recommendation. Therefore the Smithsonian has decided to finance this second year in the same amount as he did it the first one /40.-50.000 dollars/. This would be also the last year for Dr. Löve, because they will not agree with Dr. Löve continuing this work. But Dr. Löve is known as a very persistent man, so they expect that his removal will not pass without any difficulty.

They have no remarks on your work. Considering that the problems are interesting, they would like to continue this work with another, a better american scientist. You can think over about another american scientist, who would be capable for this work, and then, after the problem with Dr. Löve has been solved, continue the work with him.

DR ASKELL LOVE  
DR DORIS LOVE  
473 HARVARD LANE  
BOULDER CO 80303

Account Number 254 679  
Date This Statement SEP 30 75  
Beginning Balance \$20,155.38  
Interest Pd. This Year \$305.05

522 68 5805

1300 Walnut Street, Boulder, Colorado 80302



**United Bank  
of Boulder**

**Savings  
Statement**

| Withdrawals   | Deposits | Date  | New Balance |
|---------------|----------|-------|-------------|
| 600.00        |          | 07-07 | 19,555.38   |
| 500.00        |          | 08-05 | 19,055.38   |
| 1,000.00      |          | 08-11 | 18,055.38   |
| 500.00        |          | 09-08 | 17,555.38   |
| INTEREST PAID | 234.46   | 09-30 | 17,789.84   |

1392.02  
 546.44  
 400  
 -----  
 2338.46

~~800~~  
~~1500~~  
 2800

Fair copy — original  
 utje sör kulturen skildras, y fair  
 i sin kort på papper

~~4547.56~~ (inledning, Anslut — )  
~~360.00~~

5947.56 Eldes barn ett med, till  
 milteln utom med (i sin Högskolan  
 (7. stadi, p.)

Try to present that cultural depth speaks to  
 not enough internal field of American history.  
 (cf. deen & forward —).

För i sin Högskolan för rättlära

| Date | Balance from Statement |
|------|------------------------|
|      | Withdrawals            |
|      | Deposits               |
|      | Balance                |
| Date | Balance Forward        |
|      | Withdrawals            |
|      | Deposits               |
|      | Balance                |

My colleagues sent me ~~the~~ a letter of appreciation (exhibit 2) when I resigned my duties as a chairman, and I understood that twice they proposed me for a special honor as the annual university lecturer; ~~but~~ I have seen a copy of their letter evaluation, in the spring 1973, which was flattering but hardly untrue, and I am sure it was also sincere. Of course, ~~this~~ this was categorically refused by the theologian-philosophy and his helpdesk-committee.

This was, in fact, a theology department with a tokenology setting, and its members were struggling to do research with ~~no~~ facilities so primitive that the high schools in the smallest towns in Colorado were better off. And the incompetent administration ignored all my efforts to ~~improve this, with all the little middle school or high school or college or university~~ improve this, with all the little middle school or high school or college or university.

~~inside honor,~~  
for a special lectureship with a ~~very~~ <sup>rather</sup> ~~long~~ <sup>short</sup> ~~title~~ <sup>title</sup> ~~recognition~~ <sup>recognition</sup> that I like to believe was sincere; I know that it only ~~came~~ after the fact in the spring of 1973, when a copy was left in my mailbox; this, however, was, naturally, presented by the provost and his helpdesk committee, the usual ~~disregard~~ <sup>disregard</sup> of justice, I was just an immigrant lecturer - uncomfortable by shadow for this environment. That one spring my success as a chairman with a similar story but ~~the~~ <sup>the</sup> review on my faculty report (exhibit 2) that I am sure was also sincere. This one ~~was~~ <sup>was</sup> officer, the dean, came out so far since in 1968 -

Wife - of her rights  
Dill-up rights,  
Stubs,  
Erasing,  
Entrapment.  
Deja-vu  
by history, ~~1971~~  
1971  
Revolving  
National Foundation  
P. to ~~1971~~  
Nogus

No harm, hurt, Accusations without  
investigation of facts  
About human rights  
Great just list, great American  
Witch-hunt - reason? Study - impact  
Always, follow conviction (with law)  
(Special Service, Vopar, over ~~1971~~, ~~1971~~)  
(1949: Cain & Ad. ~~1971~~) | patronage,  
| ~~1971~~

3  
1971

Highly galled - outraged  
Dream - nightmare. Trickery and deceit.

My rational, though logical, approach  
to taking my had earned me the  
hostility of my more conservative colleagues,  
though I thought they would be as tolerant  
as I am and at least not taking backhand attacks

Clearly tried to ~~1971~~ as us scientists,  
- & then who ought to rest sound children,  
the reason of ~~1971~~ etc.  
Actively not being moral man - see had to ~~1971~~

For example:

When I met Billie Tamm last time  
in Boulder in 1973, he put his  
arm around my shoulders &  
told me not to worry, because  
I had my friends who would  
do anything for me. That was  
the last I heard from him -  
intimidation

I have never fought back  
for myself, not even as a little boy,  
though I have fought furiously for  
others - but they never fight for me.  
So I am easily treated badly by  
those who have another moral.

Human rights of scientists would be:  
with post-graduate requirements and reports -  
or the ~~1971~~ report ~~1971~~  
with expensive ~~1971~~ for re-entry -  
required of writing outside that then both  
US passport can visit free  
with requirement of ~~1971~~ when leaving re-entry

Ought to have realized but did not want to believe that we were moving into a police state filled with judges & even worse traitors, & with very small minds ~~of~~ leading the bit-its & it-its.

Bill Stease said nothing smiles, but when I asked him for help, he told me that he could not do anything because he was afraid of hurting nobody - hardly me!

Love & respect  
We know that there are nice people in America, although most of those we have encountered are either beasts or children, mainly children.

The country of those who do not want to believe involved - children  
As migrants, we protest against violence - have rights here - since they would want to muddle in there instead of going because it is such a small land.

Law agnostic at the time then as I - deeply Christian in my morals - that made my disapproval in American politics still greater because I had believed its religious talk without realizing that it is ~~really~~ a land of hypocrites - except for Luther etc.

Due process & fair revision replaced by a kangaroo court, at the U.S. (No. NSF, Smithsonian).

Two the best quality and best scientists ever to immigrate to the U.S.

Observed me into going with a group the second time, believing that they had sent the letter to Smith; then found me to show a law on exhibition recalled by the without witnesses, and requested my papers - kind of law of the risk party, otherwise longer - registered as undesirable alien.

Dialogues - The Arctic

Dialogues on evolution at (1970)



**BARTOLOMEO VANZETTI, NICOLA SACCO . . . (second and third from right) leave courtroom in undated file photo**

— AP photo

## When U.S. justice lost to prejudice

BOSTON (UPI) — Nicola Sacco and Bartolomeo Vanzetti, protesting their innocence to the end, died 50 years ago today in a dreary prison room surrounded by those they regarded as their persecutors.

The memory of their futile seven-year struggle against murder charges and the electric chair would not go away, and today Massachusetts honored their memory — at least officially.

It is Sacco and Vanzetti Day, the result of 50 years of agitation over what has become one of the most celebrated legal cases in the world.

Gov. Michael Dukakis issued a

proclamation last month declaring the two Italian immigrants were denied a fair trial because of the anti-alien hysteria that swept the country in the 1920s.

He asked that people set aside time today to reflect upon "our susceptibility to prejudice, our intolerance of unorthodox ideas and our failure to defend the rights of persons who are looked upon as strangers in our midst."

There is much to consider.

Evidence recently released indicates the prosecution, headed by Dist. Atty. Frederick Katzmann, may have fabricated evidence against

Sacco and Vanzetti. It is only the latest in a series of questions revolving around the conduct of the prosecution, the police and the trial judge.

Researcher Lincoln Robbins said notes by Assistant Norfolk County Dist. Atty. Harold Williams indicate only three bullets were found at the scene of the crime. However, the prosecution showed jurors four bullets and said one of them was fired from Sacco's gun.

In addition, trial Judge Webster Thayer is reported to have made many disparaging comments about Sacco and Vanzetti and their counsel during the trial.



Boulder Courier - 7/8/76

## CIA Won't Release Information On Riha

The CIA apparently intends to stand firm on its decision not to release secret information on the whereabouts of Thomas Riha, the University of Colorado professor who disappeared from Boulder more than seven years ago.

In a letter made public Thursday, the spy agency declined once again to release the names of sources who claim they saw Riha in Czechoslovakia in 1973. Riha disappeared in 1969.

The attorney for the Riha estate, Martin Buckley, has been attempting to obtain the names since April. They were deleted from censored CIA documents furnished Buckley in March.

In April, Buckley appealed the deletions to the CIA's Information Review Committee under the Freedom of Information Act. The committee was to respond by mid-June. When it failed to do so, Buckley, a Denver lawyer representing Zdenek Cerveny, Riha's nephew, filed a lawsuit against the CIA to force

disclosure. The suit is still pending.

Buckley finally received the belated response from the committee this week. It came in the form of a letter for John F. Blake, committee chairman.

According to Buckley, Blake said names and other deleted information cannot be released because they are classified.

Blake said disclosure of the information "would constitute a clearly unwarranted invasion of the personal privacy of other individuals."

One source has it that the person who sighted Riha was an intelligence agent whose life would be endangered were his name revealed.

The CIA told a senate committee last year that Riha is alive and living in Czechoslovakia, but denied that he had any connection to that agency.

Friends and relatives of the missing history professor say they have not heard from Riha since he disappeared.

Buckley said he needs the CIA information in order to settle the Riha estate.

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### Nation



**TWO IMMIGRANTS** who staged an unsuccessful legal battle to avoid the electric chair in the 1920s were honored today by the State of Massachusetts. Page 10A

Part I: Introductory

Manifestation of false evidence  
appearing real.

incomplete evidence appearing perfect.

85 ~~85~~ ~~85~~ 50  
~~85~~ ~~85~~ 90

Dr. Challinor 1/10



SMITHSONIAN INSTITUTION

Washington, D.C. 20560  
U.S.A.

Please wait until you have been informed of the availability of funds to complete these instructions.

Mr. Thurston E. Manning  
Vice President for Academic Affairs  
University of Colorado  
Boulder, Colorado 80302

Dear Mr. Manning:

Congratulations on your Smithsonian Foreign Currency grant.\* The grant will be formalized by the conclusion of a grant contract between the Smithsonian Institution and the University of Colorado.

Included as integral parts of this grant contract are the research proposal which describes the research and is accompanied by an official project budget; and the enclosed document "Administration of Smithsonian Institution Foreign Currency Grants." As an aid in your planning to administer project funds in the "excess" currency country, there is also attached a document entitled "Suggested Guidelines to Accounting for Smithsonian Foreign Currency Grants." Your careful attention is invited to these two documents, since very special conditions prevail in many of the "excess currency" countries and many special problems arise when carrying out research in them with local currencies.

If your project will be a joint project in collaboration with a foreign research institution, a model Research Project Agreement, which may need to be altered to fit the special conditions of your project, is also attached. The Smithsonian Foreign Currency Program will be happy to assist and advise you in concluding a project agreement with your collaborating institution abroad. This should be done as soon as possible, if you have not taken any steps in this direction.

\* "Cooperative Studies on the Cytotaxonomy of the Yugoslavian Flora"

Please let us know the date you would like your project to begin. In order to make grant funds available to you as soon as possible by the preparation of the grant contract covering your project, we need some additional information from you. First of all, though yours may be a multi-year project, funds are normally provided for only one year at a time, and contracts are only written for a year at a time. Hence, your records should be kept for a year at a time. Exceptions are made only when the amount of "excess" U.S. -owned funds in the country where you will be working is low enough to warrant obligating funds for subsequent years by a firm contract.

Secondly, you should know that the grant contract, as explained in the enclosures, will name a Principal Investigator who acts as the agent of the grantee institution in the "excess" currency country. The research is under his direction. If grant funds are to be received and accounted for by someone other than the Principal Investigator, please give us the name of this "Designated Representative." The name of the Principal Investigator and/or the Designated Representative should be the full name of the individual as shown in a passport or other appropriate identification. Grant funds will be disbursed by the Disbursing Officer at the American Embassy in the country where the research is to be carried out; (or occasionally at the American Consulate nearest your research base); this officer will request proper identification before disbursing any grant funds to the Designated Representative.

Thirdly, please tell us what schedule of payments makes the most sense for your work. The grant contract, and the subsequent authorization to disburse which the Smithsonian will send to the American Embassy, should be based on the Principal Investigator's best estimate of his needs. The first payment will normally be an advance payment adequate to cover the costs of initiating the research. This should have been specified in the budget you submitted with your proposal. Subsequent disbursements will be made according to a schedule authorized in advance by the Smithsonian. Most research projects fall into two categories for payments purposes. One covers short seasonal field research, such as a summer's field expedition; the other involves year-round research, perhaps based at a foreign institution or at an institutional base of your own institution in the host-country. If your research is seasonal, you may wish an advance of the entire grant amount; or a large advance and a smaller one later to cover close-out costs. On the other hand, if your work will be year-round,

semi-annual payments are the most common. In scheduling payment, you should make allowance for the fact that funds provided for international travel are, as a rule, withheld by the U. S. Embassy abroad which makes payment directly to the American carrier you use; international travel funds are accordingly not disbursed to the Designated Representative.

Scientific progress reports will be required under this grant; their frequency will be spelled out in your formal grant document. Scientific progress of multi-year grants are reviewed each year during one of our two review cycles and the release of funds for each subsequent year's work will be subject to the successful outcome of this review. For this reason progress reports, like renewal reports, should be submitted in twenty copies. Deadlines for review are October 1 and March 1 each year, and we can fix the deadline you prefer in your grant contract. Written requests for grant renewal are required; they should include twenty copies of your latest and most comprehensive scientific report--that is to say, the report which in the terms of the grant, provides the most persuasive case for renewal of the grant. If circumstances make inclusion of the latest report impractical, a statement summarizing progress should be included in the renewal proposal itself.

As nearly as possible the standard SEOP format should be followed in the renewal proposal, although the complete biographies and bibliographies of the key investigators need not be repeated in each renewal proposal unless they are joining the project for the first time. Budgets for succeeding years should reflect actual field experience of costs in a way that can be verified by reference to project financial reports. To the extent possible, renewal proposals should be self-contained, and should convey enough of the original research plan to permit reviewers to judge the merits of continuation of the research without necessarily going back to the original proposal.

With all these things in mind, then, your reports may be timed to coincide with the end of a field season or to coincide with reports due to another granting agency. Scholarly articles and other publications are welcome substitutes for reports prepared especially for the Smithsonian. Please let us know what scientific reporting schedule you would like incorporated in your grant.

Financial reports are also required, as explained in the enclosed document "Administration of Smithsonian Institution Foreign Currency Grants." Please consult this as well as the enclosed "Suggested Guidelines to Accounting for Smithsonian Foreign Currency Grants" for further information about the financial reports that will be required in your grant contract. Then let us know what financial reporting schedule would make sense for your research. Interim reports are generally required as of June 30 of each year.

Please inform us, also, what travel itinerary you desire, including dates and estimated lengths of stay in each place you plan to visit. Stopovers en route may be authorized if there is no additional cost on your ticket. The routing of at least the Principal Investigator under the grant must include a Stopover in Washington for briefing by the Foreign Currency Program Staff en route to the "excess" currency country where the work will be performed; no per diem in dollars can be paid for this Stopover by the Smithsonian.

As noted above, international travel funds will be withheld from your grant by the U. S. Embassy in the "excess" currency country. Grantees are asked to travel on Government Transportation Requests, or GTR's, in order to take advantage of provisions of intergovernmental Public Law 480 agreements dealing with official U. S. Government travel. Under these provisions, the carrier which you employ will bill the U. S. Embassy directly. To provide for this, we are enclosing Smithsonian Travel Authorization Request forms (SI-15's): one for each of the travelers specified in your budget. More forms can be sent to you if you need them. Once you have decided upon an itinerary, you should fill out as many SI-15's as necessary for each grant traveler in accordance with the attached sample form.

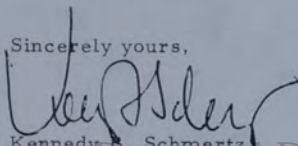
If you require further information before settling upon an itinerary, you may call the Smithsonian Travel Services Office, Area Code 202, 628-1810, Extension 5472. Identify yourself as a grantee of the Foreign Currency Program, and this office will try to provide assistance regarding itineraries, routes and so on. Do not use travel agencies when seeking information about possible itineraries since your ticket will be authorized directly from the American carrier, with no provision for paying a travel agency. You should accordingly seek information directly from the carrier you plan to use.

We are required, by U.S. Government regulations, by the way, to employ American carriers. Exceptions to this rule are outlined in Section 19 of "Administration of Smithsonian Institution Foreign Currency Grants."

When you have completed the SI-15's, they should be mailed to me for processing and approval. When approved, we will authorize the airline to issue a prepaid ticket advise to you. You will be contacted by the airline at the address and telephone number you indicate on the SI-15, unless you advise otherwise. At this point you may make your exact reservations with the airline, and arrange for picking up your ticket.

If you have any questions, feel free to get in touch with me or with Mrs. Betty Wingfield. We can both be reached on Area Code 202, Number NAB-1810. We look forward to hearing from you soon.

Sincerely yours,



Kennedy B. Schmertz  
Director  
Foreign Currency Program  
Office of International Activities

Enclosures: SI-15's (Travel Authorization Requests) - to Professor Love  
"Administration of Smithsonian Institution Foreign  
Currency Grants"  
"Guidelines to Accounting for Smithsonian Foreign  
Currency Grants"  
~~"Model Research Project Agreement"~~ (if project is to  
"Country Information Sheet - Yugoslavia" be a joint-  
cc: Principal Investigator (with attachments) collaborative  
Travel Service Office project)---

Dr. Askill Love

St.:

Dear Dr.Mladjenšvić

I thank you for the information in connection with the visit of Mr.H.B.Quine and Mr.T.A.Wastler. The information about the project "Cooperative Studies on the Cytotaxonomy of the Yugoslavian Flora" was especially important for me, because I am personally involved. A few days ago I got a letter from Dr.Löve /with a copy of the information from the Smithsonian Institution to the University of Boulder, where Dr.Löve is working as professor/, in which he is interpreting the new situation. I regret not to know the reasons, why the Smithsonian Institution has decided in such a way. I personally can guess then, so that this decision has not suprised me too much. The most difficulties are probably connected with his political views and the fact, that he is a foreigner in U.S.A. /a citizen of Iceland/.

If I sum up the first year of a work on this project, I can see the first - very interesting and promising results. The chromosome number is in fact the fundamental genetic information and therefore important so far the fundamental knowledge of the rich Flora in our country /Yugoslavia is for its geographical, geological, and ecological character the most interesting country in Europa/, as because of its interest for the applicative fields of work /pharmacology, agriculture, forestry, etc./. The first results are already published in scientific periodicals /6 papers/. Any interruption of this scientific work would entail a material, but an even greater moral damage. In the present situation we would not be able to realize the project with our financial resources only. But probably somebody could say some critics on our account too. Some time ago I wrote about it to Dr.Schmertz. I mentioned him that I will inform about the whole matter also you. As I have already said, our opinion is that the work on this project has to be continued. If we remain in the standards of scientific relations" it is unpossible to change one of the principal investigator, without explanation what is the reason. Do they mean ~~some~~ <sup>our</sup> unconfidence to our work too?

.....



Dr. Milorad Hladjevović  
Sr. Scientific Attache

Dear Dr. Sušnik

Dr. Schmertz, Director of Office of International Activities in Smithsonian asked me to transmit to you an information of a very delicate nature.

The matter in question is the project "Cytotaxonomy of Yugoslave Flora". One year's work of Dr. Löve on this project has been evaluated with a bad, insufficient recommendation. Therefore the Smithsonian has decided to finance this second year in the same amount as he did it the first one /40.-50.000 dollars/. This would be also the last year for Dr. Löve, because they will not agree with Dr. Löve continuing this work. But Dr. Löve is known as a very persistent man, so they expect that his removal will not pass without any difficulty.

They have no remarks on your work. Considering that the problems are interesting, they would like to continue this work with another, a better American scientist. You can think over about another American scientist, who would be capable for this work, and then, after the problem with Dr. Löve has been solved, continue the work with him.



17. mart 1972.

Dr. Franc Sušnik  
Direktor  
Institut za biologiju  
Aškerčeva 12

Ljubljana

Poštovani dr. Sušnik,

Dr. Schmerz, koji vodi spoljne poslove u Smithsonianu, zamolio me je da Vam prenesem jednu informaciju delikatne prirode.

Radi se o projektu "Cytotaxonomy of Yugoslav Flora". Jednogodišnji rad dr. Asker Lövea na tom projektu dobio je lošu, neprolaznu ocenu. Zbog toga će Smithsonian ovu drugu godinu da finansira u visini prve (40. - 50.000 dolara). To je, takodje, poslednja godina za dr. Lövea, jer se neće saglasiti da on i dalje nastavi da radi na tome. Pošto dr. Lövea smatraju upornim, očekuju da njegovo uklanjanje neće proći bez trenja.

Na Vaš rad nemaju primedbi. Smatraju da je problematika interesantna i željeli bi da je nastave sa nekim drugim, boljim američkim naučnikom. Vi možete razmišljati ko bi mogao drugi da dodje u obzir, pa kad se stvar raščisti sa Löveom, nastaviti sa drugim.

Sa drugarskim poštovanjem,

*M. Mladjenović*  
dr. Milorad Mladjenović  
Savetnik za nauku

## UNIVERSITY OF COLORADO

BOULDER, COLORADO 80302

DEPARTMENT OF BIOLOGY

Boulder, September 28, 1972.

Dear Franc:

I have not heard from you since we left Ljubljana, and not since I sent you the promised manuscripts, but I know that you are busy with all the many things that require your attention, not least in the fall and especially around the time of the Scopoli meeting. However, I sent you copies of the manuscripts by airmail two days ago in order to be sure that you would get them in time for publication, because one never knows what the mail can do with such things, on either side of the ocean. Hope you at least receive one of these packages.

Since I wrote last time, I have been working on our report, writing it and rewriting, and yesterday it was sent to Washington in 20 copies as required. I can see little meaning in this, if they are to be allowed to let it be judged by people who do not understand our modern approach or are against it, or if they are to be allowed to use it for defamation of our abilities even in diplomatic letters to you, because as foreigners here we are without much possibilities to protest except by aid of expensive lawyers, whom we do not want to rely upon. Naturally, I have discussed the matter with several of my colleagues, not only in Boulder, and they all agree that the best way of getting this corrected is for you, and only you, to protest the methods and defamation to Ripley himself, requesting that he personally look into the matter and correct it. There is, of course, another method for you to get away from it all, by accepting their money and some "better American scientist" with emphasis on American, but that would also be the end of the promising part of the project because without proper knowledge of modern European botany and without an understanding of the cytogenetic background of modern evolutionary taxonomy, and without a will and ability to work as hard as we have done on this material during the winters where a decent library is available, there would be nothing but chromosome numbers without critical evaluation of the nomenclature and classification. But although I know that this is not your wish and that you have a fortitude that Americans are devoid of, it does not astonish me that the Smithsonian small men want this very fine and very promising and outstanding project to do what most of their men have done in the past: rise like a sun and go down like a pancake. But I trust that you spend much time to get it going again in a very much better way, and that you request emphatically that the so-called reviewers and other judges on this side must be properly qualified and competent in this field. I would even give them the three names of the very best and most competent judges and require that they ask them to go through the entire matter and that their recommendations, even as to the funding, then be strictly adhered to, without any interference by entomologists and bureaucrats: Dr. Harlan Lewis, dean of science of the University of California at Los Angeles and the President of the International Organization of Plant Biosystematists; Dr. Billie Turner, Professor and chairman of the Botany Department of the University of Texas at Austin; and Dr. John R. Reeder, Professor and Head of the Botany Department at the University of Wyoming at Laramie.

ch. and  
you c. for  
copies  
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30 av.

Digitized by the National Archives and Records Administration

I suppose the snow has come before the flowers of Favargera could be photographed? And that there was no time to get seeds of Hladnikia? But you may well have gotten both and then not had time to send them, because although we complain about lack of secretarial help and other technical assistance, you have considerably more reasons to complain about that, and then you also have difficulties in finding other help in all your many administrative duties. But I wish we had somebody as effective and energetic as you are here in our department, where everything goes as an old and not too wise lady determines, the wife of our present chairman!

Doris has been working energetically on the continuation of the atlas, and now I will also be able to spend my research time on making additions and changes in order to make it all still better. Naturally, there will always be needs to add to the information on the chromosome numbers, since more and more are being published, and there will have to be made some nomenclatural and taxonomical corrections from time to time, especially when we know more about the relatively few species that still are either unknown or critical in Slovenia. But if everything goes well and you succeed in letting the bigots understand that in you they have met somebody who does not let American money buy your conviction and who does not allow such matters to go the common American way in Europe with miscarriage of justice and direct foolishness, then it will be very likely that when we return in the spring the atlas will be complete and ready for printing if we so wish. And the checklist of the Slovenian flora will be that of the very best known flora in any comparable part of Europe.

I hope you and Milan will have time to make a list of all the determinations so far made, with proper herbarium numbers and other voucher informations, so that I will be able to select from it a long list of new numbers to be printed in the IOPE lists every time they are published in 1973, and also several groups of 25 each for the other series. For the latter you do not need to get every detail ready before sending it to me, because when I have made my part, you can easily add your, but the former needs to be with me before the end of the third week of October, because then I am required to send in the manuscript for the February list.

The secretary for the Congress promised me weeks ago to send you by airmail copies of the first announcement and to put your name on her list for the second announcement, though she wants to get your card also I hope the envelope has reached you and that you have decided to try to come, although I would also like to see you here sometimes during the winter. You may know that about a week after our congress the Genetics Congress will be in Berkeley, so if you are able to take of long enough time for both, this would be one of the few years you could make such an arrangement. And August is usually a slack month in Ljubljana, fortunately.

Our fall weather is fine, but there is already snow in Montana and Wyoming! I hope, however, that it will not come to southern Wyoming until after the middle of October, because our car will come to San Francisco October 6, and I will be driving it eastward through Nevada, Utah and Wyoming a few days later.

With the very best regards to you all from us all here,

As ever,

P.S. (in pencil to be erased):

Still nothing about the receipts asked for in the letter to the Presovlje address. (Why?)  
Notes note that it had been delayed.

Boulder, May 5, 1970.

Professor Hobart Smith, Chairman-elect,  
Department of Biology,  
University of Colorado, Boulder.

Dear Hobart:

My six years experience in the Biology Department under a narrowminded dean of arts and sciences and a couple of years under an unscrupulous dean of the graduate school has convinced me that not only do these men, who were selected to their posts by aid of connections rather than on basis of academic competence, want to eliminate all real scholarship from the non-chemical part of the Biology Department, but also that these too powerful administrators keep their eyes open to the fact that I am a non-conservative immigrant from a small but highly cultured nation at the same time as they refuse to accept that my education, experismot and achievements are far above those of the average professors at this university and therefore far above that of the junior college teachers they prefer for biology. I do not feel too badly about being kicked by small people, as for instance when the graduate dean recently helped his handpicked committee refuse me, and no other biologist here, a faculty fellowship after six years of good work, with the explanation that my work was only on "damned chromosomes", as if he understood their significance. I am more disturbed by that a good friend at the NSF has indicated to me that this dean has prevented a renewal of our grants by telling them per telephone that there are no and will not be any facilities in the department for work of the kind we do - but this may not be true. Sometimes I have wondered if this administrator perhaps may have been behind the visit the FBI made a year or so ago to Bonde, Helvig and Shushan to snoop about my opinions and beliefs - the same as the CIA had done for years but through a direct contact through their exemplary and pleasant agent here. Be that all as it may, but I have tried and will continue to try to do my very best in whatever I do in research or other work, at the same time as I will continue to make efforts to avoid all contacts with this apparently small and insecure man, following the wise advice by good old Rudyard Kipling regarding the honesty of the Saxons that ends with "my son, leave the Saxon alone", or perhaps the following fits still better as an advice in this case: "Do'nt dance or ride with general Bangs - a most immoral man." So I will do my best to keep away from Lawson Crowe and hope that his superiors will understand his not so straight moral and disposition before he gets an opportunity to sink the university to his own level.

I am more bothered about the attitude of Briggs, who is not a great man but I believe an honest man, although not free from bigotry as so many here. I cannot understand his inability to realize that even non-chemists may be too outstanding in their research for junior college teaching, and his unwillingness to see that even an immigrant who speaks with an accent (which he once has tried to imitate to my face in a very compromising situation) may have feelings and even be internationally recognized in their fields and thus ought to be treated economically no worse than those who do similar work and are born here. That my strength lies in my profound education at one of the finest and oldest universities in Europe, and in creative research and writing, ought to be clearly documented by my thirty years of publishing several hundred research

papers that have strongly influenced the international development in my field, some much appreciated books, and numerous research reviews, all of which have caused many international honors to come my way. And it ought to be evident even to him that my strength in the fields of evolutionary botany, in which I am a recognized international leader since decades, as you so well have told me was also your impression, has been steadily growing, as can be seen by the fact that in 1964, the year I came to Boulder, I published 15 research papers (alone and with my very learned but much discriminated against wife) and five research reviews, whereas since 1965 I have published 60 research papers and books and 54 research reviews, altogether more than 1500 pages, despite a normal teaching load, heavy administration, and virtually no research facilities.

Since it has become very apparent recently that the two deans do not appreciate research in the Biology Department, and have even decided to complete their pertinent discrimination against me by professional assessment, as soon as they can invent some ways for this, with or without outside help, I am forced to draw the conclusion that if we cannot find a more congenial atmosphere elsewhere, I must ask for a transfer of my position and salary to another part of the University where good and productive research is better appreciated than under these administrators. I could do this straight, by contacting the President himself personally and in writing, but since that might be construed as an indication of disagreement between us, which is non-existing as we both know, I would rather not go above your head with such a wish. Instead I hereby submit to you the humble request that you make all possible efforts on my behalf to get my position transferred from the beginning of the academic year 1970-71 to the University Museum, where my duties could be those of a professor and curator of evolutionary genetics, evolutionary botany, or simply experimental botany and biogeography, or straight cytotaxonomy, a subject in which I know I am the recognized world leader. Or, if this should be more feasible, I could be transferred to the Institute of Arctic and Alpine Research, though my wide interest in evolutionary biology certainly would fit better into the work at the Museum at the side of Professors Meslin, Robinson and Weber. Naturally, I would teach at the Biology Department in the capacity of joint appointments as do Professors Meslin, Rodeck and Weber, if so desired, and accept the responsibility for graduate students whenever appropriate. At the same time, since at least the Museum is short of space, I would like to request that you arrange that I be allowed to keep my present office and laboratory space in the Armory until other suitable arrangements can be made.

I hope that you realize that this letter is not to be construed as a declaration of any criticism or doubts from my side of your prospective administration. But I hope that such a transfer will help to alleviate some of the illwill of the two administrators mentioned towards the Biology Department and allow your own contacts with them to become much more cordial than they allowed during my chairmanship.

Naturally, this letter is personal and confidential, though I hope you can and will use its contents to get the action asked for, the sooner the better. You have my permission to show it, again confidentially, to the directors of the Museum and Instear, but not to copy it for them or others nor to broadcast its contents even among our colleagues in the Department.

With the very best wishes and thanks for your positive action,

Yours sincerely,

Åskell Löve

Note attached.

Hope you received the letter of Sept. 4  
sent via Winkler, who probably will write me about it soon. If you do nothing, I expect no answer.

UNIVERSITY OF COLORADO  
BOULDER, COLORADO 80302

DEPARTMENT OF BIOLOGY

Boulder, September 19, 1972.

Dear Franc:

I am sorry that I have not written before to thank you for the summer and all that came with it. But when we came back to Boulder we had to move our offices and labs once more, and then I was required to send in a financial report at once, the Smithsonian even phoned me to remind me of this! Most of the time, however, I have been able to spend on writing and typing the two papers I had promised to send you as soon as possible, and they have left Boulder today by airmail so I hope you get them about the same time as this letter arrives in Ljubljana. The smaller of these papers is about some nomenclatural adjustments in the monocots, by us and you, but the other is the first in the series on chromosome numbers, based mainly upon what Milan gave me the last morning in Ljubljana. You have the manuscript of the third paper, which actually could have been printed long ago, but I hope all three can soon be printed in the new *Scopolia*, because it is extremely important that these data become available and visible to those who are judging our work without having anything to look at except our own lists. So, if *Scopolia* cannot be prepared almost at once, perhaps the wisest way would be to forget about it for the time being and instead beef up the *Biol. Vestnik* and print these papers there at once this fall? It is important that they do not wait.

As to the paper on nomenclature, I do not think there is much to be added. But the paper on the chromosome numbers needs your attention in many respects. For the first, I did not get the full number, 25, of species that we had planned to include, so I went to our older informations and added some species, for which I therefore did not have the complete informations, and in some of the other cases Milan had not been able to complete all the informations for the others. You will see that in several places the numbers in the herbarium are missing - that ought to be simple to correct; and also some details for the vouchers. Under *Degenia*, No. 13, there is, at the end of the chapter, a reference to an older count by Vorišec (? I could not read the name with certainty in the notes by Milan) 1937? in *Acta Bot. Croatica*? that needs to be checked... and the reference must be added also in the list of literature at the end of the paper. For *Iris macedonica*, the reference to the description by Horvat (year?) is missing and thus also the name of the paper (in Serbo-Croat?) in the literature list; although I had the year of publication of Horvat & Horvat on *Iris croatica*, I also did not have the title of the paper, in the same language? For the literature list. Hope you can add this, and if you feel you ought to add discussions and other remarks and correct some mistakes I may have made, it ought to be self-evident that you should do that also. And then I hope you will have time to read very carefully the proofs of all articles, because it will be difficult for those who either type them or set them to do it completely correctly in a language other than their own. Naturally, I could read proofs here, and perhaps it would be worth while to send them to me also, since it does not need to take more than two weeks to get them back and forth.

I am sorry that I was not wise enough to ask you and Milan for a list, as complete as possible as to names, numbers, information about vouchers, and herbarium numbers, of all the many taxa that actually have been studied during these two summers and winters, together with who is responsible for the actual counting of the numbers, because if I had had such a list here, I would have been able to include a good deal of the results in the IOPB list for November, and then again later next year, so that the results become known. Perhaps it would be wise to furnish me with such a list soon, so that I could use a part of it for the February list that is to be sent in to Taxon in early November? You do not need to think leaving out reports that have already been published, because I would easily be able to select for each list numbers that we have not published previously. But those species that ought to be included in the longer list in Scopelia could be given in more detail, and when I do not have the details about species that I feel ought to be closer discussed, I could easily ask for them by letter, because that long we can always wait. But it is essential that as few numbers as possible be left unpublished every year, so that others can see what actually has been accomplished.

I am supposed to report on the scientific results of the work before the end of this month, and will do it shortly and concisely. I will send you a copy when available, but it will hardly be until just at the end of the month. I have not been asked to send a new application and will not do so either, because I have a good reason to believe that this is all discrimination that can only be fought with other method if I am to do that. But it is also apparent that you are the one who can protect us from this kind of slander and treatment, and that only if you emphatically request a withdrawal of this decision and a judgement by a neutral and qualified agency, even mentioning that you (not I) will publish or make known to higher officials the letter that you received from the Embassy, if this is not corrected at once and funds made available for the continuation of this work. You have, I hope, sent your letter to Ripley? And perhaps also asked the Yugoslavian government for help? And it would not hurt if you could make a report about the entire work and our part in its success and send it both to Ripley and to Schmeertz (copy to me) either directly or through your Embassy or both, because bigots in America do not understand any other language than a strong one. I am sure that nobody knowing about what we have been doing these two years would hesitate to agree that the work has been first-class and that it is very promising, but if nothing is done from the Yugoslavian side to ensure its continuation through a more proper grant from the Smithsonian, the bigots will only be pleased. So, if you have had to let other works take all your time since we left, perhaps you could take time for this for some days if necessary and force the matter against the discriminatory powers in little Washington?

We have been trying to find some of the books on the Balkan flora, and have been successful with Rohlens's book, which was found in Praha. But Degen and Hayek still are not available to us here, and if I understand the lists from the American libraries, there are only 1 - 2 copies of Degen in the entire country, and less than five of Hayek. Any progress with the xerox?

We are also working hard, especially Doris, on the Atlas and have received fine help with the taxonomy from Holub. Any progress as to the distribution within Slovenia of the species without such informations? And any progress at the Computer Center?

And have you thought about coming to here during the winter, since we have funds available for this? Or would you prefer that they allow you to use it for the Congress, although funds for that could be arranged from the grant next year, if continued?

All the best to you all from us all here.

Added for general, to be read after reading  
Why we want about my fight by letter regarding  
the 1982/83 etc. at B. University? I read the book!



Mr. Whitehead - (202) - <sup>628</sup>NA8-1810, ext. 5091.  
(Betty Wingfield).

1/ Reaggly (30 copies)

Restrict to Slovic - Julian Adps.

Restrict number of people.

Cut down the sum to 46,800

open for some further adjustment.

2/ Will write Swissh and explain all the  
send him the revised proposal for reapplication there.

(copy of Swissh letter)

~~3/ We would like to visit~~ (officially) ~~in mid-May~~  
(include names of our reviewers) - review study and <sup>asked in mid-May</sup> - other rooms: more added when progr. period: study.

3/ Can I keep the grad. student and technician  
waiting - promising them a definite answer  
by mid-April?

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4/ Would like to make the visit to Belgium and  
Ljubljana in March or April. - (both if possible) -

Can I send the Authorization forms for approval -  
though an exact date <sup>with P.A.</sup> can't be fixed? Then, want  
to go east in Iceland, to sleep away the  
seven hour difference before coming to Europe  
and west straight from Belgium to Denver.  
What about dollar costs en route - except for  
the stop-over in Iceland?

- If necessary - let Swissh write and say that visit needed - diplomatic

(2-3 weeks)  
(or a few days)

Febr. 11: [Happy to support vis-à-vis Shumilov] (can go to Ljalline with revised project) (except for outside reviewers) pick time

Not too happy -

rec. construction turnaround - (March 15)

Much too big - for a start -

Reg. Biol. Adv. Council - 12 members - 2 scientists  
5 or 6 who do -

Revised:

- Scope too broad -
- level of budget too high -
- red. in Yugoslavia

One colleague this year.

Names of those who should have reviewed it -

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Such a huge project compared to others. -

Robinson & Martin = ecological trend down

- paleontological = 59,000 J. two years.

Would consider a new proposal at these lines -

No more than \$50,000

Ask Yugoslavs accept this -

New review cycle begin March 1 -

May the proposals - awful lot of many -

L.2L. Favayer & Gentry, two new genera of alpine Gentianeaceae. —  
Bot. Not. 125, 1972, pp. 255-258.

L.2L. Vasoumbria — new genus of orchids. — Act. Bot. Neol. 21, 1972, pp. 553-554.

L.2L. Gentianeaceae — an exercise in evolutionary classification. —  
Act. Bot. Acad. Sci. Hung. (in press).

L.2L. The natural genus of Gentianeaceae. — Rec. Adv. in Botany (in press).

L.2L. Cytotaxonomy of the forest taxa of Phyllitidis. — Act. Bot. Acad. Sci. Hung. (in press).

L.2L. Nomenclatural adjustments in the Yugoslavian flora. II. Platidiphytes &  
Dicyledes. — Prebia (in press).

—— L.2L.: Cytotaxonomical atlas of the flora of Slavonia. — Computerized.  
ca. 1000 printed pages; ca. 75-80,000 IBM cards.

Lozka, M., Susnik, F., Löve, A. & Löve, D. 1977: 10PB chromosome number  
reports XXXIV. — Taxon 20: 788-791. (114 species).

—— — — — — 1972: 10PB chromosome number  
reports XXXV. — Taxon 21: 337-339. (75 species)

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Susnik, F., Lozka, M., Löve, A. & Löve, D. 1972: 10PB chromosome number  
reports XXXVI. — Taxon 21: 345 (29 species).

Lozka, M. & Susnik, F. 1973: 10PB chromosome number reports X<sup>L</sup>. —  
Taxon 22: 285-291 (7 species).

Susnik, F. & Lozka, M. 1973: 10PB chromosome number reports. X<sup>L</sup>. —  
Taxon 22: (in press) (49 species)

Löve, A., Löve, D. & Susnik, F. 1973?: Cytotaxonomy of Yugoslavian plants:  
Introductory remarks. — Scyptica 1: (in press)

Löve, A. & Susnik, F. 1973?: Chromosome numbers of Yugoslavian plants. I. —  
Scyptica 1: (in press).

Löve, A., Löve, D. & Susnik, F. 1973?: Nomenclatural adjustments in  
the Yugoslavian flora. I. Some monocotyledons from Slavonia. —  
Scyptica 1: (in press).

Other selected authors familiar with geobotanical cytotaxonomy:

- Professor H. G. Baker, Department of Botany, University of California, Berkeley, California 94720.
- Professor J. H. Beaman, Department of Botany, Michigan State University, East Lansing, Michigan 48823.
- Professor K. I. Beamish, Department of Botany, University of British Columbia, Vancouver, B. C., Canada.
- Professor C. Ritchie Bell, Department of Botany, University of North Carolina, Chapel Hill, North Carolina 27514.
- Dr. A. L. M. Bonnet, Institut de Botanique, Université de Montpellier, 5 rue Auguste Broussonet, 34 Montpellier, France.
- Professor A. S. Boughhey, Department of Population and Environmental Biology, University of California, Irvine, California 92650.
- Professor L. Boulos, Department of Botany, Faculty of Science, University of Libya, Tripoli, Libya.
- Dr. R. K. Brummitt, The Herbarium, Royal Botanic Gardens, Kew, Richmond, Surrey, England.
- Professor G. L. Church, Department of Botany, Brown University, Providence, R.I.
- Dr. A. Cronquist, New York Botanical Garden, Bronx Park, New York 10458.
- Dr. J. Domboldt, Institut für Systematische Botanik, Königin-Luise-Str. 6-8, Berlin 41, Germany.
- Professor W. S. Flory, Department of Biology, Wake Forest University, Winston-Salem, North Carolina 27106.
- Professor R. K. Godfrey, Department of Botany, Florida State University, Tallahassee, Florida 32306.
- Professor B. Györfy, Institute of Genetics, Herman Ottó ut 15, Budapest, II, Hungary.
- Professor J. Jales, Botanical Institute, The University, Helsinki, Finland.
- Professor E. Landolt, Geobotanisches Institut ETH, Zürichbergstrasse 38, CH-8044, Zürich, Switzerland.
- Dean Harlan Lewis, Deans' Office, University of California, Los Angeles, California 90024.
- Professor Margaret Y. Menzel, Department of Biological Sciences, Florida State University, Tallahassee, Florida 32306.
- Dr. D. M. Moore, Department of Botany, The University, Reading, England.
- Professor R. Ornduff, Department of Botany, University of California, Berkeley, California 94720.
- Professor P. H. Raven, Division of Systematic Biology, Stanford University, Stanford, California 94305.
- Professor J. Rousseau, 5208 Cote St. Antoine, Montreal, Canada.
- Professor R. Soó, University Botanical Garden, Illés ut 25, Budapest VIII, Hungary.
- Professor F. A. Stafleu, International Bureau for Plant Taxonomy and Nomenclature, Tweede Transitorium, Uithof, Utrecht, Netherlands.
- Professor W. C. Steere, New York Botanical Garden, Bronx Park, New York 10458.
- Professor D. H. Valentine, Department of Botany, The University, Manchester 13, England.
- Professor R. K. Vickery, Department of Life Sciences, University of Utah, Salt Lake City, Utah 84112.
- Professor E. G. Voss, University Herbarium, University of Michigan, Ann Arbor, Michigan 48104.
- Professor W. H. Wagner, University Botanical Garden, University of Michigan, Ann Arbor, Michigan 48104.
- Professor W. A. Weber, University Museum, University of Colorado, Boulder, Colorado 80302.

UNIVERSITY OF COLORADO

BOULDER, COLORADO 80302

DEPARTMENT OF BIOLOGY  
OFFICE OF THE CHAIRMAN

Boulder, February 28, 1970.

Mr. Kenneth D. Whitehead,  
Foreign Currency Program,  
Office of International Activities,  
Smithsonian Institution,  
Washington, D. C. 20560.

Dear Mr. Whitehead:

The thirty required copies of our revised proposal were sent by airmail to you earlier today, so we hope it will not be necessary for you to extend the deadline. And I hope that this time even the budget will be found to be acceptable, and that we will not have to wait so long for the decision (or at least for an indication as to it) so that this jeopardizes the participation of our helpers or directly hurts them. I hope the reaction of the Council will be favorable, but trust that you will give me an opportunity for further modification if needed, rather than let the work be further delayed or dropped. I am convinced that this is an excellent proposal and that it will enable us to demonstrate fine results already after the first season so that your office and you yourself will have reasons to be proud of having given us and Professor Sušnik encouragement and help. Although I rather expect a reduction of species as a result of this work, I hope we will also find some beautiful new species which might perhaps get a name that could remind us of the help we got from you.

I am grateful for the opportunity to give you a list of people who seem competent to judge our work in this field, since that competence seems to be far from being widespread, at least not in America. I have listed thirty specialists who are or have been active in this field and then other thirty who are familiar with the philosophy and methods, though they may not have used them extensively themselves. I hope these lists will furnish you with some names that were not among those who read the proposal the first time, though it is, of course, up to you to decide if you will use any of these names or not, when you try to get a balanced opinion of the value of this study. Actually, the proposal is to duplicate studies we have done in Iceland, Scandinavia, Spain, and Mt. Washington and which have been done best by others from Scandinavia and the Soviet Union rather than in southern Europe and America, and I know that the results will be good.

With the very best regards,

Sincerely,

Åskell Löve

Exhibit 1: (8): Tax certificate of a student.

Exhibit 2: (3): letter for Dallas faculty, rec. 1973

3: (7): my letter to Hunt 1970.

4: (6): Letter - report to Dallas, 1972

5: (2): Letter to me for Smith for Y. J. Entomology 1972

6: (2): letter for the donor

7: (2)(1-5): ORS report for reinstatement of budget

9: (2): July 2 letter to Schmidt

10: (2): Sept. 4 letter to Smith

8: (Voucher copy) for Entomology  
(Bank receipt)

11: Note, by am. with Dulovic, July 20.

13 (12) 11: to: & (1-5) - letter for Smith Feb. 11/1973

14 (12) 12: My issue to Smith

12 (12) 12: a (1): Note for Entomology

15: a(b): Letter for Smith to Univ. + Univ. letter to me.

16: Request

17: Very frustrated request

17: June 1 letter for Smith

18: June ... last letter for Smith (last of material you sent)

19: My issue to Smith

20: ~~My issue to Smith~~ Driggs' 1967 letter about the deposit.

21: ~~Dr. ...~~ Hunt's letter

22: ~~Dr. ...~~

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Staying on the back with - letter forwarded by the Smithsonian.

Hunt tried, with help of Driggs, to get a promise to delay my departure until after the summer school, but this was categorically refused. <sup>no promise - multiple addresses</sup> <sup>Driggs' letter said there was a promise - but that actually was not - see above exhibit</sup> tolerance (or promise or grace) was possible for me who was out - better American scientist. <sup>He had no money, when I was to deliver the cheque, but he had various excuses why he could not come right then.</sup>

Nov 4, 1976:

The larger address was not right & was under mine, but I of hope with little letters, just in addition to the note above on 11/11/76. And hope in other years from future shipment from the same address. However, have since received a note only from the same & turning things, paper subject as I say. Just as saying was done.

I was apparently such a degree critical that must be promised for the program last the students be interested - although I still was worried about my lack with my cards. <sup>Driggs only a little direction - do that?</sup>

I was under the impression that anything was to make a thing that the program must be signed next morning, when I had promised to pay the bill - therefore, I should have to see with me - he tried to get a delay until 11:30 tomorrow.

Eventually might to present us for completing a decision what we had extracted for, of Hunt's, Mrs. D's, acceptance - but not a whole legend - Driggs desired.

When we hear for Colorado about signed and fair employment practices we must avoid thinking about hypocrisy...

Most of the players in the game are under to you, ~~but~~ we are listed in the American Plan of Science, but I could refer you about the background of those you may feel interested in knowing more of the positive, about, if you are justified.

1976, 11/11/76, 11/11/76, 11/11/76

San José, September 21, 1978.

Dear Jack:

I must apologize for not having written to you long ago to thank you for a fine letter after your return from Iceland, and for the nice description of the trip in the Newsletter. I have tried to start to write to thank you for your consideration several times, but on each occasion I have refrained because I could not find the words for the latter part of the letter, for reasons that I hope will be evident to you with your deep understanding of human beings and the forces that motivate their thinking and actions. But although I still am not able to write what I want to, and may be using words that do not quite tell what I want to say, because English is not my native tongue, I hope you forgive that I send this, and also hope that you keep its contents as confidential as you feel is appropriate, since I am especially anxious that it does not hurt others.

As to the Iceland and Greenland trip, I could write pages of remarks and notes that together would simply express the fact that I envy you this fine experience. You have seen more of the countries than most Icelanders have done, and you also have understood what you saw much better than even they could do, although the fast travel may have caused you to miss some of the fine details of history that your guide might have furnished, especially details that are dear to us because of our upbringing and important for the understanding of our conscience and disposition and of our character and logic. And, by necessity, you missed the northwestern parts of the country, where I was brought up and started as a botanical collector when I was 13 years old, and thus also the most fertile parts of the country. I hope, you did see this later and then in my company, since I sincerely hope that your health will continue to allow you even strenuous travels for many years to come. But you may want to see something else, there is so much in the world that a person of your learning would like to see so that even a long life of travels always is insufficient. You might like to know about the only minor mistake in your report: the place you call Skálholt is actually the crater Kerid on the way to Skálholt. Skálholt, however, is the old site of the Icelandic bishops and the first Icelandic learned school, which still is our good old college at Reykjavík; presently it has a modern church and a new adult school - and a lot of ruins from the past thousand years and a wealth of Icelandic history connected with the names of the many great men who lived and died there. Among them Jón Arason, the last catholic bishop, who with his moralic strength prevented the Danes for a generation to force the new rule upon the country and thus delayed its impoverishing and decline of its education that followed the fall of the catholicism. He was at last murdered by the Danes in Skálholt on November 7, 1551 at a place still marked by a cairn, one of the darkest days in the history of Iceland, where foreign miscarriage of justice has long predominated....As to the Greenland willows you were in doubt with, I believe the larger shrubs close to the Hotel Arctic are *S. cordifolia* and *S. arctica* and possibly their hybrids, whereas other and smaller shrubs which you may have recognized from Labrador, were *S. herbacea*, *S. uva-ursi* and *S. arctophila*...other species are not there.

As I mentioned, Icelanders of the past were not unfamiliar with miscarriage of justice, but I had never experienced it before I got into contact with the Americans in Iceland and at home. That happened even before we contemplated moving to this continent, when the American Embassy in Reykjavík honored us by putting my name on its black list of those not acceptable for American visa.

Of course, nobody at the Embassy knew us personally or bothered to learn to know us, and we were never confronted with the "evidence", a peculiar but not unknown practice in American diplomacy. I was actually pleased when I heard that I was on this list, which I shared with perhaps half the Icelandic population and with all those Icelanders who in my eyes were worth something because of their cultural or nationalistic standing or moralic stamina. Also because the "information" was said to include the remark that I had been a very active communist during my years in Sweden (where I never met anybody of that opinion, neither have I done so in America so they must be rare even here). And I had another reason to be proud of such an accusation, because then I must be a good deal greater than any of my colleagues, who had sunk into such politics and become much delayed in their studies, whereas I had succeeded in completing my studies in a shorter time than even the most intelligent of Swedes. It was, naturally, perfectly in order to list me so for internal American use, but when I applied for a visa for Canada, the American Embassy went out of its way to "inform" the Canadian ambassador, who resided in Oslo, about this "knowledge", and so hurt me and my family badly for almost a year before we succeeded in getting this corrected by aid of several outstanding Swedish colleagues. That certainly was libel or at least malicious slander, but it astonished me that when I told some of my American colleagues about it, none dared to say a word in protest, although my few Canadian friends made great efforts to help us. This was, of course, during the McCarthy era, when established scoundrels were not afraid of getting into the daylight with their character assassinations. But I have understood later, that the success of that senator and his numerous cooperators was not based on anything new and ephemeral but on the perennial conservatism and inferiority complex and tendency for cloak-and-dagger games of the generally badly educated Americans, even those with so-called university education, when confronted with good learning and honest democracy, and, perhaps mainly, on the almost total lack of fortitude and courage that seems to characterize much too large portions of this nation so that even your so-called best friends turn chicken when a government employee, a policeman or a lawyer accuses you of wrongdoing even without the slightest reason, or when a bandit bombs your house or tries to hunt or stab you or shoots you with the much too common American gun, by aid of which some minorities in this "land of law" have had to force their way to the rights to live as human beings. I realize that these are strong words, and that others than my friends might like to use them as evidence of my dissident opinions that are resisted here no less than in Russia where American politicians now protest against them, but I am sure that even you are likely to agree, since, unfortunately, they can be substantiated, not only for our times but for generations past. Although one must emphasize in all fairness that this land has also fostered some of the most learned and most democratic and most fair men the world has ever seen. I am proud that I have had the opportunity to know some of them and to admire the others.

I am sure you understand that I had lost all hope for fortitude in America years ago when all my so-called friends turned chicken when I needed them most, even those who had claimed they would help, so I was pleasantly surprised by the personal part of your letter...but it was not unexpected. I am very grateful for the friendship you demonstrate when refusing to believe the slander that told you that I had been forced to resign my position and facilities to work and even live because I had mismanaged some research funds in order to buy a car for my daughter! I can only say that this is clearly an aetiological myth, invented as an "understandable reason" for what was fabricated to harm me, probably because my European and genetical ideas do not fit in with the dogmas that still hamper progress in American botany. It is right, that I have bought cars for my daughters, not one but actually <sup>two</sup> for both of them, .. but years before we came into contact with the Smithsonian.



And the last car I bought was paid in America in the spring 1972 and used in Yugoslavia that summer, because our "rented" car from the "University car pool" in Ljubljana the year before had been highly unsatisfactory, and we had for years driven our own cars when we were collecting in America or Europe and charged the mileage to our research grants. I thought that was a common practice. Of course, the slander you heard, and many may have spread though we have never heard it before, is only a shallow additional cover-up for actions that we believe were a very serious violation of our civil, academic and human rights, very much <sup>so</sup> than and alleged but fabricated "fraud" in a place where perhaps most scientists accept so-called research funds and never return even acceptable reports of a worthwhile investigation, but perhaps again typical of the false morality of this home of superior hypocrisy. It is a complete untruth. I am hesitant to mention the real story in a letter because we have reasons to believe that some of our mail is regularly tampered with and since we are still getting threats through anonymous and very short telephone calls. Doris, therefore, does not want me to tell anything even to you in a letter, that shows how even she has become suspicious of everything American after a generation of discrimination and miscarriage of justice. However, since it would be unwise and unfair to refuse to give you information that you alone have had the courage to ask for, I will try to give you the main points below, though hesitantly and in the hope that it will not turn also you away from us.

October 12.

Since I wrote the above, I have made numerous unsuccessful attempts to find appropriate words to tell you what happened five and more years ago, but since every story has many sides and the so-called truth can be told in various ways because we never know the absolute truth or even the relative truth of the honest lawyers, I still have not succeeded to do this in the way I want, especially since the material seems to have a tendency to grow into booksize, and a shameful book at that. But I trust that you will read this with wisdom and indulgence and, especially, with benevolence and the cognizance of the fact, that it is written by a foreigner whose knowledge of your language is restricted to other fields than that of the intrigues of lawyers, and so I may misuse expressions or select words the meaning of which may perhaps at least slightly differ from what I took them for. But such mistakes are unintentional and hopefully understandable, and a wise reader will easily forgive harshness when softness was intended.

In order to, hopefully, strengthen your impression of me as an honest and trustworthy scientist, I would like to add here some information about the moral and ethical background that has affected me and made me, I hope, a more complex and a more sincere being than are some others. I should emphasize that the three Icelandic families of my grandparents have always been characterized by the strict conscience and disposition that has been the hallmark of all the many respectable Icelanders for more than 1100 years; in their genealogical records that go back to before the colonization of Iceland there are only peaceful and honest men, many of them leaders in various fields, and from them I believe I inherited the various mental attributes which are amply documented in my own activities for a lifetime. I know, however, only a little about the genealogy and characteristics of my paternal grandfather, who was a Danish merchant risen from the class of farmers and sailors as were my Icelandic ancestors of course. My father was his illegitimate son, and his mother was a very poor woman who had lost her parents when she was an infant. Despite all drawbacks in the severe conditions of the last century, he became a successful though never wealthy seacaptain who is said to have been the essential

power behind the belated revolution in Icelandic fishing when he as a young man got a loan to buy the first motor for a fishing vessel. He married twice. I am the oldest of six sons and a daughter of his second marriage, so I was brought up in the poverty between the wars, and I was still only a boy when he was forced to retire from the sea because of ill health so I and my brothers must work our ways during the depression to get through the very competitive Icelandic upper school system. With understanding from home and earned state scholarships, however, we all made it so well that two of us got doctorates abroad, one became a vice president of the Teachers University, one was the organizer and manager of the exemplary Icelandic organization of rehabilitation of people crippled by disease or accidents and a leader in the European association of such organizations, one is a successful businessman and importer, one died when still preparing to become an artist for what he had apparent talent, and my sister is a librarian who has brought up three intelligent children, all university educated as is the entire third generation. At home we were encouraged to enjoy and take part in all kinds of cultural activities and handicraft. Since we could not afford to buy more than a limited number of books, we all read through the good libraries available in our community, including of course the enormous Icelandic and Scandinavian literary heritage and the Great Books of civilization, even the Bible, the books of Plato and Das Kapital, which certainly have influenced my life no less than did my spartan upbringing and my years of study in Sweden. I even dreamt of becoming a philosopher, and good old Kant, who presently is read by much too few and probably by no scientists in America, became dear to me so that his learned but complex Kritik der reinen Vernunft still is placed in the bookshelve on the back of my desk together with some other selected works of classical philosophy and poetry. Still I continue to feel, with Kant, the greatest wonder and awe from the starry heavens above me and the moral and ethical law within me: I am sure you'd do too, and that you agree with my "Key comments of a scientist" (exhibit 1) that I used to give to my advanced students.

The influence from home and from good teachers, from the Icelandic heritage and the great books, made me a promising young poet during my college years, and I am still proud and astonished of the considerable contributions that I could make in that field during these few romantic years of my youth. That training probably led to my success in writing my own language that culminated in an essay in a competition that at graduation resulted in the Golden Pen Award for Icelandic composition, still the most prominent but rarely given award at our very old and esteemed college. I also became deeply interested in philosophy, mainly metaphysics. However, I knew all Icelandic plants then known when I was only 13 and made during two summers a mature collection in the far northwest of the country, in the most arctic coastal area, where my then retired sailor father was a lighthouse keeper. That was probably the main cause of that in 1937, when I had earned a good scholarship for studies abroad in a field that I could select myself, I decided to go to Sweden to study botanical genetics rather than to go to either Heidelberg for aesthetics of Paris for philosophy for both of which I had received a nod from these places. But it may also have been my interest in contributing to the development of Icelandic agriculture, because I made efforts to learn everything in plant breeding with which I mainly worked after returning to Iceland in 1945.

Before continuing with the main contents of this letter, allow me to add that when I was a boy of seven, I joined the temperance movement (I.O.G.T.) and promised neither to use alcoholic beverages nor tobacco. Although others certainly regarded this as only a temporary promise that could be broken when grown up, I have kept it for some reason, perhaps because that I seem to tend to keep promises slavishly however uncomfortable that may be. I have never used these

or any other drugs and still do not even drink coffee or tea. I avoid swearing or using foul language though perhaps not always successfully, and also refuse to take oaths, a custom that I find to be greatly misused in America, and I have been married only once and never been interested in adultery. Nevertheless, I believe that my friends would agree that I have shown them tolerance when they differ in this and also when of other opinions. Although my father was strongly agnostic and among the first to withdraw from the Icelandic state church when this at last was permitted, he insisted that we get a thorough religious education in the school, because he believed that Christian ethics and morals were among the most necessary learnings and the essential pillars of our civilization, and he and my mother and grandmother impressed us children with the importance of seriously trying always to follow the Golden Rule. I remain an irreligious Christian and at least try to follow the morals and ethics of the New Testament, whereas I abhor the Old Testament with its eye-for-an-eye- tooth-for-a-tooth advice with which the world still seems to be governed. I am proud of that my parents also encouraged us children to visit regularly the only Sunday school in our town, that of the Salvation Army, which we found to be a fine organization of good people dedicated to helping the poor and the weak. I am also proud of having belonged to the Icelandic nationalistic youth organization, Ungmennafélag, which during the first half of this century brought up and strengthened the democratic minds of later leaders in every important field. And I am especially proud of having been the second Icelandic boyscout to become an eagle scout and to have gained moralic strength through the influence of this fine international organization, the travels with which strongly affected my view of nature and my own development as a botanist. Since I had to start working to help myself and my family already as a boy of seven and was brought up among working men and numerous gifted children who suffered more poverty than I did, I came to be strongly class oriented so that when I became interested in politics as was typical of the college students in Reykjavik in the early thirties, it was to be expected that this and my strongly nationalistic and democratic background would push me into the labor movement and into the Socialdemocratic party, of which youth organization I was the secretary in 1935 - 37. Afterwards, in Sweden, Iceland, Canada and the USA, these leanings have changed but slightly, although I have not taken part in active politics since 1937, except voting between 1946 and 1952, for the simple reason that my energy has been insufficient for activities outside my science, as is perhaps amply documented in the fact that I have published close to 600 papers and 15 books in botany and genetics after I had completed my three Swedish degrees on record time, plus teaching and administration that I did for living.

No more of this to avoid to send you 1000 pages of biography, but I felt you should know so that you may understand better my reactions below.

You may read my abbreviated biography in the Who's Who, but we came to the States, or to Boulder, from Montreal where we had been caught up in the general fight between the French and English, but the discrimination we experienced there was doubtlessly also caused by envy by small souls, as always, because we seem to be unable to do as little as others even without proper facilities. Since our positions at Montreal had been rudely terminated, we had no bargaining power when we came to Colorado, actually wanted none because we detest all kinds of political pressure which to me is anti-democratic. However, despite of my honesty in reporting the situation as it was, we were immediately persecuted through a lowered salary, denial of employment of Doris despite her superior competence matched by none of the men in that University, etc., etc. Notwithstanding hardly any facilities, no gardens or greenhouses, no technical assistance, no laboratory equipment, hardly any library except our own, we nevertheless succeeded in doing

more research than others had done before us in Boulder, but instead of causing praise, this of course resulted only in envy and, consequently, some cloak and dagger games connected especially with a small but powerful group at the Medical School in Denver that wanted biology at Boulder to be replaced by biochemistry disguised under the new name of molecular biology. We had experienced similar reactions earlier for the simple reason that your less conscientious or able colleagues always resent when you are not satisfied with the minimum of work and ideas for maximum pay - whereas we had always done the maximum of work even without pay since we first got research support in Sweden and were told that bookkeeping was not essential but that if we wanted continued support we had better do at least what was promised in the application and publish our results as soon and as accurately as possible, a policy that we have always followed despite our observation here that it is not honored by most American grant seekers. Discrimination because of superiority is probably typical everywhere, I observed something that indicated its existence at Philadelphia and certainly at Harvard, cf. the ugly story around Merrill and Sax and Johnston and all the disguise that followed it.

Although the administration at Boulder never showed our work any appreciation, except for encouragement from Dr. Manning, the fine vice president during our first years, and Dr. Archer, the second graduate dean during my tenure, and the administration at the end of my term as chairman and later even went out of its way to run me down, e.g. when a hand-picked committee of non-peers chaired by a small molecular biologist refused me a so-called faculty fellowship for which I was more qualified than any other biologists who received this honor during these years; the reason given by the then dean was that I was working "only with damned chromosomes", his specialty was said to be philosophy of religion. However, my colleagues had elected me in 1966 for a period of chairmanship of the department; they seemed to be satisfied with my efforts since I believe their letter to me at the end of my term (exhibit 2) was sincere, and they at least twice recommended me for a university lectureship, which of course was turned down by the same dean who later felt competent to be my very partial judge, see later. And there were several other cases that can only be regarded as downright discrimination and miscarriage of justice - I was just an uncomfortable immigrant casting a scientific shadow much too big for this environment and perhaps even for this country? This same dean went even so far once in 1968 to phone me and ask why I was working for this bunch of incompetents, meaning the Biology Department, but my reaction stopped him from continuing or from proposing some change in my position...he only was waiting for a chance to kick me out. You may read more about the situation at Boulder between the lines in the next letter (exhibit 3), which I wrote just before I left the chairmanship; it was not even orally acknowledged and thoroughly ignored. I recommend that so-called university to nobody although honest men might learn much about corruption at various levels by urging a thorough study by critical outsiders and honest auditors of how the so-called overhead research funds and other outside support is distributed...sometimes not without expecting favors instead. But let this end the long jeremiads and get to the points promised.

Our final problems in America seem to have started in 1968, when in the spring I received a letter from the National Research Council asking if I could accommodate for a few weeks an exchange scientist from Ljubljana in Yugoslavia, Dr. Franc Sušnik, who was then the director of the Biological Institute of the University there. Although I was busy this summer with some graduate students who were striving towards completion of their theses, with other administration, my own research and, especially, with writing the new Icelandic excursion flora that was published two years later, I consented to try to assist him. When he arrived, he told me that he had mentioned my name because I was one of the less than handful American botanists known in his part of Europe but mainly because of my renown as the leading cytotaxonomist, a field in which he and his colleagues were especially interested.

for various reasons. I soon discovered that he was familiar with the cytological and taxonomical techniques that are basic for this approach, and also with the cytogenetical philosophy that is essential for the logical interpretation of its results. He took on a small problem with Rocky Mountain fescues which we had been looking at, but time did not allow him to complete it. A good proportion of the days and evenings that we spent together was, however, used for discussion of problems in the Yugoslav and Balkan flora that were likely to benefit from a thorough and extensive cytotaxonomical investigation similar to those that we had previously completed for Scandinavia, northwestern Europe, Iceland and Mount Washington in New Hampshire, and were working on on basis of immense collections from Manitoba, the Arctic, eastern Canada, the Rocky Mountains, southern Spain and northern Italy. As a matter of fact, when we were planning the two last-mentioned projects in 1962 by aid of substantial support from NATO, we had first planned to locate it to Yugoslavia with all its problems related to so-called endemic species and genera that had been described during the time of dominance of the Vienna-school philosophy in taxonomy that still dominates among some American and Russian botanists but differs significantly from the classical and evolutionary concepts of the Scandinavians and Germans, which we accept as do most modern botanists. Although much preparation had gone into this project through discussions with our Ljubljana colleague Professor E. Mayer and by aid of detailed studies of the literature and herbaria, political problems interfered, so our collections in 1962 and 1963 were instead located to Spain and Italy, and even to the southern Alps close to Slovenia. We were, therefore, thoroughly familiar with the botanical problems of this region.

Our discussions resulted in the conviction that a joint project on the cytotaxonomy of the Yugoslav flora, or preferably the Balkan flora as a whole, was likely to solve many of the crucial problems in European taxonomy and phytogeography and clarify some of the basic processes of local endemism anywhere. When we entered the problem of the possibilities of funding such a project and I explained to Dr. Sušnik the situation we had encountered at the NSF with its hostile anti-cytotaxonomical so-called peer reviewers and also the unlikeliness of convincing NATO, despite the success of our Spanish and Italian project. He then told me that while in Washington on the way west he had visited the foreign office of the Smithsonian Institution and learned about the availability of considerable PL 480 funds for projects in which American and Yugoslav scientists could cooperate, and also that still no such proposals had been found to be acceptable to the Yugoslavs, for reasons unknown or not mentioned to me. He promised to make further contacts in Washington on his way back home and to ask that available information be sent to me. To make a long story shorter, I contacted the Smithsonian Institution in the spring of 1969 and in the fall we presented a project which every qualified cytotaxonomist would have looked upon as excellent because it was backed by a wider and deeper experience in the field than any other couple of botanists shared anywhere. It proposed an initial study that we felt we could accomplish in five years, provided of course that the support were adequate and that no obstacles were thrown in our way. I am sorry that I seem to have lost my last copy of this proposal, but it ought to be available from official files at the Smithsonian and at the University of Colorado.

I received no answer from the Smithsonian until on February 11, 1970, when I was told by Mr. Whitehead, the friendly associate director of the PL 480 projects at the Smithsonian, that a panel of specialists (only two botanists, neither one in the fields concerned!) had found our project to be unacceptable for various reasons. He was apparently reading in part from a letter, dated the same day, to our vice president for academic affairs and received in copy by me

a few days later, in which several statements showed that the reviewers were ill-informed on several matters as could be expected by a non-specialist panel. Since I was aware that ~~one~~ of my few real peers in America had taken part in this judgement (and this was confirmed by Mr. Whitehead), despite the hardly honest but conventional claim that "scholars particularly well qualified to comment on the subject matter of the proposal" had been consulted. When I complained about this to Mr. Whitehead, he gracefully offered to accommodate a new application, if it could be presented without delay, and to present it to a group of our cytotaxonomists peers to be selected from a list that I consented to prepare, and then to present it to the Smithsonian panel at its spring meeting and early enough to allow us to start our work in Yugoslavia in the early summer. He also asked me, for peculiar reasons that indicated that our competence might have been questioned by the Washington panel, to put an emphasis on the Julian Alps, an area without problems of the kind emphasized in our project, and to restrict the project to Slovenia. Time did not allow a consultation with Dr. Sušnik, but his astonishment and anger was a reasonable reaction when he was informed. Then he flatly refused to follow these clearly ignorant "instructions", but when I could inform him that Mr. Whitehead had confirmed in a telephone conversation that a stringent conformity with this plan was not expected, this secured its acceptance by Dr. Susnik. At about this time, Dr. Susnik informed me in a letter, that I seem to have mislaid somewhere, that during his visit to Washington in 1968 or 1969, some botanists had pressured him to replace us with them or with some other Americanborn botanists, a proposal which he had categorically declined on the basis of the simple fact that none of these botanists had any real knowledge of cytotaxonomy and its principles nor were any of them familiar with the European flora in general or the problems of the Balkan flora in particular, as we already were. Later in the spring I was told by Mr. Whitehead and by a copy of a formal letter that the new proposal had been accepted because of enthusiastic recommendations from reviewers selected from my list, I believe they were Art Cronquist, Verne Grant, Harlan Lewis, John Reeder, and Billie Turner. However, because of "lack of funds", it could not be funded downright, though hopefully later in the summer, so I was encouraged to keep our student assistants ready. Nevertheless, despite of my efforts to pressure the office so the work could start before the winter would prevent all collection of plants in the Yugoslavian mountains, I gave up after July. This I reported to Dr. Sušnik, who knew about the availability of plenty of funds that had also been officially approved by Zantes, the Yugoslav Research Council, and then also told him that the Smithsonian official whom I had spoken to last time had even indicated that funds might not become available until perhaps in 1972! I have copies of several letters from this time that indicate what was going on, evidently in order to discourage us so that Smithsonian "specialists" might be pressed into our slots whatever the Yugoslavs would say, but I doubt that you or others would find them to be worth your while. They are a disgusting evidence of negativism and downright discrimination.

The Yugoslavs were not happy with this situation and seem to have remarked on it to Washington through their diplomats. Therefore, I was asked to visit Ljubljana late in 1970 to correct some misunderstandings, an indirect answer to an ignored application from me and my wife to visit the place in order to gain proper knowledge of its problems from the point of view of our finances. I believe this complaint and this visit resulted in that our project was at last funded in the spring 1971, though with last-minute reductions in our part of the budget which I have later understood as attempts to get me to withdraw in disgust. But we at last went together with three students, one of whom was our younger daughter, who had been assisting us considerably in bibliographic work fundamental to the investigations to be started in Ljubljana.

We worked intensely in Yugoslavia during two months in the summer of 1971 under very unfavorable conditions, but could nevertheless in the fall present a report of results that even we had a good reason to be proud of, including papers in the press. Therefore, we were in no doubt that our funds for the next summer would be gracefully approved. When we heard nothing about this, however, we asked the Office of Research Services at Boulder in the second week of March to enquire about this with the dean of the graduate school and the provost, and when the latter denied having received any information, we asked the ORS to contact Washington. That resulted in a copy of a letter that had been sent to the provost early in February, without a copy to me (but he forwarded a copy late in April or early in May!). This letter included not only a negative answer but a highly damaging report, as you can read in exhibit 4. We later got the information from Billie Turner, that Paul D. Hurd, a Smithsonian entomologist, had told him that this report had mainly been based on his judgement, whereas Harlan Lewis, the only one of my peers who seems to have been consulted, told me, as I recall, that he had been asked to review our report and had sent in a very positive and enthusiastic review, which, however, apparently was ignored by the Smithsonian committee. If this is not a misuse of the peer review system of which American scientists have been so proud and a case of a highly malicious libel that in addition was sent to my ignorant superiors with the intention to hurt me and without even informing me by aid of a copy, then I am badly mistaken about the protection of even immigrants by the laws against slander and libel...even by those who like to hide behind so-called anonymity of so-called peer reviewers who are no peers.

I have the feeling that the Smithsonian officials intended this report as still another means to get me to withdraw in anger from the project so that their cytotoxonomically ignorant representatives could step into my project and try to utilize my ideas whatever our Yugoslav colleagues would say. If so, it certainly was a serious blunder and miscalculation not to send me at least a copy of this libellous "report", because I could hardly react in anger to something that I did not know, and the ineffectivity of the Boulder provost also prevented me from answering at once. But the interference by the Office of Research Services coincided with a letter from Dr. Susnik, who seemed to have some information that I did not have in the middle of March, and he urged me to come during the summer and not to give up the project whatever "decisions" the Smithsonian might make. When I met Dr. Susnik in May, it became evident that when Mr. Schmertz had not received my expected angry answer in the middle of March, he lost his patience or felt that some other action must be taken to secure my withdrawal, because then he visited the scientific attaché of the Yugoslavian Embassy to ask him to directly slander me in Ljubljana. I enclose the letter as my Exhibit 5, and it would astonish me if you and other honest scientists do not agree that it contains even a greater malicious libel than the former letter. With the copy of the original, in Slovenian, I enclose a translation done by Dr. Susnik, and also of the part of his letter that answers this request; it shows clearly that he was not flattered by the much too evident cajole and that he was not going to do as the Smithsonian non-botanists recommended to replace me with "a better American scientist". Naturally, when he gave me a copy of this letter, he was greatly disturbed and could not avoid to make the remark that if an official in his country had done something like sending such a letter abroad, even using such methods inside the country, he would not be able to do it with impunity, and his institution would at least have been forced to pay substantial damages. The same would be true in my country. You will probably react similarly to Harlan Lewis (exhibit 6) to whom I sent a copy of this libellous letter after our return home in the fall.

After the letter (exhibit 5) had been sent to Ljubljana and the negative answer from Dr. Susnik had arrived, and after I had received his letter urging us to come whatever the "report" might be, the Washington received the request from the Office of Research Services at Boulder for readjustment of our budget (exhibit 7). It is my suspicion that then Mr. Schmertz may have been seized with some misgivings about the possibility that I might already have received from Dr. Susnik a copy of this second libellous letter and that this clearly unwise attempt to force our withdrawal might cause us to ask for legal assistance against the libel at least; if so, that must be prevented. Be that as it may, but it would not astonish me if at this point those who were responsible for the two letters have realized the seriousness of the situation and that now their only possibility for a successful evasion would be to get us to Slovenia where a new plan, based upon a frame-up and entrapment, could be put into action to produce a watertight cover-up for them and also scare us to withdraw and to keep silent. For such a plan, however, our presence in Yugoslavia would be essential. I believe that this is the simplest logical explanation of the fact, that suddenly there is no need to refuse us any funds but instead our original budget is gracefully reinstated and all our wishes fulfilled so that we could depart with three assistants immediately after school ended in the middle of May. We were even given permission, per telephone and without the slightest objection, to route two of the assistants over London for a visit to Kew and to go ourselves via Portugal to take part in the Flora Europaea Symposium at which we could confer with numerous European colleagues about our project. We did not have the slightest suspicion of this sudden and complete reversal and so accepted it gratefully. However, when we arrived in Ljubljana a week or so later, we found that the initial step of what I believe was a wicked plan had already been taken, when the Embassy, through its scientific attaché, Dr. Liimatainen, told me on the telephone that no authorization had been received from Washington to release any of our funds. The Yugoslav officials, who also had to be involved, were, however, of the impression that no such new authorization was necessary because these funds had been authorized in 1971 for a five year project. Dr. Liimatainen claimed the following days and weeks first to have been unable to get any answers from the Smithsonian to repeated requests for such an authorization, but later he blamed Zantes, the Yugoslav Research Council, for the delay, among other evasions that to me looked as if they were connected with the action asked of but refused by Dr. Susnik (exhibit 5) of which the Embassy likely had some knowledge, directly or indirectly. The funds were at last released at the end of June, through the Belgrade Bank, as verified by a form (exhibit 8), but my bank receipt was among the material of which I was stripped at the end of the summer, as described in exhibit 10. The details of the first part of this plot as it was carried out are described in my letter of July 2 to Mr. Schmertz, in which I informed him about what had been happening, and, thus, followed his instructions as to reports of any changes in the budget that were required in the field. That letter is my exhibit 9. I mention this again, together with informations on the continuation of the plot until we left for home in the fall, in my letter of September 4 to Dr. Susnik, which is my exhibit 10. I am sorry that the contents of neither of these letters can be properly corroborated except as copies made by me at their writing, because I was stripped of all vouchers and evidence, even my personal notebook, as described in the letter in exhibit 10, and also of all my receipts, even our food receipts, that were later replaced by incorrect receipts based on what I had also been forced, by threats, to assist with, as described more closely in the letter in exhibit 10. The receipt of these letters was never acknowledged, and they were nor registered, cf. below. In the letter to Susnik I mention a phonecall from Mr. Bađovec (or from a person identifying himself so, cf. below) on July 20, when he instructed me as to the final phase of the process; I enclose my handwritten notes of this as exhibit 11.



The story related in the letters in exhibits 9 and 10 may read as a script for a detective novel when scrutinized by an honest scientist, but they certainly created a nightmare for me that summer and ever since, with a continuation worthy of the KuKluxKlan. As I mentioned in the letter to Mr. Schmertz (exhibit 9), I mailed it the day after it was written from Klagenfurt in order to avoid the curiosity of the Yugoslav and USA secret police letter openers. It was, unfortunately, not registered since an innocent and nervous writer does not think that such a mailing might later need verification, especially because in it I was reporting happenings that I had reason to believe were not entirely unknown to the addressee himself. But my travels to Austria that day are documented by a stamp at the border in my passport, whatever that may be worth in a land of lawyers and auditors. The later letter to Dr. Susnik (exhibit 10) was also sent indirectly, through the address of an old colleague at the University of Graz in Austria whom I asked to deliver it to its Yugoslavian address without using the mail over the border. He sent me a card confirming the acceptance and delivery of the letter of which I made a note in pencil on my copy before I destroyed the card as promised. Unfortunately, Professor Widder, the intermediary, died at an accident in 1974 and so is not available to confirm my words. Even my wife and daughter were unaware of these letters because I made efforts not to upset them with information about what was going on, in part because I was being considerate and in part because of threats, so the claim that these letters were actually sent rests only on my two copies and on my Icelandic word of honor. It is, therefore, possible for others to explain the exhibits 9 and 10 differently, if my word is not accepted.

I want to emphasize in connection with what I reported in exhibits 9 and 10, that I am unable to speak Slovenian or Serbo-Croatian so that I needed help to use the telephone for calls outside Ljubljana. Since only two of the office staff seemed to know some English or German, Dr. Susnik instructed them to assist me whenever I received an outside call or needed to make one myself. The names of these two persons, I never got straight since I never saw them in writing, and they have later become only shadows of a young man and a young woman in my memory. All my long distance calls were, naturally, recorded in my notebook, with details of the conversations, but since this book was among the material requested by what I believe was the Embassy as told on the telephone (exhibit 10), only a fraction of these calls remain in my memory. However, the letter to Mr. Schmertz (exhibit 9) was written with support of this notebook which then was still in my hands, and the letter to Dr. Susnik (exhibit 10) was in part based on my copy of the letter to Mr. Schmertz of which the Embassy had no first-hand knowledge and did not mention at the time of its instructions shown on my notes from the last conversation with the voice introducing itself as Mr. Barlovec.

The two office staff members mentioned were my intermediaries for numerous calls, both those coming from the Embassy and the others from me, and they always told me who was on the phone, except in the few cases, mainly in the early summer, when either Dr. Susnik phoned from his office or received calls there, usually from Dr. Liimatainen or Mr. Barlovec of the Embassy, and let me talk to them. Therefore, I want to emphasize that I never met any of the persons at the Embassy in person and so would not be able to identify them if I met them or to claim emphatically that they were more than names given to me by themselves or others on the telephone. If I had had any reason to suspect foul play, however, I would, naturally, have made efforts to identify everyone in writing by Dr. Susnik or others. There is an exception concerning Mr. Francodruz, whom I met and exchanged funds but no documents with at the instruction of persons identifying themselves on the telephone as staff members at the Embassy; when I met him and

asked if he was Mr. Francodruž with whom I had just spoken on the telephone, he confirmed this with a voice that I recognized and also with a smile that later (cf. exhibit 10) got me to suspect that the name might perhaps have been fictitious, since I believe it could simply mean friend of Franc. He was already an old and frail man in 1972, so he may not be alive. I must also admit that sometimes later I have thought of the possibility that all the phone calls that were said to be from the Embassy and with which I was assisted by the two staff members who spoke some English and German in the office could as well have come from within the city of Ljubljana, even from other rooms in the same building. Then, however, the person said to be Mr. Barlovec must have had an unusual ability to imitate his voice if it in fact was somebody else, because I never doubted that this was the same person as Dr. Susnik had introduced me to on the telephone in 1971 and in the spring of 1972 when it unmistakably came from the Embassy in Belgrade. That such a person existed became evident when he one day in the summer apparently visited our assistants in the Botanical Garden without visiting us, and when the girl who was our assistant visited him in Belgrade later in the summer, as I believe I mentioned in the letter to Susnik (exhibit 10). The fact that I felt I recognized his voice with certainty was a strong reason for that I never doubted that the other introductions were genuine, which I still believe, and I have no reason to suspect foul play by my Yugoslav colleagues although others may want to suggest that on basis of what is said above. Nevertheless, this possibility for doubt makes me hesitate to send a copy of these two exhibits even to you, because I do not want to implicate or hurt anybody or to accuse somebody unknown to me on basis of such an evidence alone. Therefore, I trust that you will not copy these two documents and only allow others to read them under conditions when they could not be copied or xeroxed for distribution or other misuse.

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There is perhaps a need to explain that when I sent the letter and copy of exhibit 5 to Harlan Lewis in the fall of 1972, the story related in the letters in exhibits 9 and 10 had already taken place, so that I could have told it to him also and added copies of these letters. At that time, however, I was nervous because of the threats mentioned and perhaps especially because of a special one, not mentioned in either of the letters, that had been distinctly expressed at the end of the conversation at which I received the "instructions" in exhibit 11: when the speaker had completed his conversation, he said goodbye and added hurriedly before putting down the receiver: "remember that the long arm of the CIA will reach you anywhere if you squeak". I now realize that this and other threats may have been empty pranks though to me they looked serious, and that it might have been wise to break this at that time at least, since Harlan Lewis would certainly have protected me; if he had known about this all, he might even have prevented the most unbelievable part of the scheme that still had not been put into action. And I also thought that being obedient and silent I was protecting my family, my constantly first thought...this deliberate fool, Shakespeare might have said about me and my innocent simplicity.

In my letter to Dr. Susnik (exhibit 10), I described the last part of the plot in Yugoslavia when, by aid of a threat especially serious in the mind of a Foreigner who has discovered that even in his role as an immigrant supported by American funds he is clearly unprotected in a dictatorial police state, I was stripped of most of my documents by those that I believed were my protectors. This included even "duplicates" and unsigned papers that I was "required" to include for "reasons" that look doubtful at least, but if you can put you into my situation, you certainly realize that I had no selection but to conform, if I wanted to get out of the country with my family...provided that the threats were real, as I had all reasons to believe. When we were leaving, a few of

these papers were returned to me with the "instruction" not to open the envelope until at home. Most of the documents and vouchers and my important special notebook were missing, and those that were included had been "doctored" and furnished by signatures by somebody somewhere, and with them was enclosed a typed note (exhibit 12). As mentioned above, I sent this letter through an Austrian intermediary who informed me about its delivery, I never heard about it from Dr. Susnik, but received from him some noncommittal letters that answered questions concerning our mutual research and the papers we were preparing or asked about his final report that I felt was important for our closing of the affair. These letters were sent by ordinary mail and meant for his files in Ljubljana, although I had enclosed at least with one of them a handwritten note carefully asking about the September 4 letter. I received some unpleasant phonecalls from Washington demanding that I immediately report on our summer's work and especially on the finances of the project, a request that was contrary to my instructions because such reports including the Yugoslav part were hardly due until at the end of the year. When no answer arrived from Dr. Susnik, however, I at last decided to comply and to follow slavishly the "instructions" (exhibit 10, 11 and 12) and their supporting threats by what I had no reason to doubt were the officers at the American Embassy in Belgrade, trusting that they had kept their promise to "explain everything" in a way satisfactory to Washington.

I heard nothing in the matter for some months, but at about the same time in February 1973 I received an alarming and upsetting letter from a much worried and angry Dr. Susnik (exhibit 13) and a phonecall at home from a Smithsonian auditor (Mr. Stanton?) who then was at the Office of Research Services in Boulder especially to audit my papers there. I answered the letter from Dr. Susnik at once and asked for more informations (exhibit 14), but never again heard from him, not even a thanks for reprints of our works and for a copy of the large and very expensive Cytotaxonomical Atlas of the Slovenian Flora which I sent to him through the publisher in 1974, the monumental and main result of our joint efforts. ( Here I want to add that we sent the same package of reprints and the book to the Smithsonian as our final report early in 1975, addressed to Mr. Schmertz but not registered; its acceptance has never been acknowledged!). The auditor asked me in a tone of a far from friendly one who believed he had the power, where I, yes, I, had placed a microscope that had been bought for the project in 1971, and I could not avoid the impression that he was implicating that I personally had "taken care of it" ! He calmed down and seemed satisfied when I explained that this apparatus had been paid from the Yugoslav part of the budget where it had also been listed in our original application, and that it therefore had been left in Ljubljana in the University Botanical Garden laboratory. I asked him if there were other matters or problems that I could help him with or needed to explain and offered to come to the ORS despite the bad weather this day, but he said that there was no need for that, everything was under control. I had, however, almost asked him about the matters mentioned in the last letter from Dr. Susnik, but decided not to expose mine and his worries to an outsider. Little did I realize that he was in Boulder because the Smithsonian bureaucrats had decided to go ahead with a final phase of their plans that we had seen a little of in Yugoslavia so they could get rid of us and our project without contacting our peers but instead by implicating me by using the fabricated and reconstructed "evidence" by aid of auditors and other non-scientists who could help to discredit me and accuse me in such a way that the coup-de-grace could be trusted to an administrator at the University of Colorado. Still less did I imagine that political, not academic, bureaucrats of this putatively most respectable of American learned institutions who repeatedly had niggardly declared lack of funds for our fine research project that they despised for political and discriminatory reasons, now suddenly had uncovered plenty of money to send auditors and even

high administrators by air not only to Boulder but also to Yugoslavia to pinpoint on us irregularities that seemed to have been known to them before we experienced them already in the spring of 1972 and seemed to have been carried out by Embassy officers simply to force immigrant scientists to withdraw from a fine project that some unimaginative botanists at the Smithsonian were interested in pursuing, without proper knowledge. It astonishes nobody that these emissaries found everything wrong on our side of the project and that they returned with "affidavits" supporting their "evidence", as is typical in this land of lawyers and bookkeepers, neither did it astonish us that these same investigators declared that everything was in good order with the books of our Ljubljana colleagues - despite the fact that one of the main "cooperator" had done practically nothing for the project except accepting his additional salary, so that the more honest of the cooperators even complained to me about this in 1972 and threatened to report this to the rector of the University. Also, the auditors did not observe that several persons were listed as having received salaries from the Yugoslavian part of the project although we had not seen them and therefore did not know them or their "results", not to forget that during the second summer hardly any work was done by any of the Yugoslavs, who even delayed the work by our students by demonstrating unwillingness in helping them collect material for cytological studies and delaying or refusing to give them informations about material that had been collected previously. Instead of asking the two students about such matters that really were connected with the work contracted for, they were, willingly or unwillingly, given the opportunity to write affidavits against us that mentioned nothing of our ardent work for long hours during the summer. My observations here may indicate that lack of doing work contracted for and paid for is not an auditory sin and perhaps more in line with that done by others than us, who always have felt responsibility to do more than promised, irrespective of the possibilities of reward. And the auditors apparently had no interest to ask about how much time and efforts we had spent on this large project in Boulder without any remuneration from the Smithsonian funds. Why bother about others?

On basis of the highly superficial and unjust so-called "investigation" by the auditors, who had not even been informed of the details in our budget (cf. above regarding the microscope) and who certainly had not been told anything about the several earlier attempts to discredit us as scientists and to force our replacement with incompetent "better American scientists", not to mention what went on in connection with our funds in Yugoslavia during the summer of 1972, I was soon presented, through the University that apparently was given responsibility for this last phase, with a "report" (exhibit 15) that a considerable sum was "disallowed" from my financial report and I required to repay it without explanation, despite the fact that I had followed the albeit crooked but clear instructions from what I still believe was the Embassy staff and also despite the fact that we had and would soon deliver much more than ever contracted for. I could not believe that this letter could be serious, since I was not even warned about my rights to legal counsel, and since I still was under the impression of the threats in Yugoslavia, that seem to have affected my stomach ulcer ever since, I did not feel that I could even try to defend me by informing the provost of what had happened there for the simple reason that I knew from my experience that I could not trust him not to use this to hurt us more and also others. I asked, however, that the auditor of the ORS be instructed to review the audit with me and believe I sent a copy of the vicious letter from Washington to Dr. Sušnik (exhibit 5) as information of what had been going on since then. This letter was, however, not even acknowledged. Later I have wondered if this procedure actually could have been caused by some bureaucratic vigilance or simply by a mistake, or it may have been a bold attempt to find out if the threats

still would keep me from "squeaking". Irrespective of any threats, I would have kept silent about the major cover-up activities for at least five years and never revealed names to a person as little trustworthy as I had experienced those here involved because only in that way I felt certain that I would not hurt the Yugoslavs whatever their role may have been. I do not think the Washington organizers of the witchhunt had planned it that way originally, but everything went out of their hands when they sent their letter to the administration of the University, because then the culmination of the case was secured to become a most serious miscarriage of justice when it was taken over by the most unscrupulous and least competent administrator I have ever met during my more than a quarter of a century in America, a man who had wilfully discriminated against us for years and done everything in his power to obstruct even our research. Instead of disconnecting himself from the case because of evident partiality, he did not hesitate to appoint himself as my judge in a kangaroo court based on the old Colorado "law": "let us give him a fair trial and hang him", except that here even the trial was absent, and, naturally, there was no hesitation in violating my civil, academic and human rights...this was during the "law-and-order" period of American justice. When this unscrupulous man ordered me to come to his office on May 24, he avoided to inform me about the seriousness of this meeting and also about the actual matter it concerned, and did not advise me to bring a counsel, not even a neutral or friendly witness. He asked me at once dryly to sit down at one side of a table which was cluttered with irregular heaps of books and papers, and then left for a moment for the next room the door of which seemed to be permanently open. I believe I heard from that room a low click as if a tape recorder had been started (I am familiar with that equipment since many years ago). Under the table I observed a thin wire that led in the direction of the other room, though the door was hidden by a shelf, but since I had no time to lift the books or papers, I could not see for sure if there was a microphone hidden amongst them. I did not return it however, I am still convinced that our entire conversation was taped, without my knowledge and permission but for his possible protection and edition, as I told my friend Jack Ives later that same day.

When both of us had sat down at the table, one on each side, the provost, Mr. Lawson Crowe, became very formal, handed me a copy of what I have shown here as exhibit 15, became very formal using my academic title and name, and spoke clearer and louder and accused me at once of "multiple counts of fraud against the government". I tried to show him the libellous letter (exhibit 5) to get an opportunity to show that this was a part of a long operation to remove us from a scientific program in favor of Smithsonian "better American scientists", but he refused even to listen and said that he had succeeded to get copies of numerous affidavits that had been taken from my student assistants and some people in Yugoslavia in support of this claim. He held up some papers that he claimed were these affidavits and waved them towards me, but refused and avoided to give them to me or even allow me to look at them. He also said that if I did not agree to pay the sum of \$7575 at once, he would return the matter immediately to the Justice Department which, as he let me understand, already had made preparation for my immediate deportation. After what I had encountered myself and heard about the experience of some other immigrants in this country, this did not astonish me though I had almost asked the question how he would himself assess a judge who bases his highly arbitrary decision on only a very onesided part of the available data and refuses to listen neutrally on information contrary to his preconceived belief; but since I had seen that even American scientists used the same method to judge scientific opinions in which I and other Europeans had differed from them, e.g., glacial survival, continental drift, frequency of polyploids, speciation, etc., etc

I refrained from irritating this less wise man with such a question and kept it to myself. Instead I said that rather than to hurt the students and the Yugoslavs by the action he had indicated, I would agree to pay the sum mentioned without further discussion. That meant that the part of the salaries we had saved from two summers in Yugoslavia now would be reduced to less than nothing for our full two years of ardent and industrious work that had mainly been done at home during our unpaid extra hours in Boulder. I am not a lawyer, thanks for that, and so I did not realize that the false implication of admittance of so-called guilt could be drawn from this action by American lawyers using their peculiar kind of "logic", but understood later, when viewing the Watergate discussions, that this must have been a calculated entrapment with help of a threat and my ignorance as an immigrant from a more democratic country, where such an answer could in no way be so explained...and in my opinion I had admitted nothing. But as soon as I had given the provost this promise, he requested that I immediately resign from my position at the University, threatening at the same time that he would instruct the regents at their meeting next day to dismiss me if I did not comply, adding that I could later present this to his grievance committee or to the court...a peculiar proceeding from the point of view of an Icelander. I asked if this was not too much asked on too doubtful a basis, and then if it were not advisable that I got a neutral legal adviser, but was told that a further discussion with a lawyer was not needed because he, the provost, had already discussed the matter with the president (who soon was to be fired because of "incompetence") and he also said that the legal counsel of the University agreed that what was being done was the correct procedure, and he mentioned the word deportation again. So it may be in Colorado - but certainly not in any really democratic place and I even doubt that it is a proper and fair method anywhere outside the administrative offices of the University of Colorado. I received once more that the hearing with the regents and instructions and pointed as I had found this person to be earlier, so I consented to his new request also, and even accepted that he be trusted to compose the papers to be signed. In my diary that evening I wrote that I felt this was a relieve after years of nothing but discrimination and miscarriage of justice and that now I hoped that the "better place", which my colleagues Cronquist, Lewis and Turner had indicated they would help me to find from this "stepping stone" into the States, would at long last materialize and open its doors to us. Perhaps I would have fought and even avoided trying to protect the students and the Yugoslavs if I had known that even these colleagues and friends of mine would turn chicken and do nothing for me and not even acknowledge my letters asking for their help for five long years? The provost, however, said that since I did not "make fuss", he would see to that the resignation papers guaranteed me "all employment benefits" and explained that these would include severance pay, accumulated sick pay, and, especially, so-called University supplementary annuities and medical benefits and emphasized that this would be full and unreduced, whatever it meant. All this I jotted down on a piece of paper at once and wrote it in extenso at home in the evening and also the remark that the request of resigning probably was based on his assumption that I had admitted "moralic turpitude", a peculiar criticism coming from such an amoralic man. But such are people.

The same day as I had visited the provost, I told my colleagues Hobart Smith, David Rogers, and Jack Ives about this event, mentioning at least to both the latter that I was convinced that my conversation with the provost had been secretly taped. Rogers showed little interest, not only because he was soon to leave for Rome, but he proposed that he should phone to Marlan Lewis to ask for his help and advice. That he never did, however. Smith was evidently shocked and afraid, and showed next day and later that he had rapidly turned into a chicken.

Or what else can explain that although he first reacted by wanting to secure that my date of departure be delayed until after the summer school, for which he said dean Briggs of Arts and Sciences had told him there were precedents, he rapidly backed down from this when the provost refused? When Smith phoned me the next morning, just before I had to leave to deliver the cheque to the provost and, as I believed, sign the papers that I expected him to have completed since he had indicated that outside forces were in such a hurry to see me removed, he asked me for a message to the provost regarding my participation in the summer school, but I took the opportunity to ask him, as my chairman, to come with me as a witness and adviser. He became obviously disturbed and had difficulties explaining why he could not do this, his strongest argument being that he was not fully dressed at the telephone! Although I was disappointed, this reaction did not astonish me, because I had long since observed that although this industrious and certainly eminent scientist is basically a man of some fortitude, he lives with an enormous inferiority complex apparently caused by an unhappy childhood and, unfortunately, because his innate kind-heartedness prevents him from protesting, is sometimes forced to do contrary to his convictions due to dominance by a ruthless and tyrannizing wife. Jack Ives, however, became very upset and wanted me at once to get a lawyer and to refuse to sign any papers because the promise had been given under duress and without even a recommendation that I seek some counsel; however, I told him that that since it has been my principle never to break any promises, not even when given under threats, I would sign, but also gave him a permission to look for some lawyer the advice of whom I could seek when presented with the paper of resignation that would be composed by the provost and his legal counsel. However, when I came to the office of the provost, he accepted my cheque which he said he would cash and then send the money to the Smithsonian after having subtracted a fee for his "services", but told me that the papers signed were not ready and that he would contact me later for the signature. When that time came, on May 31 after a call from the office of the provost, Jack Ives had not succeeded in finding a lawyer of his liking, but instead he insisted that he himself be allowed to come with me as my adviser and witness, evidently because he realized that some foul play might be involved and because his English conscience clearly let him believe that the American doctrine of innocent until proven guilty was being violated though it ought to be valid even for immigrants in Boulder. Since I knew that he would more easily than I be able to spot any attempts of encroachment although I suspected none, I accepted his offer with delight. I had known him for many years in Canada, affected some of his research into the geology of Pleistocene plant refugia in the northlands, had even been instrumental in getting him to Boulder, and had always looked upon him as a friend as close as my brothers, so I must admit that I had expected this reaction from him, though I would never have asked him for such a favor. I was sorry that another of my colleagues and friends, William W. Weber of the Museum was absent when I tried to contact him, because I at least hoped that he would react in a way similar to that of Ives. He had been instrumental in getting me to Boulder and together we had, in 1975, proposed the need for establishing two separate projects to revive American taxonomy, a compilation of a Flora boreali-americanana and a computerized data bank for plant taxonomy. Some others might also have reacted similarly if I had contacted them and given them an opportunity, at least Jane Bock, Erik Bonde, Willy Segal and Paul Winston who showed us understanding later but otherwise seemed to follow the advice of the provost, dean and Smith not to discuss the matter with me. But I am afraid that most of those others who had benefitted from my chairmanship might have shown a strength similar though not as pitiful as a small zoologist of little standing for whom I had done more than for the others together and even accepted as my associate chairman despite my understanding of his limitations: when I a few days after my resignation was in

our neighborhood foodstore and turned into an aisle, I saw him at the other end rapidly returning when he observed me; in order to check if he really was avoiding me, I also went over to the next aisle, and he repeated the evasion - lower no man can sink.

Let us return to Jack Ives and May 31. It was comforting that he was with me when I met the provost outside his office at 4 p.m., but I was disappointed when the provost denied my request to bring him in with me as my adviser when I should sign the letter that the provost told me was composed by himself and the legal counsel of the University alone and had not and would not be shown to any others (exhibit 16). I later understood the seriousness of that refusal which was not only equivalent to a refusal of a counsel when one was needed and recommended by law and by the laws of the regents of the University but also prevented me from having a witness to the explanations of the paragraph that the provost said was to secure me "all university benefits" mentioned above that the provost again pointed out specially with the same explanation as previously. Perhaps most importantly, this denial certainly was a refusal of counsel to a foreigner who did not understand cunning selection of "legalize" and had no notion of the importance of having the support of a neutral witness or a lawyer in a "land of law" of the kind that I, and many other immigrants, was experiencing from less scrupulous men who were not my friends. I got a still better understanding of the seriousness of this refusal later, when two of my real friends tried to help me find these benefits by contacting another officer of the University, a very recent law graduate woman who had rapidly advanced as a professor of law to a lower administrator and then to vice president in order to allow the University to fulfill requirements for women and minorities on the faculty and in the administration to hide a rampant discrimination. This administrator said that she had discussed the matter with "all concerned" with my resignation (if this means others than the provost and the legal counsel and the former president, does not that indicate a conspiracy of which I knew nothing?) and then claimed, in a letter to Professor Weber, dated February 7, 1977, that "the University has no further obligations to Professor Löve". Dr Weber explained to me that he had been told that this was caused by the fact that I had not retired but resigned, perhaps a blunder by the provost and his legal adviser? Or is it possible that the written promise, agreed upon and signed by the provost and myself, was added only to fool me, or was it perhaps simply a dishonest act as so much around it? My kind of scientific and philosophical logic has some difficulty seeing that my situation could be negatively different from that of president Thieme, who little later was fired outright by the Regents for incompetence, as it was called, except for that he is Americanborn and I am an immigrant from a small and powerless country occupied by America. Does not the entire story remind of some "thousand sly things", to paraphrase Coleridge?

The provost had promised -and forced me to do the same with my signature - that his "public notoriety" be given our "agreement" or its contents. However, I soon became aware that he kept his word less stringently than I did. Already on June 1, I heard from a couple of my colleagues that they had received from Dr. Smith a letter in which they were informed about my resignation and told that I had requested (sic!) that nobody discuss it with me, thus depriving me of their sympathy and help. Evidently, all others complied as expected, that is the reaction which gives the crooks security in chickenland; but he did not show me the courtesy to send me a copy for information. Also on June 1, I received the letter enclosed as exhibit 17 from Dr. Smith; such a letter I would have refused to write or sign with a reference to the old and established fact that civilized men refuse to follow an illegal or immoral order from their superiors



except if connected with serious threats that might also hurt others, of my "acceptance" of the Belgrade "cover-up" actions. Although I was in no doubt that at least some of what the letter requested could be refused legally, I conformed because of my state of mind. Since I was not sure what equipment actually was mine and what really was the property of the University, because I had replaced from my own funds several expensive items that had been stolen from my laboratory and not reported by my students at the time of discovery of the losses (two microscopes that were our property also had disappeared, but they had been insured when I lent them to the Department, albeit below their price) I tried to get in touch with Dr. Smith in order to receive a list of the material in question. However, he clearly eluded me both at home and at the office, and his secretary, Mrs. Owen, had apparently been instructed to protect him from such a visit and also not to give me any information! Although I indicated that there was some doubt regarding the ownership of some of the items, it took a week before the receipt of this equipment was acknowledged, I believe that Jane Bock told me that she had repeatedly reminded him that this must be done, though a common courtesy, but at last I received the note which I enclose as exhibit 18. Even those who sometimes demonstrate fortitude may other times lack a moral muscle.

I would like to make special remarks on point two in the "decisions" listed in Dr. Smith's letter (exhibit 17), because I dare to greatly doubt not only its fairness but also its constitutionality, still more its moral and ethics. Doris, my wife, came to Boulder from Montreal where she had been for eight years associate professor of research with academic qualifications surpassed by no other woman working in American botany or cytogenetics, with more experience in teaching and research and herbarium administration than any woman ever in Boulder, and with more publications in her field of research than any of the great men then on the Boulder faculty of biology. Dr. Pennak, a zoologist-limnologist who then was the chairman of the Biology Department, told us blankly, without being asked, when we came for an interview on our way to the wedding of our older daughter in California in the summer of 1964 (we were not reimbursed even for the cost of motels in Boulder or in Aspen to where he sent us to see Dr. Weber, another peculiar story), that she could have no position in the department because of the University's "nepotism rule", a certainly peculiar and undemocratic rule if it in fact was more than a common bigoty excuse. When I soon after our moving to Boulder received an NSF grant for research in which she was to be my co-investigator as she had been very frequently since 1940, Dr. Pennak first maintained that she could not even be a research associate without pay from the University, but from that ridiculous position he backed down when I insisted (he might perhaps have done likewise if I had protested when he invited me to a marked reduction in salary and position, even refusing my tenure that I had since 1951 in Canada and since 1943 in Iceland, but I felt that was of less importance than getting the position at that time). She not only worked diligently for years after that on various research projects paid with grants that we felt were similar to alms and far below her Montreal pay when a proper salary with similar benefits as others received would have been proper, but although the University apparently did not feel it could acknowledge her qualifications, it had no qualms in accepting the so-called "overhead" that certainly was never used to further her research facilities. Naturally, she also published numerous papers that received wide acknowledgement for their quality and for which the University received credit. She also advised and took care of several advanced and graduate students and even taught important courses, especially at the Institute of Arctic and Alpine Research, without pay and sometimes even at considerable costs to us.

Jack Ives, the director of that Institute made repeated efforts to get her a professorship at the INSTAAR, connected either with Biology or Geography, but dean Briggs of Arts and Sciences counteracted this for reasons unknown. When the good graduate dean James Archer, who had made the promise of such an arrangement when Jack Ives was offered his directorship, was forced out and replaced by his assistant dean, Lawson Crowe, as graduate dean, later provost, at last chancellor, he also prevented that this selfevident action could be fulfilled, also for some reasons unknown to us that I surmise were not only based on the principle of "better American scientist" or that she was a woman, but perhaps also on the fact that she is my wife. I also have the feeling that these two gentlemen, Briggs and Crowe, were the cause of that when the two women administrators, dean and chairman, at the Denver Center, at which she and I had helped considerably at various times, wanted to offer her a vacant position there, this was also thwarted. ~~Then~~ Briggs even tried, though with evident doubts, to get her salary on our second budget for Yugoslavia lowered from the far from adequate salary proposed, so that it would instead conform with the unfair treatment she had experienced previously, although he must have realized that this summer salary actually was her full year's salary because no funds were available for our work outside Yugoslavia though it would be the essential part of our work on that project; the then graduate dean, William Love, a physicist, however, supported her (exhibit 19). (I cannot refrain from mentioning in this connection that even I was the subject of a salary discrimination by dean Briggs, since even during my period as chairman, which was a fulltime job in addition to my teaching and research, my salary never reached the levels of the nine months' salaries of my colleagues Pennak, Rogers and Smith, all born Americans but only the last one really outstanding in his field, and the salary discrimination is still greater if compared to that offered those "better American scientists" (or so the administration wanted us to believe) who were hired to lead the so-called Cider department, which was actually the cause of our department to death as a much more attractive and important and financed new biochemistry department disguised under the meaningless name of molecular and cellular biology that works with unicellular life and its biochemistry. But that all is a sidestep).

When I had almost completed my four-year term as chairman of the Department of Biology, and and my executive committee (my invention) proposed to the dean that a committee be established to decide and arrange for my successor. This committee soon seems to have decided, without asking me, that it must find some other, preferably a zoologist of course, to replace me, but although it would have been a simple matter of courtesy first to ask for my interest, which certainly was nil after my efforts to improve the impossible department against its will (cf. exhibit 20), they showed the little wisdom to use foul methods instead. At one of its meetings, Pennak went out of his way to get the support of some of the members by claiming that the dean had told him that as soon as I ceased to be a chairman, Doris would be hired as a full professor. That, of course, was only a smokescreen of the usual dishonesty. When some time later a position became open for a geneticist, a field in which Doris received her outstanding degrees Ph. D. and D. Sc. with great honors and in considerably shorter time than was average in Sweden (cf. explanations in exhibit 21 which is a letter from Professor Bertil Hanström, then Prorector of the University of Lund) which is among the oldest and most respected of European Universities) and a field in which she has published extensively, she was not contacted, but her file at the Department, certainly very incomplete though impressive, was apparently brought forward, without her knowledge, and scrutinized by a committee on which there was only a single geneticist of a much lower reputation than she enjoys. This committee of small non-peers who were looking for a yes-man of their own qualifications had

the stomach to declare her less fit than the formal applicants on basis of the purged file that certainly was insufficient for any such judgement, and instead the position was offered to a pleasant but recently graduated forest ecologist with a very limited experience in genetics. When this was reported to her in a letter, without an envelope, that was put in our mailbox, I saved her from seeing this meanness with which she was once more being treated by those who frequently had received her help, and destroyed the paper - she was then also spared the anger which would have been unavoidable if she had known that her "application" had been included without her, or our, knowledge. I may have become overly sensitive after years of miscarriage of justice by small souls, but I am inclined to believe that this was not done only to get an opportunity to show her despicableness, but also likely aimed towards me by those who dared not kick me directly - although I, the certainly only geneticist of the Department with worldwide reputation, had been carefully kept out of the committee!

Doris had never been interested in complaining about the treatment she received because she is like me in that she does not have interest in fighting for herself when mistreated, although she gladly has fought for others, although it certainly hurt her to see numerous women added to the University Faculty these years, not because of their academic excellence but because they were "better Americans" and frequently connected with some politics. And when the frequent complaints about women and minorities came up for the almost annual whitewash, she kept her grievances to herself and nobody even mentioned her case although it certainly was the most blatant of all cases of discrimination at Boulder and perhaps elsewhere. But she continued to do her good work without remuneration and without official encouragement, considerably more research work than the average at this University, and at an international standard.

After all this and very much more naughty experience at Boulder, the provost and the dean of the faculty of Arts and Sciences and the chairman of the Department of Biology could sink so deep as to downright kick her out of the few facilities she got with her unpaid title of research associate, only because she was my wife and because I had abstained to fight their unfairness. In addition, Dr. Smith must be held responsible for having added to the official paper of discharge the word "resignation" to hide the truth, a mean act of forgery that in a real land of law would hardly be given impunity, but the dean and other officer who signed also must have been aware of the incorrectness of that claim of which she knew nothing until she got her copy of this infamous document (exhibit 22). The explanation was, however, already in the letter from Smith (exhibit 17) when he says that in this way I must be prevented from "access to facilities" - it was apparently so important to keep such a "criminal" away lest the students be contaminated that even criminal methods were acceptable, although I was not known for having broken promises or lied to students or boasted of the easiness with which I could get coeds into my bed, as had one of the high officials involved in my removal. Perhaps moral turpitude is an accusation valid for others than "good" Americans?

Although it was evident that everything had to be done to prevent us from delivering the results of the contract for the Smithsonian Institution which dubtfully qualified reviewers and specialists had wanted to be continued by "a better American scientist", we of course decided to do what we could to complete our work, as we had always done previously whatever obstacles.

The essential part of the Slovenian work was a critical checklist of the flora of this area with references to chromosome numbers known for its species. This work was already proceeding towards its completion by aid of computer methods, but now we had been stripped of the facilities needed. When I asked our colleague David Rogers, who had introduced us to these methods and whom I had saved from the dwindling interest in his work that I had observed at Fort Collins to where he had come from New York, he claimed inability to help us for reasons that I knew were invalid. But our friends at INSTAAR, Jack Ives and Pat Webber, had another understanding of our needs, and the computer center remained open to us, despite lack of University funds to pay for its services. So we could complete our 1241 page large book, the "Cytotaxonomical Atlas of the Slovenian Flora, which we got printed and published in Germany. Naturally, it has been very favorably accepted and reviewed by highly competent European botanists, who probably would like to see the "better American scientists" who felt they could do this more thoroughly - perhaps they were the same as compiled the disgusting list of so-called "endangered" species of plants or had the stomach to print an even more unscientific so-called checklist of the North American flora? Our stabbing in the back by the University authorities with the dagger furnished by the Smithsonian bureaucrats also was insufficient to prevent us from completing several special papers from the same project that were published in respectable European journals, although other papers that we had sent to our Yugoslavian colleagues for printing in their planned new journal *Scopolia* still do not seem to have been printed...we have a reason to believe that that has been caused by pressure on Dr. Susnik, who seems to have left the University in Ljubljana to work with the cultivation of medicinal plants in the country where he was born and where his family still lives.

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and in some court in order to mention that I have refused to believe that even after I thought I had received enough harassment from whoever they are who felt compelled to save Boulder and American botany from my kind of ideas and culpability, we have not wholly avoided their attention. For the first years after this happened, I made several attempts to find another position and applied to places for which every fair scientist would have agreed I was well qualified. Despite the written promise of the University provost as to "notoriety", it became apparent in several of these cases (and then especially in letters from Albuquerque, Winnipeg and Copenhagen that I will not enclose here), that somebody in Boulder had slandered me even more strongly than fine scientists as, e.g., Verne Grant had recommended me. That could be expected. But somebody also seems to have felt that more threats were needed to secure that I would not tell my incredible story to honest and really good Americans who might react unfavorably to the culprits. As early as on June 7, 1973, a Thursday, at about 2 p.m., my telephone at home in Boulder rang, apparently long distance, and a female voice who said that she was speaking from some place in Virginia...I got the impression that the name of the place began either with Mac or Lang, though that may be wrong, it was not repeated...and asked for me. When I had said that I was speaking, another, male, voice took over without mentioning his name, but I got the feeling that I had heard it before though without recognizing it for sure. He then said, as I hastily tried to write it down: "Your decision (or agreement?) not to fight was wise, but the friend who offered you help is in a bind (?) and does not dare. We know that you know Thomas Riha. He renegated (?) so he and the woman he talked to had to go (? leave?). If you do, we will deport you without warning, or... Don't forget the fate of Thomas Riha." Here he slammed down the receiver. I did not even tell this to my wife because I could not believe this and hoped it was an hoax by some students or by some prangsters, but the shock apparently affected me since within a week my old duodenal ulcer acted up, although that

that it had down for many years in the middle of the summer. A month later, at about 2.30 p.m. on July 7, the phone rang and a female voice asked for me. Then I heard a weak click and a hoarse, changed male voice that said twice, as if recorded: "Don't forget the fate of Thomas Riha". Click. A similar call came on September 11 at 12.30 in the morning!, on November 19 at 4.30 p.m., and on December 28 at 11.45 p.m. It continued irregularly until we left Boulder in 1976, the last call having been on July 23 at 9.30 a.m., we left on August 11. It is possible that the calls have been many more but that they were not completed because my wife may have taken the phone when I was not in. But we heard nothing more of this kind again until May 2, 1977 at 2.30 p.m., and then it continued with irregular intervals again, and the last one was as recently as on September 22, although then much abbreviated, cf. below, so perhaps that was only my imagination? I did not tell Doris about this until in the spring of this year, 1978, because I did not want to worry her or my family, but in a letter of May 24 I mentioned this to Bill Weber in Boulder - and received a call May 26, with the same recorded (?) "reminder", followed by a similar voice asking "Why did you tell Bill Weber?" After that the voice has been heard by me three times when it said only "Don't forget" and a click, but I am not sure that the call on September 22 mentioned above was the same since the female voice was not followed by the male one...perhaps she has heard that I was trying to connect a tape recorder? Since nothing has happened, at least yet, since the "voice" realized that I no longer kept quiet, perhaps it has never been more than a hoax or the sick reaction of a bad conscience, who knows? But I felt I ought to tell you this also.

This will have to conclude this much too long and still not satisfactorily organized review of this ugly chapter in our American odyssey. It could easily grow into a book of considerable size in which we could add much that is unsaid about our observations and experience in Boulder, especially much of the kind mentioned though, and also of what we have heard or observed from other immigrant colleagues of the highest competence who have been forced to accept positions secondary to "better American" scientists,...or else... But in all fairness such a book would differ much from this report because despite our negative experience of what we like to look upon as the reaction of small minds of bureaucrats, we also are able to look back to immense memories of great people, magnificent landscapes, and remarkable research that we have enjoyed to such a degree that we have learnt to love this land and its people and forget its hopefully few scoundrels and many chicken - and we know that many really better American scientists would be shocked if we had told our story to them rather than to keep it to ourselves for so long. It is thanks to your reaction to the unbelievable slander that I have at long last written this all down, and I hope you will forgive that I am bothering you with this all. Most of the players in the game are probably unknown to you, although some can be found in the American Men of Science and I could add clarifications of others should you so wish. But I believe the Romans would have described the situation best: Res ipsa loquitur - the thing speaks for itself.

Our experience seems to agree with what Edmund Burke once said: The only thing necessary for the triumph of evil is for good men to do nothing -- but I believe that many of the good men are still waiting to hear our side of the story, since it certainly is not their fault that they have been even more considerate than you and never asked us...how could even our best friends know our story if we have never even forced it upon them and left their questions unanswered? I have the feeling that I know your reaction, and hope others of the many truly good men will react similarly in this land of our dreams - and nightmares. I trust that I will soon hear your first reaction.

Yours ever,