

1991-92 Corr AG 2-0

**II En relación a los temas tratados en general en esta etapa**

**Doce intercambios de la segunda etapa de correspondencia  
1991-92 Corr AG 2-1a, 2-1b, 2-3, 2-14, 2-15, 2-28, 2-29, 2-37, 2-38, 2-43, 2-51 y 2,55**

**entre  
Alfreda Galt  
Entonces Presidente en funciones de la Lifwynn Foundation  
Y**

**Juan Campos Avillar  
Entonces vicepresidente Primero de la IAGP**

1991-92 Con AG 2-1a

INTERNATIONAL ASSOCIATION OF GROUP PSYCHOTHERAPY  
ASSOCIATION INTERNATIONALE DE PSYCHOTHERAPIE DE GROUPE  
INTERNATIONALE GESELLSCHAFT FUR GRUPPENPSYCHOTHERAPIE  
ASOCIACION INTERNACIONAL DE PSICOTERAPIA DE GRUPO

XI INTERNATIONAL CONGRESS  
MONTREAL QUEBEC CANADA  
AUGUST 22-28, 1992



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*a folder for minutes of the Board of AGP to all members of the Board of AGP*

Barcelona, August 2nd, 1991

Dear Otto Kemberg,

It is likely that a few of you have received a complimentary copy of the maiden issue of "Lifeynn Correspondence", with a cover letter introducing and announcing the participation of the Lifeynn Foundation in the Montreal Congress. That is to be the first time that this organization - mother organization of groupanalysis and precursor of most group psychotherapies, family and community therapies - is to present in a World Congress of our kind the "laboratory research in analytical and social psychiatry" initiated by Trigant Burrow and Clarence Shields as early as 1917 - that is three-quarters of a century ago. They first tried to present their work in a congress of psychoanalysts - Bad Homburg, 1927 - but nobody there was ready to listen; five years later Trigant Burrow was ignominiously sacked from the APA, and consequently from the IPA and, mind, he had been "charter member" of both. In 1927 they had to found their own "learning environment" - the Foundation. Curiously enough it was in Montreal, on occasion of the Silver anniversary of the Foundation that in 1949, as former President of APA, he was granted the Abraham A. Brill Memorial Medal, a much delayed and useless honour but no true act of reparation; his work went equally ignored.

A year from now, when we meet in Montreal we will be celebrating there another Silver Anniversary, that of "Group Analysis International Panel and Correspondence" launched by S.W. Foulkes in 1967, known amongst friends as GAIPAC. This Correspondence little by little became more formal, lost his original logo and, to my understanding, could not resist the change of format that took place since its Volume XV/2 of August 1982. Soon before, in Copenhagen, prompted by me, a first step back to the origins - a sort of "Operation Phoenix GAIPAC" - was given in a joint meeting with another of Foulkes' organizations, the Group Analytic Society (London), a big boost for the European Group Analytic movement and the platform from where sprung the work that at an international level we are trying to nurture with the activation of a "network of study groups in group analysis" being sponsored by this Committee.

1991-92 Con AG 2-16 ->

I am sending you in advance the accompanying summary report just in case you feel like joining us at Lisbon working towards activating this Study Group for Group Analysis. In principle, we have in mind two lines of action: to start anew a correspondence - this time multilingual - and to convene a meeting - a thinking party - of delegates of group analytic study groups attending the Congress. Any feedback before that date is welcome. You will be duly informed on the results.

Hoping to see you soon in Lisbon, I wish you a nice Summer,

Juan Campos Avillar

Copy of this letter and enclosures is sent to the President and Officers of the IAGP, members of the Board of Directors, former Presidents, IAGP's Executive Councilors and people signing the original SAGA/GAAS application.

Founder J.L. Moreno, M.D., 1885-1974. Former Presidents: Samuel Hadden, M.D., 1973-1977; Raymond Bategay, M.D., 1977-1980; Massimo Pines, M.D., 1980-1984; Jay Fisher, M.D., 1984-1986; Greta Leutz, M.D., 1986-1989. Incorporated: Swiss Civil Code Art. 60 ff., 1972.

# Report of the Standing Committee on the STUDY GROUP FOR GROUP ANALYSIS

Prepared for the European Meeting of the Board of Directors of the IAGP at  
the Hotel Tivoli, Lisbon, September 6-8, 1991

by Juan Campos Avillar

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The Report will cover three main points:

1. A short history of the establishment of this Committee.
2. The institutional significance of the concept of "Study Group" as an alternative to the establishment of "Sections" contemplated in Article X of the Bye-Laws in regard to the attainment of the general purpose of the IAGP.
3. Undertakings and projects contemplated by this Chair, proposals for the organization of the Committee and philosophy and line of action to be implemented by the Study Group for Group Analysis.

## 1. A short history of the establishment of this Committee.

This Committee was set up on recommendation of the Board of Directors Meeting at Cumberland Lodge, England, on September 3, 1988 and its Chairmanship, later, entrusted to me by President Fern Cramer-Azima on November 15, 1989. It is not clear to this Chair if the role, functions and purpose for the "Group Analytic Study Group Standing Committee" are the same than those suggested by the above mentioned Board of Directors resolution under the denomination of "Study Group for Group Analysis" but we take it they are and prefer the latter, abbreviated SGGA. Since in the specialized field of practice under the denomination of group analysis the question of names and their meanings as they relate to communication are important, I suggest we start by re-reading what has been officially published up to now in this respect.

On page 5 of the Cumberland Lodge Minutes, under the heading "Special Section of the IAGP" it says:

"Juan Campos expanded on his previous proposal for a permanent section of Group Analysis within the IAGP. A Brief was distributed to the Board in advance of this meeting which contained statements from various Board members as to the viability of his proposal. In the following discussion various members spoke mainly of the difficulties that might ensue from a Section formation and that the IAGP remain not partisan. Cramer-Azima introduced the concept of Study Group for Group Analysis, or for that matter a technique for other groups who wish to explore interests in group properties, research, etc.

"The Study Group may be disbanded after a period of time when its function has been completed, or may continue over time. In this format sections are not permanently created in any competitive fashion. Different group theoretical models

may thus exist in loyal harmony within an interactional organization. The consensus of the Board was that the Study Group was a good solution for this and other petitions. Campos agreed and asked that the Study Group be announced in the next Newsletter".

Correspondingly on page 3, of the IAGP Newsletter, Vol. VIII, No. 1 of January 1989 appears the following statement under HIGHLIGHTS: IAGP BOARD OF DIRECTORS MEETING, CUMBERLAND LODGE, ENGLAND, SEPTEMBER 2-4, 1989:

"A petition for the creation of a Formal Section within the IAGP of Group Analysis was put forward by Dr. Juan Campos. It was felt that the creation of a field subgroup within the organization would not be advantageous in view of our small membership. The concept of Study Group for Group Analysis was then proposed and accepted as a viable alternative for members to bond together for a certain period of time for common professional interests."

This settles the question of the SAGA/GAAS proposal: to my understanding it was approved, although under a different administrative category and a new denomination. I think it would be helpful, however, to quote here in full the content of the proposal where the original aims are stated. On May 29, 1987, I circulated among my fellow-members of the Executive Committee a memorandum notifying them that on behalf of a group of twenty-five individual members of our Association, headed by myself, we were getting ready to submit to the Board meeting the following September at the Vrije Universiteit, Amsterdam, a petition to establish a Permanent Specialized Analytic Section in Group Analysis of the IAGP (the SAGA/GAAS project) in the following terms:

"Ever since 1970, and specially since 1980, an important number of group-analysts, individual members of our Association, have been trying to establish an international network of communication in order to further develop the work that in favor of the development of Group Analysis in theory and practice they perform in their local or national societies, institutes and organizations.

"Most of these activities have taken place so far at events coinciding with the World Congresses organized by our Association and/or at the European Symposium in Group Analysis. At this point of history, however, and after serious consideration, the below signing members of the Association believe that the aims by them pursued will be better served under the auspices and coverage offered by a Specialized Section considered under Article X of the IAGP Constitution.

"The Statutes for this Section will adopt... the blueprint designed originally by S. H. Foulkes for the organization of GAIPAC in 1967, that is, the one of a large continuous study group and continuous seminar constituted by a network of operative local groups linked together by correspondence and periodical face to face meetings."

This memorandum to the Executive was accompanied by a personal letter asking their opinion about this project and was made extensive also to Past presidents and other senior members of the Association asking them their honest opinion regard the viability of the project and its appropriateness for the whole of the Association. That was part of the procedure that back in Zagreb in 1986 President Leutz had advised us to follow in order to avoid resistances to its approval. Some of the commentaries received and the whole argumentation justifying the petition were summarized in the report supporting the application before the Board of Directors Meeting at the Vrije Universiteit in September 1987. Other members' response was so positive as for signing the petition themselves. (An outline of this procedure, the draft of the Amsterdam presentation and of the above mentioned Brief have been re-edited and brought up to date and ~~are available for the Lisbon-1991 meeting~~. *accompany this Report.*)

Those are the official texts. They cannot reflect, though, all the work done nor the emotions kindled by the question of "Sections of the IAGP" which extends itself, in our case, as far back as 1979 and, in the case of Foulkesian Group Analysis proper, maybe as far back as before the Vienna Congress in 1967. It is striking, however, that after a long roundabout in the case of the SAGA/GAAS project, our organization came to grant permission for the development of the initiative as originally intended, namely the one of ~~the largest continuous~~ study group in Group Analysis - paradoxically, the same outcome which Foulkes was forced to follow in the previous attempt: **The GAIPAC project.**

Some points of caution before we proceed to the question of Sections in the IAGP. **Firstly**, even though in the above text there is no reference to it, the question of a Section within the IAGP was prompted by events within the European Group-Analytical Movement, initiated at an encounter between "UK-members" and "Overseas-members" of the Group Analytic Society (London) and active correspondents of GAIPAC that took place during the 1980 Congress of Group Psychotherapy in Copenhagen. **Secondly**, regardless of their society membership, the people who sponsored the initiative for a Section within the IAGP, the concept of group analysis they adhered to is not the restrictive one equivalent to group-analytic psychotherapy but the slightly more comprehensive one adopted by Foulkes himself during the First European Symposium of Group Analysis in 1970 in Lisbon referring to the various methods and theories essentially compatible with psycho-analytic and group-analytic assumptions. <sup>1</sup> **Finally**, it has to be considered that many of the people interested in this approach are neither members of the G A S nor of the IAGP but would be likely candidates for the latter in case it included such a specific interest area.

Of course, for people who are not familiar with the history of Psycho-Analysis and Group Analysis and who have not lived in their own flesh the difficulties involved in taking such a step, the actual developments we are considering are not easily understandable. In my own case, my familiarity with these matters and the reflection upon my own experience has helped me to develop the concept of "professional plexus" which I consider my most serious theoretical contribution to Group Analysis.

**2. The institutional significance of the concept of "Study Group" as an alternative to the establishment of "Sections" contemplated in Article X of the Bye-Laws <sup>2</sup> in regard to the attainment of the general purpose of the IAGP.**

One of the obvious facts that comes from reading the whole of literature generated by this proposal is that the main source of concern expressed and the kind of difficulties manifested in accepting the petition for a Section in Group Analysis are more directly related to a matter of principles involved in setting up sections - "...the difficulties that might ensue from a Section formation and that the IAGP remain not partisan" - than with the opportunity and viability of the concrete proposal under consideration as should be expected. What the Group-Analytic Section, unleashed, could have been prompted by any attempt at section forming regardless of denomination, Psycho-Analysis, Psychodrama, or whatever... There is no disguise either that the remedy, the "...concept of Study Group for Group Analysis, or for that matter a technique for other groups who wish to explore interests in group properties, research, etc.", in Cramer-Azima's words, was intended and understood as a foolproof panacea as well as a preventive measure for all future attempts, "... The consensus of the Board was that the Study Group was a good solution for this and other petitions." and "a viable alternative for members to bond together for a certain period of time for common professional interests." (The Newsletter) *January 1989*

It was over this topic, and not about Group Analysis, that the discussion at the Board came to a deadlock. It had very little to do with the temporal or permanent character of the petition for a Section, eventuality foreseen in Article X and easily solved by accepting it on a temporary basis or even after a time of probation. The solution of a Study Group for Group Analysis had the virtue of solving once and for ever a bothering problem without having to amend the constitution. So now that Article X is no longer applicable, if a group of members wants to set up an interest section the only thing to do is to ask for the establishment of a Committee. This "salomonic verdict", unless it changes nothing but names, is unlikely to satisfy neither those in favor nor those against the approval of that particular Section. It escapes my comprehension why that solution, more akin to an ad hoc committee contemplated in Article XI-H.<sup>3</sup>, is more acceptable than to one of a Section, unless it is because the former is under the direct surveillance of the Presidency.

Also, I find questionable the logic behind the argument that we cannot allow for Sections because of the small number of members in our Association and, for the same token, those which claim that "the IAGP is far from reaching the level of organization which would allow for differentiation". Certainly most of the threats to the "integrity" of the Association and of the risk of "fragmentation" are based on these premises. I also ask myself how many more decades have to go by before we reach the number of members and the degree of maturity which allows a safe enough differentiation. Could it not be that precisely those biases and fears are the ones restraining the Association from growing and attaining higher degrees of organization? Are we not precisely forcing people to go out and form their own international organizations, formal and independent of the IAGP, as Dr. Fidler would recommend to Psychodrama and Groupanalysis?

Finally, regard the social projection of our work, I wonder if by avoiding to face those "secessionist" issues, we are not failing as well to investigate a problem highly relevant to society today, at a point of history when, with the cold war melted and the wall of Berlin demolished, states and blocks of nations cannot find a better alternative than the upsurge of nationalists and integristas and as an only response war - civil or world wide - theme very much akin to the one which gathers us in Montreal. Could we - specialists in group living - not find healthier solutions? The Catalan definition of health says that health is a way of living which is autonomous, solidary and joyful, the joy that comes from coping successfully with internal and external problems. Would that do for our group living? Is that what we think of when we talk of Study Groups?

The concept of Study Group has different meanings according to the context in which it is used, and needs further clarification. Could this not be one of the first tasks of such a Committee? For instance, within the psychoanalytical framework, study groups always have been forerunners of societies and, in this sense, the **Psychologische Mittwoch Gesellschaft** of Prof. Freud in Vienna could be understood as one, the same than the **Group of Freudian Researchers** - or Physicians - in Zürich. Even today, the IPA calls Study Groups its Societies in the making, when they are on probation and under the surveillance of more senior societies. That was not the case in the early days of the American Psychoanalytical Association, when direct membership to it was still allowed. A similar situation to the one of the IAGP with IM's and OM's.

Quite on the contrary, within the group-analytic framework, the **Lifwynn Foundation for Research in Analytical and Social Psychiatry** constituted by Trigant Burrow and collaborators in 1927, may as well be considered a Study Group and, we could also consider a Study Group the **Monday Group at 7, Linnell Close**, convened by S. H. Foulkes, where those practicing group psychotherapy in London used to meet from 1946 onwards. Finally, as was mentioned in the application, the model firstly considered by the SAGA/GAAS project was the "continuous large study group", launched by S. H. Foulkes in 1967 with GAIPAC when he no longer could maintain the hope of establishing a Section of Group Analysis within the International Council. Maybe the phantom of that unsettled business is still pending! <sup>4</sup>

In English "study" refers to "*applying the mind as to acquire knowledge or understanding, as by reading, investigating, etc... and also the careful attention and the critical examination and investigation of any subject, event, etc...*", to me all good enough descriptions for the group task of a Study Group for Group Analysis. In other words, an attempt of applying our own special knowledge and skill to the "group settings" where we gather, the same sort of task Hernán Kesselman and myself set out to perform when we studied the consonances, dissonances and resonances between Foulkes and Pichón-Rivière with our Operative Group Analysis (Zagreb Congress 1986), or the definition Fabrizio Napolitani used for Group Analysis as a Training Network in Action (European Symposium of Group Analysis, Zagreb 1984).

I would like to add that my interest in what can be described as a Study Group started in my own country as soon as 1979 in the more senior of our group association, the SEPTG

(Sociedad Española de Psicoterapia y Técnicas de Grupo) which became an Organizational Member of the IAGP as a result of applying this sort of study in its own organization from the 80'ies onwards. For example, the "Colectivo de trabajo grupal Barcelona", while reviewing the group work done in that country during the 70'ies in preparation of the theme of the Symposium of the SEPTG in Mallorca in 1980 on "The Group Approach for a National Health Service", for more than one year constituted itself as a Study Group. Then, in the early 80'ies, we formed **Convergencia Analítica Internacional** and later **Grupo Análisis Operativo**, two professional groups in the making, Study Groups in the proper sense, seeking an international context to develop and which could fit neither into the AGPA nor the IAGP because there was room for individuals or organizations but not for groups. All these experiences are the material from which my concept of "professional plexus" developed and which in turn inspires the creation of <<Grup d'Anàlisi Barcelona>> and the Jornadas Internacionales from which **Sociedad Española para el Desarrollo del Grupo, la Psicoterapia y el Psicoanálisis**, another Organizational Member of the IAGP sprang.

Summing up, as I understand, the aim to be achieved with the Study Group for Group Analysis is to operationally take measures and effectively establish means that favor at an international level the sort of communication and interactions which better serve the development of that branch of science and body of practice that goes under the denomination of Group Analysis. I hope that this definition fits with the one employed by the Board of the IAGP. For Group Analysis certainly this is good enough. After all, the first paper ever presented by the Linnell Close group back in 1948 at the London Congress for Mental Health was "The Study of Communication in a Group by a Group" (TGA, p.269).

Meanwhile, I think that the time has come to report on the work done under this denomination, regardless of the definition, and to concretize its actual organization and the work to be done in the future.

### **3. Undertakings and projects contemplated by this Chair, proposals for the organization of the Committee and philosophy and line of action to be implemented by the Study Group for Group Analysis.**

What do we understand by Group Analysis? and, who is interested in Group Analysis and in what way? would be the first questions to answer by this Committee in order to start its task. A first step in this direction was given by the Membership Directory 1986-89, where individual members identified themselves with a particular denomination and had a chance for a self-presentation.

We will use this source to start a correspondence in relation to these questions, informing of the existence of this Committee and convening a face to face encounter at the Montreal Congress.

There are two other tasks arising from previous Congresses which I propose this Committee to assume: The one of PLEXUS Editore(s), whose first meeting took place



during the Amsterdam Congress and the one of Group Analysis Mare Nostrum, a Panel which has been running since Opatija in 1985 and also met in Zagreb and Amsterdam. Both endeavors have in common the attempt of building a bridge across language barriers and orientations between group analysts at an international level.

The Committee contemplates the establishment of an international center of information and documentation in Group Analysis addressed at linking those existing already in different language areas, asking their institutional support and fostering cooperation between them.

We are well aware that all these activities imply a dedication of time and money that cannot be expected to be absorbed by the budget of the IAGP and that people joining the Study Group will have to pay a small subscription on top of their membership fees.

The Committee will encourage members to do group work on the theme of the Intl. Congresses, foster workshops to that effect in between Congresses in their local or national associations and favor group presentations, true symposia, at the Congresses. We have experimented with this approach facing the Amsterdam Congress with two Spanish societies: the Sociedad Española de Psicoterapia y Técnicas de Grupo and Sociedad Española para el Desarrollo del Grupo, la Psicoterapia y el Psicoanálisis with very good results. As mentioned above, not only the societies benefitted from the experience but they joined the IAGP as organizations as many of their individual members did. Finally, we completely agree with the recommendation that Dr. Fidler made in reference to Specialized Sections in that the responsibilities assigned to them and the privilege afforded them should be clearly defined and we think that this would equally apply to this Committee. *STUDY GROUP IN GROUP ANALYSIS COMMITTEE.*

Contemplating the schedule for the Lisbon Meeting, we see that there is plenty of time for discussion groups to be arranged, so we ask now for meetings of our Committee to be scheduled for Friday afternoon and Saturday in order to further develop our program and to concretize questions such as appointments of members to the Committee, relationship with the Executive and the Presidency, subscription fees, etc. So, that what we report back on Sunday morning can be circulated among the people who formally signed the petition and others who have shown interest in the project ever since.

Barcelona, August 1, 1991

**Copy of this Report goes to:**

Officers of the Executive,

Past Presidents and Members of the Board who were consulted about the project and/or who signed the petition.

<sup>1</sup> S. H. Foulkes, 1970: "The Symposium in retrospect: An introduction to the discussion in GAIPAC" (OR/AN 4-1-1971). "I have adopted the term "group-analysis" only after it had been relinquished by Trigant Burrow and for many years I was the only one to use it. Later I spoke more specifically of group-analytic psychotherapy, ~~group-analytic therapy~~ etc."

much thought to what was the best term to be used in order to express the fact that this method is based fundamentally on the group... I used the terms group-analysis and group-analytic psychotherapy as synonymous, but have more recently found it useful to use group analysis as a slightly more comprehensive term for various methods and theories as they are on a basis essentially compatible with psycho-analytic and group-analytic assumptions." \*

<sup>2</sup> **ARTICLE X - SECTIONS** "The Board of Directors may in its discretion establish temporary or permanent sections based on specialized interest in order to serve the purpose of our Association and to provide for the cooperation in the organization of congresses. Under such conditions as the Board of Directors may set up, sections may be organized for their individual cooperation in ways consistent with the organization of the International Association and its broad purposes. The range of sections will depend upon present and future needs. Action to initiate a section may be initiated by an application to the Board signed by twenty five members of the Association"

<sup>3</sup> **Art. IX, II. Ad-hoc Committees** - Can be appointed for certain purposes by the President with the approval of the Executive Committee, informing the Board of Directors within sixty days.

<sup>4</sup> In my Brief I quoted the letter from Foulkes to Raul Schilder of October 10, 1967, written while they were working at the Constitution of the IAGP and for the Vienna Program. Foulkes complained there in these terms: "At first sight I have no particular criticism to make about the program in principle except that what was agreed in Biemmo, has not been implemented, namely that we should have separate sections, in particular in Group Analysis...". The interim of the International Council work between Vienna and Zürich was the one when Foulkes' enthusiasm for the IAGP grew to the lowest. January 1967 is when he launches the No. 0 of GAIPAC which he himself edited for the following eight years. In all these years, not a single comment, news or announcements about the activities of the International Council or Congresses appeared in GAIPAC, and mind he was the Vice President of that Council. The only exception to this rule of silence was a very brief report by Malcolm Pines (GR/AN VI/3 November 1973, pp.190-192) covering the International Congress of Group Psychotherapy when the final draft of the Constitution of the new Association was approved and its first Board of Directors was elected. In the first issue after he retired as editor (GR/AN IX/1 March 1976, p.66), besides stating that the eventual aim of GAIPAC has always been an international association of group analysts, in his obituary of Dr. Werner Kemper, a psychoanalyst who introduced group psychotherapy in Brazil, Foulkes made the following comment: "Kemper and I met again at International Congresses of Group Psychotherapy, forerunners of those to be organized by the recently founded IAGP, which I supported in order to provide a forum for all kinds of group psychotherapy, including an independent analytical section." As Křížová wrote, in the last issue of Group Analysis, he well remembered a talk we had during the 1963 Milan Congress and the need we felt for an international groupanalytic organization to run parallel with the other."

# THE SAGA/GAAS PROJECT A BRIEF ON THE SPECIALIZED INTEREST SECTION IN GROUP ANALYSIS

Board of Directors, Cumberland Lodge September 2-4, 1988

(Proposal submitted by Dr. Campos)

Barcelona, August 20th, 1988, Reprinted for Lisbon August 2nd, 1991

On behalf of twenty six "individual members" of our Association and in accordance with Article X<sup>th</sup> of our Constitution I was commissioned to submit an application to the Board of Directors to initiate a permanent specialized section of Group Analysis in the IAGP. Most of you have been already duly informed about the situation of that proposal and, also, you may be aware of the reasons why the applicants at the Amsterdam Meeting decided, for the time being, to withdraw the application.

There is the precedent of a similar petition being made by a group showing interest for a section in Psychodrama, during the Mexico Congress but, I am told, "the proposal was withdrawn to avoid fragmentation of this small Association". See below Dr. Fidler's commentary who was by then President of the Association. Nevertheless, the outcome was the same that for us in Amsterdam: none to deter applications of that sort to have the chance of being considered by the Board of Directors!

It seems that those negative attitudes against setting up Sections go back to the early days in our Association, when competition between founding organizations was always a threat and the chances to get into a single social body were very small. It is understandable then that initiatives as such were systematically put aside and even purposefully avoided. A good example of what I mean is in the letter of S. H. Foulkes to Rasch Schindler of 10 October 1967. While still the Constitution was being drafted and facing for the Vienna Congress, it reads: *At first sight I have no particular criticism to make about the program in principle, except that what was agreed between us at Birmm, has not been implemented, namely that we should have separate sections, in particular in group analysis. I would not be at all surprised if it was Dr. Foulkes himself the one who sponsored the "idea of sections"*

As you will see, the matter concerning such a proposal is complicated indeed. It implies questions of principles, of ideology and politics and strategies related to organizational developments that have a long history: the whole history of our Association may be! Do we have to remember for example that the decision of holding together a single "First World Congress at Toronto instead of two separate ones (AGPA and ASGPP) came not as a free decision of the International Committee but as a pragmatic solution imposed by the hosts, the organizers of the 2nd International Congress of Mental Health? It is because of that early imprinting we are still, institutionally, fighting old battles around the pseudo problem, a false dilemma, the phenomena of group polarization on spontaneity versus standards, or revolutionary movement versus responsible professionalism, which personified by our founders is just a neoclassical clash? If that is so, is more easy to understand why questions likely to evoke "reactions" stir so much negative feeling while the problems and interests that may be behind this negative reactions remain unspoken. My statement in "Brainstorming for New York": *"Questions of standards and the related issue of professionalization takes us back to the first twenty-five years feud between Sirovica and Moreno which I am afraid is not settled and has to be taken up anew. The alternative is not either standards or orthodoxy but standards for a sound heterodoxy"* was misunderstood as if I were proposing to rekindle those old feuds. Nothing more contrary in my mind! What I was preaching then, and still maintain now, is that once and for all we stop suffering from reminiscences, and start doing something to remedy our "phylogenetic neurosis", our collective "I-person complex". Maybe it is precisely from there where come all those not to be spoken problems, irrational fears which justify our institutional phobic allergy to sections!

I do not know any longer if initiating a specialized interest section in the IAGP was a good idea or the better of solutions for the development of Group Analysis internationally. Neither do I know if the aims sought by the applicants were that way better served or not. The fact is that we have been to no avail trying to attain those aims, the GAIFAC way, within the context of one of the organizational members of IAGP -The Group Analytic Society (London)- since 1981. That was why we wanted to give it a trial under the auspices of the IAGP. Maybe that also is a wrong step, but after so much time and work it is hard to conceive that it was a precipitated move. Maybe it is not a question of time, but of political circumstances and the balance of power which are not yet ripe for such a step.

Maybe, having gone so far as we did this time, it was not a wise we that we did not go ahead with the application while in Amsterdam. Had we done so, we will have now a "public decision" on a "test case" The Board would have had to take a stand on the viability and soundness of Art. X. and then according to Art. VI (*"...actions of the Board of Directors shall be reported at each general assembly of membership and annually by mail to each individual member and to each organizational member"*), the only guaranty that members might have against the Board taking whimsical or arbitrary decisions in matters left "to its discretion".

The SAGA/GAAS process, however, generated a correspondence which is worth being reviewed since it clearly shows, to my understanding, which are the believes and the prejudiced attitudes influencing us when actually having to take a decision for or against establishing sections of the IAGP in general. Of course that conflict becomes activated with any particular case. As you may well imagine, the range of responses covers a broad scope from enthusiastic or promissory acceptance to well thought or scathing rejection. Let me show you what I mean in the words of most authorized persons.

Former President and Chairperson of the By-laws Committee Prof. Dr. Raymond Battegay's views are plainly and squarely stated: *"I agree with your proposal to form a special section in Group Analysis. Namely, also all other directions are allowed to constitute a special section"* What Dr. Battegay, our expert in By-laws, finds so natural - a constitutional right after all- from which no group of members can be deprived unless Art. X I was amended- to others is cause of emotionally tinted reactions and serious concern. Past-president Dr. Jay Fidler's opinionize what I mean, since they are the most significative of one of the other extremes. He makes it a question of principles and his immediate reaction on July 23rd 1987: was *"My reaction is less than enthusiastic for your special section of the AGPA. I recall a similar proposal for a psychodrama section being made when we were in Mexico. That was withdrawn to avoid fragmentation of this small Association. ... Once we get 1000 members in the IAGP we can consider the specialized sections. However they would preferably be held as special sections of the regular Congress. By that time we might hope to hold a Congress every year and a regional one the odd years. In the meanwhile I think your proposal would seriously wound the IAGP."*

Close to a year later, in response to my letter of March 14th, Dr. Fidler had sort of mellowed the tone of his statements even though I he continued as definitive on his opinions and adamant in his judgements as always: *"I'm not sure of all the points I made in my previous response to this propo-*

and. My reactions are several and they focus on the integrity of IGPA. I am aware there is a formal international structure for Transactional Analysis. I know there is international structure (informal!) for Psychodrama which keeps people in the field in contact with each other. It is my strong impression that in London is the focal point for a strong network of people involved in Group Analysis. My first recommendation would be to make the Psychodrama and Group Analysis organizations formal and independent of IAGP. When considering the internal structure of the IAGP I feel we should not have asymmetrical structures in the subdivisions that develop. If there is a Specialized Section in Group Analysis, we should simultaneously insist on a Specialized Section in Psychodrama, one on Psychodynamic Group Psychotherapy, and one on Transactional Analysis. That would be acceptable to me if simultaneously WE DEFINE THE RESPONSIBILITIES OF THOSE SPECIALIZED SECTIONS TO THE IAGP and THE PRIVILEGES AFFORDED THEM. Unless that is done with all specialized sections to IAGP and all are started simultaneously, I am definitely opposed to a lopsided and ill defined development".

President elect Dr. Fern Cramer-Azima, even if she shares, as we all do Dr. Heller's concerns regards "the IAGP integrity" and possible negative effects brought along by the establishment of sections, commented: "I can see positive and negative aspects to the proposal. On the positive side, there is identification and homogeneity for the section membership. On the negative side, there is the splintering of the whole group of the IAGP - and further sections may be quickly formalized by competitors. One of the values of the IAGP, is its identity as an umbrella or overall group to which societies and associations with diverse group theory identities join with equal ease. Since our membership is so small, such splintering will likely reduce cohesion and loyalty"

Finally, so as not to be accused of being lopsided in favor of presidents, I will end by quoting the opinion from to Committee Chairpersons who show in a more gentle manner their doubts. Dr. Raquel Berman, Chairperson of Membership writes to me on May 30, 1988 (my translation) "I am still interested in a Specialized Section in Group Psychoanalysis; but I am still worried regards the effects on the IAGP to count with specialized sections based on different theoretical orientations. In the AGPA where I am a member there are no sections. I would like to know about the experience of other societies where the diverse tendencies are housed in specialized sections, f. e. the American Psychological where there is a special section in psychoanalysis. I wish we could broadly discuss the subject"

And, Nominating Committee's Chairperson, Dr. Lise Rafalsen's opinion was on May 30th: "I feel very much inclined to join you in your ideas because I know, how much work you have done and because I do appreciate your way of thinking. However, I think as you mentioned yourself in your letter that it is premature. IGPA is far from reaching the level of organization which would allow differentiation. We are working hard to keep the organization running, and I therefore think a too early fragmentation will be unhealthy. I much prefer the informal getting together of "group analytic people" as I believe you did in Copenhagen"

Opinion which ties up with a commentary I heard Dr. Max Rosenbaum, actually Chairman of the Committee on Ethics recently, August 3rd, which I sure, since it is so much to the point, he will not mind me quoting: "There is a lot of work to be done to make of the IAGP a really viable organization... They have a tendency to depend on AGPA for the USA input. But the AGPA has not grown since I was membership chairman and helped double membership"

Since leaving things as they are at this point certainly would not pave the way for more fortunate tentatives of that kind in a future, but rather there is the risk on being taken as a well thought precedent against them -what I am proposing is that, in the light of the ideas exposed, we examine carefully the general issue of specialized sections based on theoretical or methodological orientations and we pronounce ourselves in principle regards the limits and conditions set up for sections to organize for their individual cooperation in ways consistent with the organization of the International Association and its broad purposes. Once we have done so, we may define rules and regulations and procedures for sections being initiated. On the other hand, if as a result from reviewing the trends and philosophy implicit in the activation of Art. X, we come to the conclusion that establishing specialized sections based on theoretical directions goes against the spirit of the letter of our Constitution, then we will have to make that explicit and propose the according amendments.

The common denominator of the objections mentioned above, in my view derive from a deeply engrained conviction that unhealthy competition among us is inevitable and, what is worse, if unchained it leads into incredible collision and fragmentation of our social body. All the other arguments, considerations regards number of members, concerns about "equal rights opportunity", and "envy prevention measures" check on the viability and soundness of Art. X. "", I see them all rather as symptoms or consequences of defensive mechanisms of a community engaged in a hopeless "as if cooperative action". If that is so, we rather look for some group therapy for ourselves. I do think, we should be really worried about the small number of members and the rapid turnover of membership -individual and organizational. Also, if after forty years, since in London we started to contemplate the convenience of an International Group Association, and another fifteen since it was founded in Zurich, it has not reached the level of organizational development that allows for "individuation differentiation" then we are engaged in a very sick sort of group interaction and cooperation and, mind, we are the experts in that field!

I still maintain some hope regards the possibility of healthy competitiveness for cooperation in our human kind, as against the multinational cooperation for competitiveness of the "conservative revolution" of our days. My feeling is that we have taken as a model for "international cooperation" the one of the United Nations, and that we are confused as to the means and ends and the narrow and broad purpose of the Association.

If the purpose of the AGO was exclusively to organize "International Congresses", we could say that we have been quite successful. We managed to lead a good program and gather around 1500 every three years. But, in case that congresses are only one among the many means used to promote communication, we should rather think of developing the rest.

My question though is if our basic assumption that mutual respect in communication between representatives of different theories and methods is extendable as well to the very professionals who share the same theories and practice. My conviction is that this should be so. This is why I sponsored the establishment of sections of that kind. What is more, I do not believe that this is being achieved at a local, or national level in our own professional groups of identification and belonging.

What is needed is a group of reference safe enough to make true Foucault's 1954 dictum "We are not here to iron out differences, but to contrast our hypotheses in the operative area of the group". My guess is that this cannot be achieved unless each of our members reaches his or her own individual and collective identity as a professional. In this direction a step has been given with the self presentation of individual members in the Directory during the actual period, which could be improved if individual members were asked to say to which professional organization they are affiliated.

## Procedure

CIRCULAR as August 22, 1988

So, as far as procedure is concerned things cannot be more clear, even though they are quite time-consuming, expensive, and complicated if the initiative is taken in between Congresses and has to collect the 25 signatures by correspondence. To give you, an idea in our case, we coincided with President Dr. Grete Leutz with that it would be wise to inform members of the Executive by letter before we meet in Amsterdam and, if possible, submit a draft on aims and regulations for the Section the steps were as follows:

1. The initiative parted from conclusions reached at a Seminar held at the Sacro Cuore, of Rome, on April 3rd, 1987, among groupanalysts, members of different affiliate organizations and experts in organization which had been implied from the beginnings, and for years in the both, or either the development of COIRAG, the Confederation, of Italian, Organizations concerned with group analytic research and of European Group Analysis. Later, on April 10th, this decision was communicated to several psychoanalysts, actual or prospective members of our Association attending the European Forum, of Psychoanalysis held in Barcelona. Similar meetings and equal conclusions were reached at "ad hoc" meetings held in Madrid, April 11th, Bilbao April 24th, Valencia, June 12-15th, and finally during the Oxford Symposium of Group Analysis, September 1-7th 1987, just before going taking the application to Amsterdam.

2. Since there was no precedent of such a situation we were well aware that, by taking action in favor or against a special interest section we were likely to set up procedure we agreed with President Dr. Grete Leutz that it would be wise to inform members of the Executive by letter before we meet in Amsterdam and, if possible, submit a draft on aims and regulations for the Section.

The first I did by sending the memorandum, and a personalised circular letter, attached to all Executive Officers and Councillors, which were made extensive to former and past Presidents and Members of the Board of Directors who were likely to be concerned and also to representative persons of groupanalytic organizations, regardless if they were affiliated not to our Association or members in good standing, but who may be interested in supporting or opposing the proposal. We also sent copies to people whom we had invited to sign the application or to support our petition.

On second thought, however it was felt that it would not be advisable to draw of a definite outline for regulations of the Section's. Its organization, financial accountability and representability, should be the prerogative of the founding members of that Section once the Board of Directors was to make explicit the adequate framework and limits of delegated responsibility and autonomy. With this restrictions, and as a matter of principle, it was understood membership to Section limited exclusively to "individual members" of the IAGP in good standing, and not to "organizational members" or "members of affiliated organizations". That way it was hoped to increase and enrich in individual membership to the Associations, instead of decreasing it. As a matter of fact some people already became apply for membership under the assumption there was in the Association such sort of sections... 3. Had the SAGA/GAAS group submitted the duly signed application then it the Board's at its discretion to decide if to establish or not the section and to report on the action taken to the next General Assembly and to all its members by now... They do not so they save the Directors from the tricky question of having to pronounce themselves regards if to establish this particular section serves or not the purpose of our Association and to provide for the cooperation in the organization of congresses... 4. The explicit motivation of people signing the application was, namely, "to establish an international network of communication in order to further develop in theory and practice the work in favor of Group Analysis (that is to say, all group theories and praxis in accordance with the Fundamental principles of Psychoanalysis and of Group Analysis) and the enthusiastic response of those who support the project even if have adhere to the project -some non members of the Association considered membership in case we counted with a Section. ...

NEW ADDRESS

**THE LIFWYNN FOUNDATION  
30 TURKEY HILL ROAD, SOUTH  
WESTPORT, CONNECTICUT 06880**

72, HILLDALE  
WESTPORT, CT 06880

\_\_\_ I will see you on June 9th.

\_\_\_ Sorry, I can't make it.

Name \_\_\_\_\_

1991-92 Com AG 2-3 <sup>1921 1991</sup>

TO: Lifwynn Conference Group  
FROM: Jack Wikse & Alfreda S. Galt

This letter is to confirm the date of our next meeting, Sunday, May 5, here at The Lifwynn Foundation. We are going to follow through with Nancy's suggestion that we focus on social self-inquiry as a method. Alfreda will facilitate this meeting and will use selections from the videotape from the conference to illustrate her understanding of Burrow's method of group analysis.

This letter is also to follow up on a suggestion we brought up at our January 20 meeting at Bailey Farms. We hoped the continuing "Lifwynn Project" might bring together some of our network of collaborators for "skill-sharing" workshops and we've begun to organize a series of such meetings in Westport. We are inviting conference participants and other friends to lead workshops, give papers, or otherwise demonstrate their approaches in relation to social self-inquiry. These meetings would be open to all of you and would also introduce some of our local professional colleagues to the Lifwynn Project. Travel expenses will be paid and a small honorarium provided for the presenters.

We started in this direction at the conference with David Bohm's and Monte Ullman's contributions and had this in mind when we asked Nancy Baker to facilitate our "sitting" at the March 10th meeting here.

There are two possible formats we're exploring:

- (1) We're beginning a cooperative venture with the Temenos Institute (a Jungian center) in Westport for some events aimed at a wider public and hope to bring Linda Leonard here in September (date not set). Linda has expressed interest in doing a workshop at Temenos and a smaller "skill sharing" group with us at Lifwynn.
- (2) But such arrangements with Temenos will be limited in number. Since we have opened up a space for group work here, we hope to be able to schedule regular meetings to develop a group working on social self-inquiry and sharing its skills to empower a peer investigation.

Alfreda's presentation on the 5th will carry this idea forward. By that time, you will have received the new issue of Lifwynn Correspondence which contains a report on the addiction conference. The latter includes versions of the descriptions of social self-inquiry we sent out before the conference together with a brief comment on this approach by Trigant Burrow.

Vol 1 No 2

The May 5 meeting will include lunch at noon and will last until 4 PM--or later for any who want to stay. Please use the return card to indicate that you can attend and also whether you would like to take part as a presenter/participant in the skill-sharing workshops.

Best to all,

*Alfreda + Jack*



1991-92 Ann AG 2-14

## THE LIFWYNN FOUNDATION

72 Hillandale Road  
Westport, Connecticut 06880  
(203) 227-4139/4130

August 28, 1991

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Dear Juan:

The enclosed description of a social self-inquiry presentation at Montreal is very tentative and subject of course to whatever changes you feel should be made. We regret that we could not get it to you earlier but I think there will still be time for you to telephone us about changes that you might recommend.

It is doubly tentative because we will not be able to get together with Max Rosenbaum until September 5th to talk about it. Between our moving and the upcoming Labor Day weekend, time for a meeting has been short.

Jack and I do have an appointment with Max on the 5th at 2-PM at his place in Putnam County (914-225-6476). It occurred to me that if you wanted to call us from Lisbon while we are there, to clarify anything in relation to our abstract, it might be feasible to do so.

We have not yet been in touch with Dr. Serrano about the enclosed abstract as we would like to have your and Max's input before doing so. It follows the format requested in the Congress materials but must be retyped on their form for submission.

We have been somewhat handicapped in preparing this statement because we would like very much for it to come out of the group as a whole and social self-inquiry is just not at the place yet where it can deal with this kind of long-range planning. So we have suggested a format which is sufficiently flexible to permit the articulation of an organic group expression.

Our moving has been rough but thanks to the remarkable organization of our staff and their devoted and dogged work, things are beginning to become livable.

THE ORIENTATION OF TRIGANT BURROW, M.D. IN AN ONGOING SOCIAL SELF-INQUIRY: An Investigation of Ambivalence and the Authority Function in Groups. (A symposium)

Juan Campos M.D., Chairperson, Grup d'Analisi Barcelona, Spain; Max Rosenbaum, title, Carmel, New York; Philip Woolcott, Jr., M.D., Professor of Psychiatry, University of Illinois at the Medical Center, Chicago; Alfreda S. Galt, Acting President, The Lifwynn Foundation, Westport, CT; John R. Wikse, Research Director, The Lifwynn Foundation, Westport, CT; Mary Alice Roche, Bulletin Editor and Archivist, Sensory Awareness Foundation, Caldwell, NJ; Deborah Hillman, Ph.D. Narcotic and Drug Research, New York, NY; Montague Ullman, M.D., Clinical Professor of Psychiatry Emeritus, Albert Einstein College of Medicine, New York, NY.

Social Self-Inquiry is a research method in process of development. It is based on the group analytic investigations introduced by Trigant Burrow, M.D., in the early nineteen-twenties and reported by him and his associates in over 100 books and papers. It is a study by the group as a whole of its own ambivalences and addictions regarding leadership, self, love and hate, aggression, frustration, etc. In social self-inquiry, the group is regarded as biologically unitary but ideologically fragmented resulting in a basic conflict in feeling which is played out throughout the world in individual and social disruptions.

Our presentation at the 11th Congress of the International Group Psychotherapy Association will begin with remarks by Juan Campos, M.D. and Max Rosenbaum, Ph.D. to orient the audience to the historical and theoretical connections between Burrow's group analytic research and later developments in this field. This will be followed by brief statements from members of the present social self-inquiry group regarding the scope, status and problems of the current inquiry. The meeting will then be open for participation of the audience to join with the speakers in a social self-inquiry session. *c the last*

We are considering at this time the possibility of a one-week

*meeting.*

*Bibliography*

preliminary session (dates) at Lifwynn Camp in the Adirondacks where Trigent Burrow carried out his original group research. If this study session can be arranged, it will involve people who are participating in social self-inquiry or who are interested in it. The participation of several members of the IAGP in this session could be significant in strengthening our presentation at Montreal. Any members who are interested should contact The Lifwynn Foundation, 72 Hillandale Road, Westport, Connecticut 06880, USA.

We hope all goes well in Lisbon. I wish I could be there. With warm greetings to you and Hanne.

Sincerely,

*Alfreda*

Acting President

Dr. Juan Campos Avillar  
Passeig St. Gervasi 30, Rt.  
08022- Barcelona, Spain

*and Hanne's*

P.S. This was ready to go to you when your splendid package arrived. I am writing separately to Hanne for her remarkable letter and the delightful pictures and will reply within a few days to the interesting material you sent. So glad to see the tearsheets from the Group Analysis Bulletin.

cc: Dr. Max Rosenbaum

1991-92 Cor AG 2-15

THE LIFWYNN FOUNDATION

30 Turkey Hill Road South  
Westport, Connecticut 06880  
(203) 227-4139/4130

September 13, 1991

Dear Juan:

It was great to talk with you on Wednesday, and also to have a few words with Hanne. I have tried to get down the highlights of our conversation in the enclosed memo for Max, Jack, and others here. Please correct any misinterpretations I made and fill in any omissions. We are delighted we will see you in February.

With affectionate greetings to both.

Sincerely,

*Alfreda*  
Acting President

Encls.

*P.S. Also enclosed is the memo I mentioned about our visit with Max.*

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Psychiatry Emeritus, Albert  
Einstein College of Medicine,  
New York, NY

Telephone call from Juan Campos. September 11, 1991

Juan would like the abstract to be submitted by The Lifwynn Foundation. It should be sent on the blue form we received some time ago from the Association. The title should be on top as indicated on the form; below it, submitted by TLF with JW and ASG as presenters.

Present plans are for a symposium, four hours or so. We can have as much space as we want.

Juan would be willing to have the abstract submitted in its present form except that he would like to see the final paragraph (bottom of page 1) made more positive. Instead of "we are considering," say, "The participants in this symposium will spend a preliminary week at Lifwynn Camp," etc. He thinks we should not mention that the camp session is open to IAGP members--since we can accommodate only about five or six, the invitation should be extended through Lifwynn Correspondence and personally.

Juan feels we should have a "more touching" title for the symposium and suggests: GROUP ANALYSIS: Operation Phoenix presented by The Lifwynn Foundation.

He was interested to hear of Max's suggestion about not attempting a "demonstration" in Montreal and thinks the latter might be difficult. However, I said that we will soon be having a meeting of our members to consider this and other activities, and we would like to have the question left open until we can discuss it with them. Any suggestions would go to Max and Juan in time for them to comment before the deadline for abstracts Nov. 1.

I congratulated Juan on the prospectus for the Study Group on Group Analysis and said I hoped that our Montreal presentation could tie in with the purposes of that committee. Juan mentioned that there are three members at present, a doctor from Portugal, I believe, and one from Italy. Juan would also like me to be a member but I believe I would have to become a member of the Association first. Juan said that he and Max could sponsor me.

Juan would also like to have the Foundation become an organizational member. I said we had just begun to consider this when he called. I have wondered whether it would be appropriate since Dr. Burrow had stated so clearly that he was not involved in group therapy. Juan feels that membership would help the Foundation claim priority as the forerunner of group therapy--my name (as acting president of the Foundation) would be lost among the thousands of individual members but there are only about thirty or thirty-five organizational members so the Foundation's name would

stand out. He feels it is important that TLF claim priority not only as a forerunner but as having introduced the term group analysis.

One way of doing this is through providing plenty of literature at the meetings and offering writings for sale. He feels Burrow's papers should be available and some of the books. (Earlier he had suggested offering copies of the microfilms of TLF publications.) I am to get information from Dr. Serrano's office about what facilities they will have in Montreal for book sales. Juan hopes to have his book on group analysis, with passages on Burrow, available by that time.

Juan will be in New York next February 17 to 22 for the fiftieth anniversary of the American Association of Group Psychotherapy. There will also be a meeting of the Executive Committee of the International. We should arrange for a visit here or at least meet in New York. A warm-up for Montreal.

Juan spoke with appreciation of our mailings--he reads them even though he is not always able to reply.

Hanne also came on the phone briefly and thanked me for my letter. She has been studying the post-conference dialogue and asked me to tell Jack she had some thoughts on what he had written.

We are free to use any of her letter in Lifwynn Correspondence.

Both sent hugs and kisses to Jack and me.

ASG

## Meeting with Max Rosenbaum, September 5, 1991

Max felt that the most important thing about the overall presentation in Montreal was to relate Burrow's work to the present--not to put all stress on the historical background but to indicate the relevance of his views to current problems. He suggested that we drop the idea of conducting some kind of social self-inquiry that would involve the conference audience as participants--he felt that this might be better to plan for the next Congress which will take place in 1995. (Jack pointed out that we might not be in existence at that time but we didn't go into that.)

Jack and I agreed strongly with Max's thoughts about emphasizing the timeliness of Burrow's observations. We wondered whether we would be able to fill a full day if we were not going to try to have the "demonstration inquiry;" Max thought we should ask for a day and then settle for half a day if that is all we could get. We would probably still need a large number of presenters. Something we should try to get a handle on is whether the IACP membership would demand "professional" qualifications, such as would obtain for Phil, Tom Jobe and Monte.

Max spoke of the need for a neuroscientist on the panel and we said that of course Tom Jobe could fulfill that function. Max was not familiar with his name or writings and at some point we should send him an up-to-date list of Tom's publications, especially if Tom is able to agree to come to Montreal.

I have been thinking of the importance also perhaps of having David Bohm's perspective represented. Perhaps my own presentation should in some way cover the relationship between Burrow and Bohm; or a Bohm scholar could speak.

We were all conscious of the fact that at the moment that we were meeting Juan might very well have been talking to Dr. Serrano in Lisbon and getting his reactions to our plans. Also that a meeting of the Study Group for Group Analysis might well be in progress. It seems as though our presentation in Montreal should be tied in rather closely with this Study Group and Juan will undoubtedly tell us what we might do to further that possibility. Jack and I were struck by the remarks made by Ronald Sandison in his letter to Juan on May 1, 1991. He said "it seems to me that...Trigant Burrow's views could be restated and developed as a unifying agent among the diverse ideas and practices of group psychotherapists throughout the world." Perhaps this concept can provide the integrating focus for our presentation in Montreal--I'm not suggesting that we quote it but rather that we ourselves think of it in preparing our various contributions.



Max felt it was okay to include the reference to the camp season and asked how many of the group analysts we would want to have. I said "three or four perhaps" without having really thought this question through--it would be affected by the number of family members that would accompany them, for one thing, and whether they would be there for the entire session.

When the abstract is ready it will be sent to Juan, Max, Phil Woolcott, et al for suggestions and approval. I asked Max whether he felt that after this it would be desirable for Jack to try to get an appointment with Dr. Serrano in Philadelphia to go over our plans with him. Max thought this might be a good idea: we will be anxious to know how Juan reacts to it. Dr. Serrano is Program Chairman for the Montreal conference by virtue of his being president elect of the association.

I am happy to say that Max gave us an address for Patrick de Mare--we will get the first two issues of Lifwynn Correspondence off to him as soon as we can.

In addition to the above, we had a wide ranging and delightful conversation with Max and Belle. His expert knowledge of the relationship between Freud and Burrow is a constant source of interest and pleasure. We heard with regret that the people in charge of the history of the APA to which Max is contributing the history of group analysis had cut out two of the pages he wrote about Dr. Burrow!

ASG

Sent by FAX 7:50

12.13.1991

MAIL BOXES ETC

1991 1203 259 805.4

Barcelona, 13 th December

Dear Alfreda,

Congratulations! I learned from Alberto Serrano - who by the way is delighted with Lifwynn applying for membership - that you accepted his offer of a 1 1/2 hours morning coplenary which can be followed in the afternoon with an up to 3 hours Symposium and include other presenters. That is great, more than ever I had expected. He did not tell which day, but I assume will be the historical one, and I assure you we will make group analytic history with our presentation. (See the last paragraph of my letter to Berman sponsoring the application that here I attache). Already we have created quite an expectation.

I was not aware you waited for some comments from me on your letter of application. I was so perfect though that the only thing it occurred to me that was to send to you my compliments. Anyhow, I will make sure the Lifwynn is accepted in February, remember you are making the IAGP a great favor. It will help if Max were to write as myself in this sense to Berman without waiting for he consultation. I ignore his address at the moment and have no phone or Fax number to reach him. So please convey to him my message and send my regards and Season's Greetings.

Regards the possibilities of going to New York in February and attending a SSI group meeting I did try in vane to contact you by phone last Sunday from 2 to 6 p.m. at the 140th West 73rd. The Studio for Sensory Awareness has no phone registered number. Chances are that I not go at all, since I am sort of feed up with congresses and all the pseudo-group politics of the IAGP, and one of the main objects for attending the February Executive and Board of Directors meetings -our Montreal presentation- has already been successfully achieved, and the other, business related to the S.G.G.A. may be done by mail. May I count with you for the Committee, I hope! Professor Ancona of Rome, one of our most enthusiastic "Lifwynn correspondents" will join too. By the way I have not been able to read the diskette you send me on the ones we approached. May I include in the mailing of the next issue a letter? When is it going out? But do not worry, I may do with the printed Mailing List you send me, was written. But I may do with the printed list you joined.

The situation is as follows: the IAGP meetings take place at the Waldorf Astoria Hotel, February 15-16th, the weekend preceding the Annual Conference of the AGPA -American Group Psychotherapy Ass.- this year celebrating its Golden Anniversary from Thursday to Saturday (20-22). I have been a member of this organization since 1960 and a Fellow since 1981 but never attended one of its meetings. So even though the IAGP agenda is quite heavy if it was possible to hold the SSI meeting on Sunday the 16th in the morning (say up to 2 or 3 p.m.) it would me better for me than the following February 22nd. Regardless in the case I go, I would

I love to visit you and do work in the Library or we may get together in N.Y.C. where I will be quite free Monday to Wednesday (17-19).

My trip to Roma was personally satisfying, I recovered a peace of life I have left there behind close to 30 years ago and I gave further steps on the concept of Plexus on which I started to work there in 1981. Hopes for cotention are low in the world we live. The meeting with Prof. Girolano Lo Verso and the people of the Laboratorio of Group Analysis in Palermo, Sicily, quite disappointing regards prospects for the IAGP Study Groups of Group Analysis Network.

I hope the upheaval of the move not to be too hard on you all.

Love

JUAN

PS. We have no commented on the following paragraph of my June letter: "One of the things I am concerned with the move is what is going to happen with the collection of papers such as the one you gave me. I will explore here the financial and technical possibilities of taking good laser xerocopies, so to count with a master out of which further copies may be taken. By the way, do you have a list to which people or institutions have you send collections of copies such as me along the years? It would be helpful to contact them and find what they have done with them. Please, keep me posted of any feed back on the L. C. mailing. I have not received directly any so far." Well, this morning I will explore the possibilities and cost of electronic reproduction with and on the desk editing a software program and I will contact Montreal to see the possibilities of distribution (selling) during the Congress. By the way did I send you our first bilingual production? My book on Professional dreams? Let us remember to talk about that on the phone.

*Copie de Cart = Reguel*

1991-92 Cor AG 2-29

THE LIFWYNN FOUNDATION  
72 Hillendale Road  
Westport, Connecticut 06880

December 20, 1991

203-227-4180  
24139

Dear Juan:

I have your Fax of December 13th in front of me and will just go through it paragraph by paragraph answering the matters that need a response.

Yes, I had a good talk with Dr. Serrano who seemed very friendly to our plans. The idea of a one-and-a-half hour co-pleenary in addition to the symposium is certainly quite a challenge and will require careful planning.

I notice in the Prospectus for the Study Group for Group Analysis that you contemplate that "group reports" may come out of it. This is somewhat in line with our original plans for the Congress but we may be limited on the number who can participate due to the cost of transportation. As it is, we are hoping to avoid hotel bills in connection with it wherever possible (making it a day trip from Lifwynn Camp?).

The above paragraphs were written before your welcome call of Sunday. As I understood your plans on the phone, you are not yet entirely certain you are coming. But, if you come, you will plan to leave Monday and Tuesday, February 17 and 18 free to work with us up here. And remember we can put you and Hanne up overnight, if you care to stay at the Foundation.

As I understand, it you will probably be tied up with meetings Sundays, February 16 and 23, so it looks as though it will not be possible for you to get together with the social self-inquiry group and we won't take your plans into account when setting the date for the February meeting. Please confirm that I got all this information accurately as I find so many possibilities for misunderstanding in a phone conversation.

As a matter of fact, meeting with our ssi group does not seem as important to me as those couple of days conferring with you and Hanne about plans for Montreal and doing this on the basis of our common interest in cotention and phyloanalytic investigation. How do we get at the underlying assumptions of people of different cultures? Can we lean less on words and more on feeling and sensation? As I mentioned in an earlier conversation, we have been experimenting with this recently-- Jack, Lloyd Gilden and I-- and are doing so also in the ssi group. I am anxious to put you in touch with this phase of endeavor, and will be disappointed if you decide not to come; that planning period seems important if we are to take advantage of the opportunity offered at Montreal.

Now I am returning to the letter I drafted before you called:

About *Lifwynn Correspondence*, we are just putting the next issue together and can enclose a letter to your group analysts et al, if you want us to do so. There are 136

names on the list not counting the following whose envelopes were returned to us: Centro Analisis Terapeutica; Australian Association of Group Psychotherapists and Juana Gutman de Kaufman.

We did have responses from two and I am fixing copies with this. The items they ordered were all sent.

We will have an item (not yet written) about our Montreal presentation in the next issue of *Lifwynn Correspondence* and the accompanying item about the SGGGA may be part of it or separate. In either case, I would like to have you Fax your comments on it to me--any changes that are needed plus the names of the members of the Study Group.

I have not been able to reach Max by telephone either at Carmel or Florida, so will drop him a line to the latter address to give him your message about writing Dr. Berman.

Now I have come to your P.S. in which you mention that I have not commented on the paragraph from your June letter regarding Laser Zero Copies of Foundation publications. We don't seem to have a copy of this letter in our file and I am wondering whether we ever actually received it as I could have answered quite easily the questions that you raise in the present P.S.:

In 1973 we microfilmed all of the Foundation's publications up to that time--books and papers. One hundred copies of the microfilms were distributed to research libraries in this country and to a number of university and other libraries abroad. We have the master films stored at Iron Mountain, a record depository in New York State. Paper copies of Dr. Burrow's published articles have likewise been distributed widely to libraries in this country and overseas and we still have a supply of most of them. I would be interested to know what information you get about sales in Montreal--when we inquired about that last fall, they said that information would be distributed in due course with other data about the meetings.

No, we have not seen your book on *Professional Dreams* and would like to do so before your visit if you can send us a copy.

We received only part of your June 5th letter with your Fax of December 12th. About the disk we sent you, it is an IBM Personal System II. We have not heard anything from Shelley--we invited him to our two skill-sharing workshops (one with Linda Leonard and one with Joe Couture) but didn't hear from him. Actually there was no need for him to respond.

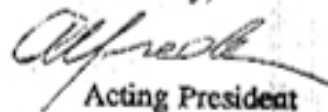
The Fax of your letter to Dr. Berman also arrived mutilated--it was cut off after the first line but I'm sure was an excellent letter. If you have an extra copy, why not

send a copy to Max at 2295 S. Ocean Blvd., Palm Beach, Florida 33480. Telephone no. is 407-586-9733. You could also send us a copy by Air Mail.

I hope that I have adequately responded to your interesting communication. We are busy now, of course, preparing *Lifwynn Correspondence* and developing our fund-raising presentation. Last week we had a long conference by phone with Mary Alice, Steve Rosen, and Deborah Hillman in an effort to define the uniqueness and importance of the Foundation's work and why it merits support. We want it to be the best presentation as possible as our financial situation is critical.

This repeats the greetings given on the phone to you and Hanne for a delightful holiday season and a good 1992! We are greatly looking forward to collaborating with you on our various projects in the coming year.

Sincerely,



Alfred  
Acting President

P.S. I have not yet dug out my notes on insomnia and the cotentive exercise which we talked about on the phone recently, but will put them in the mail for you before long.

P.P.S. I haven't commented either on the plan for translating *Toward Social Sanity...* into Italian. A great idea but of course a difficult project to carry out. Did Dr. Ancona indicate who would be the translator?

Encls.: 3

rec. 9/18/91

The Lifwynn Foundation  
30 Turkey Hill Road South  
Westport, CT 06880  
(203) 227-4139

- Please enter my subscription for Lifwynn Correspondence. I enclose the fee of \$7.50 for the next two issues.
- Please send me copies of the following:
  - A full transcript of David Bohm's remarks on addiction, and the subsequent dialogue at the conference.\*
  - Deborah Hillman's essay, "Reflections on a Group Inquiry into the Meaning of Addiction."\*
  - The Prospectus for the 1990 Conference on Addiction.
  - John Wikse's report on the addiction conference.
  - Alfreda Galt's article, "The Phenomenology of 'Normality' in the Context of Trigant Burrow's Group Analysis" (Reprint from *Journal of Humanistic Psychology*, Winter, 1991).
  - "Psychoanalysis: A Study of the Social Self" (selections from Burrow's writings on social self-inquiry, from *Trigant Burrow: Toward Social Sanity and Human Survival*, Ch. 2).
  - Hans Syr's book, *Of Being and of Meaning*.
  - Bibliography of writings by and about Burrow.

1 1/2  
7.50  
29.50

(Please enclose \$2.00 per item for cost of handling.)

\*Available after June 1, 1991.

I suggest that you send a complimentary copy of Lifwynn Correspondence, to the following people:

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|--|----------------------------------|
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| Address <u>Göteborgs psykiatriiskt</u> | Address <u>BREDÅNGEN 10</u>      |
| <u>STORÅGATAN 13</u>                   | <u>3427 00 BILLDAL</u>           |
| <u>5411 24 GÖTHENBURG</u>              |                                  |
| Name <u>LENA ENGMAN</u>                | Name <u>MARIANNE ABRAHAMSSON</u> |
| Address <u>KÖBERGS GATAN 28</u>        | Address <u>YASOGATAN 42</u>      |
| <u>5416 71 GÖTHENBURG</u>              | <u>41132 GÖTHENBURG, SWEDEN</u>  |

You may / may not use my name in addressing these people.

- Name Siv Andersson  
 Address FHU-9144P/12 ERIK DAHLBERGS GAT. 14  
5411 26 GÖTHENBURG SWEDEN

SIV ANDERSSON  
FNU-Gruppen, Tel. 031-11 09 26  
Erik Dahlbergsgatan 14 U  
411 26 GÖTHENBURG

The following books are available at \$10.00 each from The Lifwynn Foundation:  
Trigant Burrow: Toward Social Sanity and Human Survival: Preconscious Foundations of Human Experience by Trigant Burrow; A Search for Man's Sanity: Selected Letters of Trigant Burrow; About Possession: The Self as Private Property by John Wikse.

all sent  
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VBS 5  
6/17/91

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  - Philip Woolcott's essay, "Narcissism and Human Cooperativity: The Work of Trigant Burrow and Contemporary Psychological Thought."
  - Betsy Behnke's article, "How We Shout Into the Woods is How the Echo Will Sound: Remarks on the Reintegration of Projections."
  - John Wikse's essay, "Gifts-in-Common."
  - Hans Szyz's book, Of Being and of Meaning. 10.-

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Name: \_\_\_\_\_  
 Address: D Dr. DIETER SANDNER  
 Schenkerstr. 106/II, rechts  
 8000 MÜNCHEN 40  
 Tel. 089 / 129 25 43  
 \_\_\_\_\_ Germany (IAGP)

The following books are available at \$10.00 each from The Lifwynn Foundation:  
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 A Search For Man's Sanity: Selected Letters of Trigant Burrow  
 John Wikse, About Possession: The Self As Private Property.

I enclose a cheque about \$ 42.50

5.6.91

Dr. Sandner

30.-  
 -----  
 \$ 427.50  
 -----



THE LIFWYNN FOUNDATION

Dear Joan -  
I thought  
you might like  
to see these -

Refreda

1991-92 Can AG 2-32

THE LIPWYNN FOUNDATION  
72 Hillandale Road  
Westport, Connecticut 06880  
(203) 227-4139/4130

April 14, 1992

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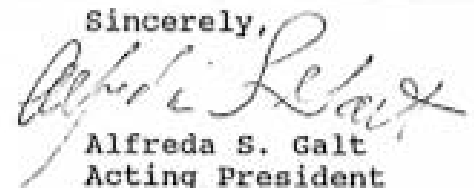
Dear Dr. Pigott:

Juan Campos of Grup d'Analisi, Barcelona, visited us recently and in the course of the conversation mentioned a recent book of yours in which there is reference to Dr. Trigant Burrow and his early group analysis. I was delighted to hear of it, as I recall our exchange of letters some years ago with pleasure. I'm writing now to ask you to let me have the title of your book and where in this country I can order it.

I hope too, that there might be an opportunity for us to meet at the Montreal Conference of the International Association of Group Psychotherapy next August. I am planning to be there as part of a panel on Burrow's contributions and recent experiments here to revive his social self-inquiry. This will take place on Thursday, August 25, at 1:30. It would be splendid to have your input also during the discussion.

With cordial greetings as always,

Sincerely,

  
Alfreda S. Galt  
Acting President

Dr. Claude L. Pigott  
5, rue Edmond Gondinet  
75013 Paris, France

ASG:pw

copy

April 27, 1992

Dear Dr. Ferreira:

At the suggestion of Dr. Juan Campos of Barcelona, Spain, I am sending you the enclosed copies of our bulletin, Lifwynn Correspondence. Volume 1 No. 1 starts with excerpts from Dr. Campos' brochure about Grup d'Analisi which refers tellingly to the early group analysis carried on at The Lifwynn Foundation by Trigant Burrow, M.D.

Volume 2 No. 1 contains a "report" on our present social self-inquiry--an effort to revitalize Burrow's early investigations. I am not sure how much acquaintance you have with this pioneer group analyst so am taking the liberty of enclosing also a copy of a paper of mine which appeared last year in the Journal of Humanistic Psychology and which recapitulates the Foundation's early research.

We are looking forward very much to participating in a symposium this August at the meetings of the International Association of Group Psychotherapy in Montreal, to be chaired by Dr. Campos. It is described on Page 29 of the latest Lifwynn Correspondence. Presenters will be Max Rosenbaum, Lloyd Gilden, Associate Professor of the Department of Psychology at Queens College, New York; Dr. Wikse, our Research Director and myself. The symposium title is "Beyond Dichotomy: the Orientation of Trigant Burrow." I enclose a copy of the abstract and hope that you will be able to join us that day.

With kind regards,

Sincerely,

Alfreda S. Galt  
Acting President

Antonio G. Ferreira, M.D.  
Av. Mouzinho de Albuquerque  
Lote B1. 7'  
1100 Lisboa, Portugal

ASG:pw

April 14, 1992

Dear Dr. Ancona:

Juan Campos has told me of your interest in Trigant Burrow: Toward Social Sanity and Human Survival and I was very pleased to hear that you felt that this presentation had merit. We appreciate very much your having recommended it for possible translation to Edizione Unidelsitarie Romano; a representative, Ms. Pallai, called us in February and we sent a copy of the book for their consideration. If you should hear their reactions to it, we would be glad indeed if you would let us know.

Perhaps I will have the opportunity of meeting you in person in Montreal in August--I am taking part in a symposium on Burrow's research and its applicability to problems of today. If you also plan to attend the IAGP meetings, I will hope to see you.

With cordial greetings,

Sincerely,

Alfreda S. Galt  
Acting President

Dr. Leonardo Ancona  
Via Tiziano,  
19- ROMA, Italy

ASG:pw

P.S. I take the liberty of enclosing a recent paper of mine.

1991-92 Con AG 2-38

~~JW~~, ~~NC~~, ~~NA~~

Memo of a conversation with Mr. Dufresne, April 30, 1992

Mr. Dufresne is open to our sending books for sale in the bookstore they will set up at the IACP Conference. They should not be sent before August 1st but then he wants to get them as quickly after that as possible. They can take a supply of different books and, if they run out of them, will be prepared to take orders. But he said something about there being 2,000 people involved so that he will appreciate our not sending too large a supply of books.

He says that they can take microfiche copies of the Foundation's publications, if we want to send them; and also boxed copies of the Foundation's papers (Burrow's only or including Syz, Galt, etc.). As he said, "Anything that we can sell is okay." I believe that Penny and Maureen have started to get information about preparing these non-book items.

Regarding prices, Mr. Dufresne says that we are to set the retail price that we want to charge of which he will keep 40%.

Mr. Dufresne would like to have a list of the books, etc., that we are going to send him. First of all, though, he would like us to FAX him our name and address so that he can send us information about the operation at the Congress.

ASG

for an 5/4/92

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ASSOCIATION INTERNATIONALE DE PSYCHOTHERAPIE DE GROUPE  
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Barcelona, 24th June, 1992

Dear friends and student-colleagues in Group Analysis,

As the Montreal Congress is coming closer, and thus the chance of meeting you or a representative of your organization there, I would like to get in touch with you and inform you about events our Study Group in Group Analysis Committee is sponsoring which may be of interest.

First of all, let me remind you that on Tuesday afternoon, August 25, from 3:30-6:30 PM, The Lifwynn Foundation (Westport, CT, U.S.A.) will present the symposium "Beyond Dichotomy: The Orientation of Trigant Burrow" which I will chair and to which you are most welcome. This will be an advance report on the three-year social self-inquiry project they have been conducting which included an interdisciplinary conference on our "addictive society"—a most relevant theoretical and practical contribution toward the "understanding of love and hate," the main theme of our Congress. If you are a subscriber to *Lifwynn Correspondence*, you are likely to be up-to-date on the development of this symposium. For those of you who are not yet subscribers, I am enclosing a form in case you care to become one.

The Board of Directors meeting in New York on February 16, 1992, had the pleasure and the honor of accepting the application of The Lifwynn Foundation as an organizational member of our Association. This Foundation, established in 1927 by Trigant Burrow and his associates, for laboratory research in analytic and social psychiatry, is not only the most ancient of group analytic organizations and forerunner to them all, but also the only one dedicated exclusively to research on the community-at-large.

Directly following the Lifwynn Symposium, after a break, the Study Group in Group Analysis will meet for one hour in the same room. The main issue will center on a research proposal on the "identity of the group analyst" and on "aims and means" for the Committee on Study Groups in Group Analysis. The long report in this regard, submitted by me to the Board of Directors in Lisbon last September, will be available for distribution at Montreal.

Finally, we hope to be able to have an exhibition of group analytic publications and periodicals at the Congress site. Raymond Dufresne, a local bookseller at Iris Diffusion, 5090, rue de Bellechasse, Montréal, Québec, H1T 2A2, Fax #1-514-256-5078, is ready to handle orders and/or subscriptions with the usual commercial percentages. Books should arrive at Montreal in the first week of August (before August 7th), 1992. Three copies of each book and a full collection of journals will suffice for exhibition.

Hoping to see you soon and with best wishes for our next Congress,

*Juan Campos Avillar*  
Juan Campos Avillar  
Chairman, Study Group  
in Group Analysis Committee

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September 17, 1992

Dear Juan:

In response to your request of last week for some biographical information on Dr. Burrow, I am sending the accompanying material from our files. As you will see, each was prepared for a specific purpose and may not be suitable for a Spanish version but together they will provide whatever facts you don't already have at your fingertips. If there is anything further I can do to help on this, please let me know.

On the Satisfaxion, I am also sending copies of a couple of memos I've prepared for distribution to our directors to inform them of some of the happenings at Montreal. One of them is a resume of the meeting of the Study Group on Group Analysis which I wrote with help of a phone call to Jack Wikse. If there are matters that we failed to include, please let me know.

I've been in touch with Shelley Waxenberg in response to a note he sent about the old play Dr. Burrow wrote under a pseudonym early in his psychoanalytic practice. It is really outdated, as you may have been aware if you glanced at it while you were here. I don't think it would do credit to anyone at this point and discouraged Shelly and his wife from considering it for her drama group.

As you asked, I am sending copies of Lifwynn Correspondence to Ana de Quiroga and Enrique Richon (sp?) Riviere in Buenos Aires (I believe she was at the Congress but not he); Dr. Skolnick, and Theresa Howard who has worked with Pat de Mare. She was not at the Study Group meeting, perhaps because she, too, could not find the room. Those who were there will be hearing from you to follow up on the meeting and I won't send L.C. until I hear from you further as to their names and whether they should receive it.

I still don't have any ideas about a statement on "How to Read Burrow." At one time I may have mentioned to you that I would like to write something along that line in relation to Burrow's Table Notes—it would be very short and stress the need to keep in touch with one's ongoing reactions (irritation, defensiveness, approval) together with accompanying sensation and note to what extent they relate to "I"-persons affects. But this would be too advanced for people who are just beginning to read Burrow. (Presumably a reader would be well acquainted with Burrow's writing by the time he/she tackled the table notes.)

I put Toward Social Sanity... together for newcomers to phylobiology and I am hopeful that they will find it readable without having a guide. If I have misunderstood what you had in mind about a "How to read Burrow" statement, be sure to let me know. As a matter of fact, you would be more aware of pitfalls that group analysts might encounter in reading him than I would be.

I look back on the experience in Montreal with pleasure and satisfaction, both the opportunity to be with you and Hanna and the chance to bring Burrow's work to the members of the Association, thanks to your effort and persistence. It was a good job!!!

Love to you both,





# Lifwynn Correspondence

Volume 2 No. 2  
Autumn 1992



CORTESIA de  
Grup d'Anàlisi Barcelona

## THE LIFWYNN FOUNDATION IN TRANSITION

This issue of *Lifwynn Correspondence* is composed mainly of papers on the work of Trigant Burrow which were presented in August, 1992, to the International Association of Group Psychotherapy. Those of Juan Campos, Max Rosenbaum, and Alfreda Galt are principally concerned with the historical background of Burrow and his work, while the present focus of The Lifwynn Foundation is more specifically reflected in those of Lloyd Gilden and John Wikse.

The latter's paper reports on the three-year experiment undertaken in 1989 to develop a new phase of research at the Foundation. We are now exploring with Queens College, Flushing, New York, the possibility of establishing there a Center for Research in Attentional Development. This Center would be under the direction of Lloyd Gilden, who is an Associate Professor at Queens and has been on their faculty for twenty-seven years; his contact with The Lifwynn Foundation dates back to 1950. Last spring Dr. Gilden was authorized by the Foundation's Board of Directors to submit to the President of Queens College a proposal for such a Center.

The paper Dr. Gilden gave before the IAGP explains the discrimination of mood and attention which emerged from Burrow's original group analytic research and remains the underlying concern of this organization. This aspect is emphasized in our present planning; and a Queens College Center would offer opportunities for group and instrumental studies into the contrasting attentional modes identified by Burrow. Space could also be provided for the Hans Syz Memorial Library and for our continuing research group meetings in social self-inquiry. The Lifwynn Foundation would undertake to finance the Research Center for the first two years, with initial funds coming from the limited resources remaining from the recent sale of the Foundation's former property in Westport, Connecticut. After that it is hoped grants will be available.

The lack of substantial financial support has been a limiting factor in all our attempts to carry forward the work of this organization. In February the Foundation is giving up its rental quarters which has housed our office for the past two years and which it has shared with Alfreda Galt. She has taken a condominium which can also accommodate a small office for the Foundation but without the assistance of Penny White our Office Manager of twelve years. There its networking functions can be continued: distribution of books and reprints; correspondence; collaborative projects, and, we hope, the continued publication of *Lifwynn Correspondence*.

Lack of finances also played a decisive part in Jack Wikse's leaving his position as the Foundation's Research Director in May. He has returned to his teaching position at Shimer College in Illinois. But he will continue to participate in our projects and serve on The Lifwynn Foundation's Board of Directors. The energy and imagination he brought to our three-year experiment continues to provide impetus for many of the activities described in this journal and indeed for the journal itself. We hope you will find interest in its pages and will continue to share your comments and observations with us. We hope, too, that when the question of the Queens project is settled and we can accurately estimate our future budget, you will want to lend financial support as well. Such financial support will be necessary if we are to continue our work. We will keep you informed about our various projects and their needs.

## Lifwynn Correspondence

Editors: Alfreda S. Galt/Mary Alice Roche

ELEVENTH CONGRESS OF THE INTERNATIONAL ASSOCIATION OF GROUP PSYCHOTHERAPY	2
Symposium: "Beyond Dichotomy: The Orientation of Trigant Burrow"	
<i>Burrow, Foulkes and Freud: An Historical Perspective</i> by Juan Campos	2
<i>Trigant Burrow--A Seminal Thinker</i> by Max Rosenbaum	9
<i>Experiencing the First Group Analysis</i> by Alfreda S. Galt	13
<i>A Developmental Perspective on Burrow's Theory of Attention</i> by Lloyd Gilden	20
<i>Dreams, Dialogue and Bi-polarity--Our Three-Year Experiment at The Lifwynn Foundation</i> by John R. Wikse	27
THE STUDY GROUP FOR GROUP ANALYSIS	31
DIALOGUE ON SOCIAL SELF-INQUIRY	31
(A conversation among Lloyd Gilden, Steve Rosen, Mary Alice Roche, Maureen Cotter, Alfreda S. Galt and Margaret Rockoff)	
DAVID J. BOHM 1918-1992	35
JOINT CONFERENCE WITH THE INSTITUTE OF GENERAL SEMANTICS	35
TRIGANT BURROW IN TRANSLATION	36
"CELUI QUI A OSÉ"	36

### Cover paintings

The oil paintings reproduced on the covers of this publication come from a series called *Crowds* by Michael Sperber, M.D. They were done, he writes, "while I was a medical student at the Free University of Brussels, where I undertook my studies after failing out of a U.S. Medical School. I felt ashamed, depressed, angry and 'cut off' from humanity. Fortunately, I found a superb psychotherapist who encouraged my art and, by expressing my feelings in the paintings, I became reconnected to the humankind family. The experience allowed me to become part of the 'brotherhood of those who bear the mark of pain.'" (Albert Schweitzer's words)."

The titles are "Crowd I" (front cover) which is in the collection of Dr. Claud Bloch, Brussels, Belgium; and "Crowd II" (back cover) in the artist's collection.

## ELEVENTH CONGRESS OF THE INTERNATIONAL ASSOCIATION OF GROUP PSYCHOTHERAPY

The Eleventh Congress of the International Association of Group Psychotherapy took place from August 22-28, 1992, in Montreal, on the topic of Love and Hate. Through the efforts of the Foundation's advisor, Juan Campos, vice president of the Association and chairperson of Grup d'Anàlisi, Barcelona, Spain, a symposium entitled "Beyond Dichotomy: The Orientation of Trigant Burrow" was held at the Congress on August 25.

Juan Campos, after briefly introducing the speakers and the subject of the Symposium, expanded on what he had already written for the Book of Abstracts of the Congress in the following terms:

### **Burrow, Foulkes and Freud: An Historical Perspective**

*by Juan Campos*

At the Spring Meeting in Montreal in 1940, the American Psychoanalytic Association bestowed the Abraham A. Brill Memorial Medal on Dr. Trigant Burrow, as one of its living ex-presidents. It is quite paradoxical--the making of this small token reparation to the man who, in 1933, had been denied membership in the New York Society, thus expelling him from the Association. Burrow, though, excused himself from being present to accept this "honor" in person. Why he did so will forever remain a secret. Maybe he just wanted to spare everybody embarrassment.

Trigant Burrow (1875-1950) was the first American-born practitioner of psychoanalysis; together with Carl Jung, he co-invented "didactic analysis;" and he was the only American present in Nuremberg in 1910 as a charter member of both the IPA and the American Psychoanalytic Association. Burrow was also the discoverer of our "social neurosis" and one of its remedies, "group analysis."

The 500th anniversary of the discovery of America coincides with the diamond anniversary of one of the greatest American discoveries in psychoanalysis: it is seventy-five years since Trigant Burrow, together with Clarence Shields, initiated the "mutual analysis" out of which was to develop group analysis, the forerunner of all future group therapies. Ten years later, as an enlarged group, they established the "analytic community," a "commune in group analysis," in order to study our common social neurosis.

The IAGP is honored that this "most ancient" of all group therapy organizations--and the only one to remain ever since a "study group in group analysis"--has applied for membership to our Association and will join as one more of the study groups of the network whose committee I chair. It is emblematic that

this should happen here in Montreal, so close to Lifwynn Camp on Lake Chateaugay in the Adirondacks of upstate New York, the summer research station where the first experiments in group analysis took place in the early twenties.

Our new organizational colleague, The Lifwynn Foundation for Laboratory Research in Analytic and Social Psychiatry (as it was originally called) was and still is, I am afraid, the only social body of this kind to systematically apply to itself the same procedures they provide for others. Many of us are eager to know about this group analysis and its origins, how it managed to survive, where it stands today and where it is heading in the future. Mostly I hope they will help to solve the problems around love and hate that the world is facing at this moment.

Ignoring the work of Burrow and his associates, Sigmund Freud in 1929, in one of those "epochs of civilization, or possibly a time when the whole of mankind had become neurotic," agreed that an attempt to carry psychoanalysis over to the cultural community was not absurd or doomed to be fruitless. He even conceded that "an analytic dissection of such neuroses might lead to therapeutic recommendations which could lay claim to great practical interest...we may expect," concluded Freud, "that one day someone will venture to embark upon a pathology of cultural communities."<sup>1</sup> Those who dared such a venture found that it not only was a question of practical interest but implied a theoretical breakthrough which leads into a new scientific *Weltanschauung*.

Trigant Burrow's findings were reported in over 100 books and papers by himself and his associates, and still very few people know about him or his work.

One of the few exceptions is S. H. Foulkes, the man who borrowed the term "group analysis" to name his own group psychotherapeutic procedure, in the conviction that Burrow had abandoned it in the thirties when he coined the denomination *psyloanalysis* to describe his group method.

When in April 1946, Foulkes presented his work with groups during the war to the British Psychoanalytical Society, he started his talk "On Group Analysis" by saying: "It has been rightly said that group therapy has a very long past and a very short history. To compare and contrast different methods might prove confusing. Until very recently I only knew by hearsay of the work done by others and my knowledge of it is patchy at present. I may, therefore, take it that you are interested in having some information about the development of group treatment as I know from my own experience....I want, however, to make it clear that in confining myself to my own work, I am doing so for lack of adequate knowledge and not from any disregard or disrespect for the work done by others."

In this report Foulkes went to pains to avoid any connection of his [group analysis] work with the man who coined the term, Trigant Burrow; and went so far as to attribute its paternity to Karl Mannheim. He did this in the chapter dedicated to group analysis in his "Diagnosis of Our Time" (1943), where reference to Trigant Burrow is omitted. Still more startling is, however, that after a few years Foulkes not only had overcome this initial refusal to recognize his predecessor, but then never ceased stating that

the idea of group analysis as a form of treatment was put into his mind by reading a paper of Trigant Burrow in the mid-twenties.

Of course, Foulkes was no historian. The historian in group analysis is E. James Anthony who, in *Comprehensive Group Psychotherapy*, states that history can be written in at least two different ways: just to tell it as it happened or to reconstruct it in the context of present day theory and practice. He sided for the latter by stating that:

The essence of the dynamic historical method is to select the significant facts of history and arrange them within a temporal sequence. Inevitably, some manipulation is involved in this selection and arrangement, but there must be no distortion of the facts. The clinical historian must be ready to admit that, although he may have a bias in his arrangement of facts, he is still able to recognize other perspectives, even when they contradict his own thesis. Like the good therapist, he should be able to see in this multiplicity of incompatible perspectives not failure or foolishness but the very richness of life...<sup>2</sup>

If this symposium is taking place here today, it is because, during 1986-87 there arose within the pages of *Group Analysis* a heated controversy regarding the person and work of Trigant Burrow. It was prompted by a book review<sup>3</sup> of *Trigant Burrow: Toward Social Sanity and Human Survival*, a selection of his writings compiled by Alfreda S. Galt for Horizon Press, New York, in 1984. Burrow is well known for his description of social neurosis and what he calls the "I"-persona complex that shows up in groups as well as in individuals. Alfreda Galt's book loosed in the reviewer, J. Roberts, such an attack of social neurosis that the editor of *Group Analysis* felt compelled to remedy this by presenting in the same issue Max Rosenbaum's more positive view of Burrow's work. J. Roberts, after recognizing that he found some of the jargon hard to stomach--particularly neologisms introduced by Burrow, such as *cotention* and *ditention*--and recognizing that the book had prompted in him an aversion to Burrow and his works which he was sure was irrational, concluded that one must read these pioneering authors in their entirety or not at all. This was a very Freudian reaction, as we will see later on. On the contrary, Max Rosenbaum--who has read Burrow's work from his whole professional career and who already in 1960, on the occasion of the publication of *A Search for Man's Sanity*, had made an extensive review of Burrow's life and work--concluded his article in *Group Analysis* with the following paragraph: "Galt's book is a brief sampling of Burrow's theories, but the book should serve as a stimulus to read his full works. Galt has added an excellent glossary to her brief synopsis of Burrow's work and this should be of immense aid to the reader who is unfamiliar with Burrow. Burrow was a pioneer. It is time he received his long overdue recognition. Begin with reading Galt's book."

Good and bad, right and wrong, are typical polarities of the "I"-persona. In the following issue of *Group Analysis*, however, a third party joined in the discussion--another American pioneer of group psychotherapy and, obviously, a militant Freudian psychoanalyst, Dr. Saul Scheidlinger, who most authoritatively took exception to Rosenbaum's conclusions as to why Burrow has failed to attract support from most of his psychoanalytic and psychiatric colleagues. One of the main points of Rosenbaum's

thesis, although not the central one, is the fact that Burrow had chosen Jung as his analyst--which did not endear him to Freud who could be extremely critical and hold deep-seated resentments--and that Freud's resentments towards Jung rubbed off onto Burrow.

Saul Scheidlinger said, "There is considerable evidence for the notion that Freud's opposition to Burrow, beginning in the early twenties...was primarily due to Burrow's increasing deviation in theory and clinical practice from what was then deemed to be the established approach. Thus, Burrow's idea of a social neurosis encompassing both the patient and the analyst, written...in a language with its own semantic rights,' were highly suspect. In addition, his practice of lumping together patients, coworkers, students and family members, including children, into his 'analytic' groups came to be widely perceived as eccentric if not unethical." There are many more such statements in Saul Scheidlinger's commentaries, but what is really amazing is his conclusion:

In sum, I believe that not unlike the once highly regarded Wilhelm Reich whose 'going too far' with his orgone enterprises lost him the membership in the American Psychoanalytic Association, Burrow's abstruse physiological perspectives and global aims to solve society's ills, enunciated in his later years, lead to a similar fate and caused some of his uniquely original ideas to be ignored. I have a strong hunch that Burrow, who over many years kept besieging Freud with reprints and letters with extravagant claims for his group methods, became the unwitting cause for Freud's dropping forever the theme of group psychology, quite suddenly, after having written an impressive treatise about it in 1921. <sup>4</sup>

Amazing! This is how history is written. Of course, that led Max Rosenbaum to reply and, in turn, Saul Scheidlinger to reply to Max Rosenbaum, until, finally, the whole issue was smoothed over with a soft reply from the editor of the book, Alfreda Galt. In her reply she said quite gently: "I feel that he (J. Roberts) approached the book with an open mind and is frank and explicit about why he found it wanting. I do not argue with this opinion but I would like to speak a word in Burrow's defense on the use of neologisms, particularly the terms *cotention* and *ditentation* which Dr. Roberts 'found hard to stomach.' These words were introduced to define the contrasting modes of attention and feeling which Dr. Burrow observed and demarcated in the course of his group research. Everyday language did not contain words or phrases to describe these somatically distinctive patterns and, like other investigators in new fields, Burrow coined words to fill the gap." Actually Trigant Burrow coined many other terms beside cotention and ditention, such as "social neurosis" and "group analysis" which last term he never disclaimed.

We should be thankful to the editor of *Group Analysis* for having given "the word" a chance to be treated group-analytically in the pages of that journal. This proved that the spirit of old GAIPAC (Group Analysis International Panel and Correspondence) was not at all dead and buried. Personally, Max Rosenbaum's piece there led me to The Lifwynn Foundation in Westport, Connecticut, and to the discovery that not only is this organization still alive, but that it was the first psychoanalytical learning community and the first study group in analysis. I mean that literally. It is the only one that through its

Constitution applies to its own social organization and administration the very same group analytical principles carried out in their studies of personal and social neuroses everywhere. Kurt Goldstein, maestro of Foulkes, wrote to Burrow on one occasion: "You are one of the few scientists who make one feel that for him life and work are closely related." <sup>5</sup> I have a very similar feeling about the people I met in Westport, and at Bailey Farms during the Lifwynn Conference on Addiction--those engaged in the Lifwynn Project 1989-92 which is to be discussed in this symposium.

We are not here to eulogize Trigant Burrow or The Lifwynn Foundation--nor to blame anybody. I have learned that it is not your fault or my fault when we cannot think along, feel along together. The impairment for cooperative action--the social neurosis--lies in the system and we are all part of that system. Freud was, Burrow was, Foulkes was and we still are in forced connivance to support all that goes against human health and healthy development. What I do hope we achieve here is simply a longer history for group analysis, and an even longer future for the study groups of group analysis.

Much has been written as to why Trigant Burrow was ostracized first by Freud and then by the psychoanalytic community. To do justice to these occurrences one should go to the Freud/Burrow correspondence which is archived at Yale University. I did so; and there I found also the correspondence of S. H. Foulkes with Hans Syz, long-time president of The Lifwynn Foundation. Regarding the latter correspondence, which clarifies the relationship of Foulkes' ideas with those of Burrow, I shall write on another occasion. Now, however, I would like to quote a few paragraphs that give us a hint about an issue that very much puzzled both Freud and Foulkes.

In my Rome position paper on institutional resistances to group analysis, I began by quoting the following paragraph from Freud's letter to Foulkes of May 1, 1932:

To me it appears to be the greatest disappointment in analysis that it does not effect a greater change in the analysts themselves. No one has yet made a subject of study the means by which analysts succeed in evading the influence of analysis on their own persons. <sup>6</sup>

In 1937, in Chapter 7 of *Analysis Terminable and Interminable*, Freud makes an attempt in this direction. He states:

This alone [completion of his training analysis] would not suffice for his instruction; but we reckon on the stimuli that he has received in his own analysis not ceasing when it ends and on the processes of remodelling the ego continuing spontaneously in the analysed subject and making use of all subsequent experiences in this newly-acquired sense. This does in fact happen, and in so far as it happens it makes the analysed subject qualified to be an analyst himself. <sup>7</sup>

For Freud, obviously, learning to learn under new and different circumstances, and learning how not to resist possible changes required by the situation, are the criteria by which an analyst is qualified.



However, it may be seen in the paragraph that follows [the above quotation], this satisfactory result is rarely achieved by training analysis:

Unfortunately something else happens as well....It seems that a number of analysts learn to make use of defensive mechanisms which allow them to divert the implications and demands of analysis from themselves (probably by directing them onto other people), so that they themselves remain as they are and are able to withdraw from the critical and corrective influence of analysis. <sup>8</sup>

S. H. Foulkes seems to have read in this text the confirmation to the question that he tentatively had put to Freud in the letter which brought the above-mentioned answer from Freud. Early in 1932, Foulkes thought that by analyzing others, analysts defend themselves against recovery and cling tenaciously to "illness and suffering," and what is more they do so not only "like the patient but perhaps through the patient." It can be considered that Foulkes dedicated his lifetime to studying how not only patients but also analysts change and do not change. By 1975, however, he concluded:

I can well believe that this happy result (which is necessary for one to be a qualified analyst) is not too frequently achieved, but if it is not, I doubt [whether] further analysis, however often repeated, will bring it about. I suspect that the interminable analysis will, in turn, produce more interminable analysis.

This concern about the qualifications of the psychoanalyst does not seem to have been far from the mind of Freud in his correspondence with Trigant Burrow and, maybe, this was the main reason for not being able to understand him. If we go back to this correspondence, we can extract the following excerpts of August 15, 1926:

*I see you are grappling with an important, still unsolved problem, but I have made no progress in understanding how you manage it. A plain and clear description of the way your laboratory-method is performed, would help me more than all your theoretical speculation and your reference to the Theory of Relativity which to me seems out of place. [Italics added]* <sup>9</sup>

Freud's answer, instead of discouraging Burrow, prompted him to this enthusiastic response of October 16, 1926:

Dear Professor Freud: I was indeed glad to have your letter of August 15th....It heartens me very much to know that you realize something of the difficulty of the work I have undertaken. When you write me that you are interested in my publications and realize that I am '*grappling with an important, still unsolved problem*,'... [Italics added] <sup>10</sup>

In response to this, on November 14, 1926, Trigant Burrow received from Freud his final appraisal which I quote in full:

Dear Dr. Burrow: I would not like you to form an incorrect idea of my position regarding your innovations. My final judgment I must naturally withhold until I have a better insight into your technique, but my expectations are not at all favorable to you. At the present time I do not believe that the analysis of a patient can be conducted in any other way than in the family situation, that is, limited to two people. The mass situation will either result immediately in a leader and those led by him, that is, it will become similar to the family situation but entailing great difficulties in the function of expression and unnecessary complications of jealousy and competition, or it will bring into effect the "brother hord" where everybody has the same right and where, I believe, an analytical influence is impossible.

I have in general the impression that you let yourself be led too far by speculative analogies and that you neglect practically significant differences in favor of certain correspondences. I think back with displeasure to your attempt to transfer the viewpoints of the theory of relativity into psychology. The neuroses of society cannot be simply identified with the neuroses of the individual. One would better compare them with the distortions of character and with inhibitions of the single individual. I do not believe that we should be grateful to you for the fact that you want to extend our therapeutic task to improving the world.

*The unsolved problem toward whose clarifications I see you are working is probably the defining of the requirements which have to be demanded from the practicing analyst. It is apparent that he has to be different from other people and, on the other hand, one cannot demand from him the realization of those ideals which are most difficult to fulfill. With my great respect, (signed) Freud. [Italics added]* <sup>11</sup>

Tomorrow, in the meeting of the Study Group on Group Analysis I will make the proposal to explore up to what point the work of Trigant Burrow is still relevant to the solution of the problems that as group analysts we are facing today. After all, maybe Anthony is right when he concludes the paragraph I quoted above with the following reflections:

The ultimate lesson from history, therefore, is that for coherent, logical development in a discipline, one must constantly and consistently remember where he came from and where he is going. The past is conglomerate, complex, confabulatory, and conflictual, but it is incumbent on every worker to resolve these perplexities and complexities for himself and, by so doing, discover his own professional identity and ultimate purpose. Each group psychotherapist must become his own historian and thread his way with open-mindedness and relative impartiality through the shoals of psychobiologically, improbable, mythological, mystical, and paralogical ideas of the past and present, asking his own questions and seeking his own answers within the

totality of what is known or imagined. He has to undertake this job for himself, since no one can do it for him. <sup>12</sup>

- <sup>1</sup> Sigmund Freud: *Civilization and its Discontents*. CW, James Strachey, ed., Vol. XXI. London: The Hogarth Press, 1961, P. 144.
- <sup>2</sup> E. James Anthony. "The History of Group Psychotherapy," in *Comprehensive Group Psychotherapy*, Harold I. Kaplan and Benjamin J. Sadock, Eds. Baltimore: The Williams and Wilkins Company, 1971, pp. 4-5.
- <sup>3</sup> *Group Analysis*. Vol. 19, No. 2, P. 188.
- <sup>4</sup> \_\_\_\_\_, Vol. 20, No. 1, pp. 75-76.
- <sup>5</sup> William Galt et al, Eds. *A Search for Man's Sanity: The Selected Letters of Trigant Burrow*. New York: Oxford University Press, 1958, P. 69.
- <sup>6</sup> *Group Analysis*. Vol. 13, pp. 6-7.
- <sup>7</sup> Sigmund Freud: *Analysis Terminable and Interminable*. CW, James Strachey, ed., Vol. XXIII. London: The Hogarth Press, 1964, pp. 248-49.
- <sup>8</sup> *Ibid*, P. 249.
- <sup>9</sup> Unpublished letter, Trigant Burrow Papers, Department of Manuscripts and Archives, Yale University Library.
- <sup>10</sup> William Galt et al, Eds. *A Search for Man's Sanity: The Selected Letters of Trigant Burrow*. New York: Oxford University Press, 1958, P. 149.
- <sup>11</sup> Unpublished letter, Trigant Burrow Papers, Department of Manuscripts and Archives, Yale University Library.
- <sup>12</sup> E. James Anthony. "The History of Group Psychotherapy," in *Comprehensive Group Psychotherapy*, Harold I. Kaplan and Benjamin J. Sadock, Eds. Baltimore: The Williams and Wilkins Company, 1971, P. 5.

## Trigant Burrow – A Seminal Thinker

by Max Rosenbaum

(Max Rosenbaum, formerly Clinical Professor of Psychology at Adelphi University and Clinical Professor of Psychiatry at New York University, is immediate past chairman of the Ethics Committee of the IAGP and is on the Board of Governors, Nova University, Fort Lauderdale, Florida.)

In 1919 Woodrow Wilson went to Paris to work on forming the League of Nations. In his idealism, he began to explore certain political avenues and did not study the history of the problem he was working with. George Kennan, the political historian, now points out that this resulted in a total disaster.

It seems to me that we, as professionals, are responsible to look at the history of our profession, the people we have overlooked and what we could learn about these people today and how it could help us. Trigant Burrow was probably fifty to a hundred years ahead of his time. I'm going to tell you a little about him; I've written about him at greater length. My interest in Burrow was stimulated by one of my great professors, Gardner Murphy, the personality theorist; my all too brief studies with Martin Buber, the philosopher; my belief in the importance of the dialogue; and a phrase used by the columnist, Walter Lippman, that hung in my office for many years. This phrase said, "We define first and then see--rather than see first and then define." In our field, we mental health professionals have a tendency to define very quickly in the same way that we have a tendency to be expert about living but not expert in living. Understanding the work of Trigant Burrow can help in both these cases.

Burrow was born in the United States--in Virginia--in 1875. He was raised in a Roman Catholic environment--his mother being a very strong-willed and devout Roman Catholic. He attended Fordham University in New York City, then medical school at the University of Virginia. In Europe, at the Allgemeine Krankenhaus, his studies with Wagner von Jauregg and Krafft-Ebing aroused his interest in psychiatry. When he returned to the United States, he continued his medical studies at Johns Hopkins Medical School. There he met a student nurse, Emily Bryan, who became a very important influence; she became his wife.

In 1909 Burrow obtained a Ph.D. in experimental psychology in the field of attention. He met Freud and Jung who were passing through New York on the way to Clark University where Freud delivered his epic lectures. A.A. Brill had met them--Freud and Jung and Ferenczi--at the boat, and they were attending a theater performance in New York City. Burrow was introduced to Freud and with the encouragement of Adolf Meyer, the great pioneer in American psychiatry with whom Burrow was working, decided to enter analysis. But Freud did not see Burrow as an analysand, and I think this is where the tragedy began. I don't know whether Freud was overwhelmed with his own schedule, or if he just wanted to send some patients to Jung, but Burrow ended up in analysis with Jung. He went back to the United States and planned to return to Europe and begin analysis with Freud, but this was at the beginning of World War I.

Burrow was one of the founders of the American Psychoanalytic Association. Clarence Oberndorf, the historian of psychoanalysis, describes him as one of the foremost creative people in the history of psychoanalysis before and up to about the middle of the nineteen-twenties. Burrow continued his contact with Freud. He was one of the first presidents of the American Psychoanalytic Association. He became interested in the authoritarian relationship and the egalitarian relationship. After World War I, Burrow took into treatment a young outdoorsman named Clarence Shields. Shields, a rather quiet, retiring individual, challenged Burrow about the kind of relationship that existed between doctor and patient. Essentially, he asked, why should he, Shields, be on the couch, and Burrow sitting up--what did this say about the relationship? While Burrow was astute enough to recognize the resistance mechanisms, he was challenged enough to explore what would happen if you reversed this kind of position, this kind of relationship. And from this first exploration he embarked wholeheartedly upon his study of relationships

that exist among people as well as the group phenomena. He then wrote a classic paper on "The Group Method of Analysis."

Freud was very negative to Burrow's studies and interest. It is kind of sad to read their correspondence and follow their contact--Burrow kept trying to get Freud's approval, and Freud was consistently negative. I have speculations about this and have talked about this through the years: Freud's behavior may have been related to the disastrous outcome of his relationship with Jung; hostility from this may have spilled over onto Burrow because of Burrow's analysis with Jung. Also, Freud was very nonpolitical. If you recall, he was threatened by people like Alfred Adler, whom he saw as a socialist. Certainly he was upset by Sandor Ferenczi. He was upset by anything which he felt would dilute the intensity of what he was trying to accomplish. Also, many people in our field seem to forget the fact that in his basic paper on group psychology and the analysis of the ego, Freud was concentrating on the individual and his thinking; in terms of the group, he related to Le Bon and the concept of the mob. (Malcolm Pines has pointed this out in his writings. S.H. Foulkes picked up this point rather early in his professional career.) Burrow began seeing patients and colleagues over the summers at a place in northern New York State on the tip of the Adirondacks at Chateaugay Lake. Later he began meeting with them and exploring the relationships that existed among them. Alfreda Galt, a young older person, was one of the people in the group there during the nineteen-twenties and later on she is going to tell us about that experience.

At one point Burrow worked at the Henry Phipps Clinic in Baltimore and met Harry Stack Sullivan. Later in his life, Burrow claimed that Sullivan had not given him credit in regard to Sullivan's development of the interpersonal theory of psychiatry.

In 1921, a Swiss psychiatrist, Hans Syz, came to the United States and began to work at the Henry Phipps Clinic of John Hopkins Hospital. He met Burrow, became very intrigued with his work and was deeply involved with it for the rest of his life. In 1927, D.H. Lawrence wrote a review of Burrow's first book. Lawrence felt Burrow was on the cutting edge, with a dramatic breakthrough in the whole field of psychodynamic treatment. But Freud continued to be hostile.

Burrow became more and more interested in getting into the physiological aspects of group relatedness. When the American Psychoanalytic Association was reorganized in the mid nineteen-thirties, he was expelled from membership because he was seen as too biological. He was in good company at that point, because Paul Schilder was thrown out of the Association, too. I think the charge was that Schilder lacked training. But consider the kind of training many of these people had--two months of analysis with Freud in Vienna, or something similar! I always remember Albert Einstein's line that "great ideas have always met violent opposition from mediocre minds." Of course Schilder was also a genius and one of the great pioneers in our field. Burrow became increasingly isolated; he was seen as being outside of the psychodynamic framework and he withdrew.

Years later in my own professional development, I rediscovered him and brought him to the attention of many people in the analytic, psychodynamic, psychotherapy community--because I felt that what he had

to tell us was very important. Historically, you will hear much about Burrow's moving into the physiological aspect. I think he was clearly on the cutting edge. Today in our studies of neuropsychology, we see what Burrow was talking about. One of my good friends and a valued colleague, Phil Holtzman, is doing work on eye movement in schizophrenia at Harvard and at McLean Hospital. He has been looking for genetic markers in schizophrenia. Burrow has very specific things to say about the physiology of attention and Lloyd Gildea will talk about this from his background and his research experience and his rich training with Donald Hebb, the psychologist who had much to tell us here in Canada at McGill University.

Lloyd, I hope you will elaborate upon this: there is a coalescence that is sadly lacking in our field. Earlier today I listened to Malcolm Pines in his strong effort to push toward what I believe is the importance of developing a theory in the field of group therapy. We are still a field fumbling, looking for theories. Malcolm Pines mentioned Vigotsky. I found that very interesting and I want to interpolate this: I am a fellow of the New York Academy of Sciences and they asked me to prepare a program. I prepared one about the work of Vigotsky who, I think, has much to tell us in the field of group behavior. But they turned down the program because they felt it was too way out and most people wouldn't understand it! I thought it was our job as mental health professionals, as scientists, to reach out to those things, kind of over the horizon--as we keep reaching out to try to understand what's going on.

I hope you people will read Burrow and think about what he has to say. His letters are beautiful--the letters that Alfreda Galt played such a large part in editing for the book, *A Search for Man's Sanity*.<sup>1</sup> This book presents an excellent introduction to much of his thinking. However, when Burrow wrote his professional books and papers, his language became convoluted--he needed a good editor. But don't be put off by that. I think Burrow's great contribution is that he urges us to think of the larger meaning of the group. Whether we agree with his belief in biological relatedness or what he later called "phyloanalysis," I think is irrelevant. The important thing is that we should come together in the search for a theoretical structure for the field of group therapy and I think Burrow has a lot to tell us to help us in that search.

Alfreda Galt will tell us something about the history of her contact with Burrow and the development of The Lifwynn Foundation, which is still in Westport, Connecticut. Burrow's papers, by the way, and his correspondence are at the Yale University Library. Some of his books are in the bookstore here and will be available. Juan Campos took on the momentous and horrendous job of translating one of Burrow's works into Spanish and it is at the bookstore. What we're trying to do is bring Burrow more and more to the attention of the professional community. We're not doing it in any religious or cultist sense--we're doing it in the sense of, "What can we learn from this man to help us in the development of the theoretical structure for our work in group therapy?"

Before we met today, I kept thinking about Burrow. I was kind of immersing myself in his thoughts. Then I had the experience of meeting two psychiatrists from what is now Croatia. Dr. Klain and Dr. Morra discussed the horrible things that are happening in that part of the world. I was intrigued by the fact that the president of the Serbian nation is a psychiatrist, and I was intrigued by Dr. Klain and Dr.

Morra describing him as "such a thoroughly nice guy." I said, "then how do you explain this 'ethnic cleansing'? I mean, what does this tell us about our profession?" And all Ed Klain could say was, "I don't know." Well, I think this is Burrow's search--to find out how does this happen, how does this exist? I felt Malcolm Pines was trying to confront this at the Plenary Session. He asked us to think about it--why we haven't been called upon to play a part in the effort to understand these terrible tragedies that are going on in the world today. It's kind of ironic that we have a conference here on *Love and Hate* and yet we're not called upon to investigate why a nation fragments, why people are killing one another and why children are dying in Somalia. Somewhere we're failing, and I think this is what Burrow was asking us to explore.

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<sup>1</sup> William Galt et al, Eds. *A Search for Man's Sanity: The Selected Letters of Trigant Burrow*. New York: Oxford University Press, 1958.

## Experiencing the First Group Analysis

*by Alfreda S. Galt*

(Alfreda Galt, a student of Burrow in the early days of group analysis, is president of The Lifwynn Foundation and chairman of its Editorial Committee. She has written extensively on Burrow and has edited some of his writings.)

### I

Trigant Burrow's work has to do with the contrast between, on the one hand, the busy, gyrating, verbal intellectual side of our lives with its countless dichotomies--love/hate, good/bad, etc.; and, on the other hand, the processes of the organism as a whole. These latter processes are self-balancing rather than dichotomous, homeostatic rather than dualistic. Inhalation follows exhalation in a rhythmic cycle. The heart beats, rests, then beats and rests again. These homeostatic processes preceded language by many eons and are of course functioning within us as we sit here--it is not so much that each of us has an organism as that each of us is an organism.

The total organism is so important in Burrow's formulations that I want to borrow an experiment briefly from Charlotte Selver and the practitioners of Sensory Awareness to remind ourselves of this homeostatic whole. It will also give us a break from so much sitting and listening. [Pause] I'm going to invite you to stand and close your eyes, so for a moment we are not bombarded with the visual world. [Pause] We can ask ourselves how does it feel? Is it quiet behind the eyes? [Pause] Is there any sense of the support of

the floor? [Pause] Can you sense your own weight as it makes contact with the support under your feet? [Pause] Perhaps there is a sense of muscular tension in one place or another, some holding. [Pause] Perhaps in the shoulders. [Pause] When we open our eyes again, how does the world come in? [Pause] When we sit down, do we now feel the support of the chair under us and at our backs?

Again, I should make it clear that this exercise was not a practice of Burrow's--I have introduced it only to remind ourselves of the functioning totality that we are. This whole is of crucial importance in Burrow's orientation because of his position that basically this same homeostatic balance extends throughout our societal life as well. In 1927 he defined the group not as a collection of individuals but an assemblage of elements bound together by organic inherency--like a colony of ants, say, a herd of deer, a tribe of primitive people. This kind of organic bond, in his observation, unites the members of the human species.

It is interesting to compare Burrow's definition with Kurt Lewin's who defined a group in terms of its "interdependence" <sup>1</sup> and Foulkes' and Anthony's comment that "the group is a more fundamental unit than the individual." <sup>2</sup> Burrow himself did not dichotomize between society and individual. He felt that each of us embodies both the basic integration of our species and the secondary disorder that is so pervasive throughout the community-at-large. This disorder expresses itself in what Burrow called a "monstrous social mood, an unrecognized mood of systematized prejudice and absolutism." <sup>3</sup> This mood is related to the sense of self as a separate entity.

In 1928 he wrote:

Group or social analysis is the analysis of the immediate group in the immediate moment. A social group or community consists of persons each of whom is represented under the symbol he [or she] calls 'I' or 'I, myself.' This proprietary symbol is socially accepted by the individuals of the group. It is the basis of their intercommunication. But the 'I' that is socially accepted is socially elusive of analysis. The group composed of individual 'I's' is equally elusive of social analysis. The sum of impressions symbolized as 'I' would center attention everywhere else than upon the sum of impressions thus symbolized. Group analysis is the objective analysis of this subjective symbol as represented in the immediate group in the immediate moment. <sup>4</sup>

It seems clear that this "sum of impressions symbolized as 'I'" is not the equivalent of the ego. Rather it is the accumulation of inferences about ourselves and our fellows that we receive over time through the medium of mood, words and symbols. It is itself a symbol of the organismic self, an insubstantial image. Burrow called it the "I"-persona. This pseudo-identity came to light in the rigorous daily probing to which the original investigators in group analysis subjected themselves. Burrow and Shields questioned not only so-called negative emotions, but also the things they liked in themselves--love of children, art, scholarly discourse, the kindly gesture. And they found that even these expressions contained a marked element of self-consciousness, of possessiveness, of self-admiration (meaning literally "at the mirror").



This same inversion was conspicuous also in the community-at-large with its inequalities and exploitation; its elations and depressions; its deadly wars and restless periods of peace. They called this condition the "social neurosis," and group analysis was undertaken to investigate it.

As already indicated, this was not an analysis of the group by a healthy psychopathologist but analysis of the psychopathologist and others by what Burrow called "...the united societal consciousness composing our group...." <sup>5</sup> Thus, Shields, Burrow and the rest of the group were on an equal footing--both the investigators and the material to be investigated.

## II

I took part in 1926 in a study session at Lifwynn Camp, the Burrows' summer place in the Adirondacks. It was the fourth and largest study session there. Seeing all the activity in this splendid Congress, it is difficult to realize how much has developed in the lifetime of one person. So I want to share some of my experiences of this pioneer group analysis. There were twenty-four campers, aged fifteen to fifty-five, and we spent two months in an isolated and rather primitive setting. My mother, a teacher, had become interested in group analysis through a friend and had met several times with Burrow and others to discuss her problems as a widow with teen-aged daughters. She was invited to come to camp that summer and bring my older sister and me. Several other young people were to be there, too, including a student of Burrow's, William Galt, who later became a trusted co-worker of his and whom I had the good fortune to marry many years later.

The first surprise my family encountered on arrival at camp was to find that we were not going to occupy the same cabin--we were separated and assigned other roommates. The break-up of family units was purposeful as a means of interrupting familial dependencies and exploitations. Recently I asked my sister what she recalled from that summer and she said, "The feeling that we were doing something absolutely new." For me the outstanding feature was the sense of inclusiveness. There was a feeling that all of us, young and old, professional and lay, were equally subject to a fixation on the "I" and that it was socially engendered. This being in the same boat, the emphasis on feelings in the immediate moment, created a culture of openness which I believe was unprecedented at the time.

What we were asked to do was to look at our own reactions and those of others as they occurred, to monitor our feelings and share them with the group. We were to try to get beneath the surface of normal social interchange and expose to public view our covert motivations and assumptions. And this was much harder for us novices to do in 1926 than people find it now, precisely because of its novelty. Decades later when I took part in T-groups and encounter groups, I was impressed by how easily people articulated their feelings--it was almost as though a new generation had learned from our struggles at Lifwynn Camp. But of course, their established sense of self--their very experience of "I"--was not being assaulted as was ours in 1926.

It was a very busy summer. First, the campers did all the work and it was considerable. Then there were constant meetings: every meal was regarded as a group meeting at which one could bring up affective reactions; there would be reports of irritations and miscommunication, infatuations, efforts to please, etc. In addition each camper was assigned to a small group (five or so members) that met two or three times a week. They included a medical group comprised of Burrow, Shields, Hans Syz, other physicians and a psychologist. Then we all attended general meetings and seminars, each scheduled also several times weekly. And impromptu meetings were encouraged whenever the situation warranted it--for example, when my mother returned from a shopping trip having forgotten to buy coffee, the housekeeping committee put everything aside to consider with her the factors that contributed to the oversight.

Obviously this was not a therapy group in the sense in which that term is ordinarily used. But I think all of us felt that the honesty and forthrightness we were attempting was somehow healthier than the "normal" repressive tendencies of the day. As an example, I want to read you a brief excerpt of a general meeting held July 30, 1926. My mother is speaking:

I want to speak of a matter that has been on my mind for a great many years and which I had no thought of speaking of when I first came up here, but I feel on several occasions to have kept it a secret has kept me from complete participation in the group life and I'd like to talk about it and there's another reason that will come up later.

Then she went on to describe her marriage and the fact that my father suffered from depressions and melancholia. After about five years of her marriage she realized that they were recurrent and came periodically. She said he "suffered a great deal for many years before we were married; a very dependent relationship existed between us. He depended on me and looked to me for a great many things. I was very pleased to respond to that but occasionally got very tired of it and wanted to have some petting for myself, so that I used to have breakdowns of health and finally I achieved a particularly successful one and had to be sent to Saranac [for treatment of TB]. This was at the beginning of one of his periods of depression. He was in New York teaching at Columbia University" and she goes on to speak of how he suffered from insomnia and even hallucinations and sought the advice of a famous psychoanalyst who told him that indeed he was quite ill and might have to go to a sanitarium for treatment and then sent him home and told him to come back on Monday. Well, that weekend he took his own life.

My mother goes on to say, "I have never told my children; I have lied to them consistently about this; I never meant to tell them. But in these weeks here I feel that I was defrauding them of something--their opportunity to face life as it is." And she adds, "I want to say that this isn't our tragedy...there isn't a person in this room whose life has not been touched by the same kind of tragedy, probably no one in the world."

The meeting went on to talk of the sense of failure that is always associated with such an event, and Mr. Shields spoke of the secrets in families and the pain associated with them. I think it is possible that my sister and I might never have been told of the nature of our father's death if we had not been at the camp;

and the simplicity and clarity of the telling, the placing it in a wider context were significant in softening the effect the news had on us.

### III

But actually the investigation itself was about to move to a more concrete dimension. For while I and other neophytes were learning to identify and share our private feelings, the experienced investigators were extending this process, probing even more deeply into the impulses that motivated their daily lives. And at a time when the exacting experiment had reduced Burrow to "a state approaching interrelational nihilism," there suddenly occurred "a most unexpected phenomenon"—a reaction which he could only describe as a sensation of pressure or tension in the head.

The perception remained only long enough to be noted. But with the continuing routine of affect-negation, it made itself felt again in the forepart of the head in the region within and back of the eyes. And what was of special significance was the absence of affect images and self-oriented bias when the sensation was in awareness. Here was something tangible at last, something palpable.<sup>6</sup>

Gradually Burrow and his co-workers learned to bring the tensions in the oculo-facial area into awareness, or perhaps one should say, to permit the tensions to come into awareness. From then on the induction of this sensation became their only recourse for eliminating self-oriented bias.

Burrow went on to formulate his observations in terms of a conflict between two contrasting patterns of attention, using the word attention broadly to signify the ecological process that unites organism and environment. He called the normal attentive mode *ditention* because it is divided, that is, it involves a deviation from direct participation with the environment: part of our attention reverts back to the self-image ("How am I doing? Am I getting what I deserve?" etc.) By contrast *cotion* (co: together) is the basic attentive mode of humankind. It is the mature development of the sense of identification experienced by each of us in the earliest moments of sentient awareness—a development now stunted by ditention and the "I"-persona.<sup>7</sup> Cotion is the tensional integration of the species in its balanced interrelational function, and the important words here are tensional and function. Cotion and ditention have to do with organismic process, with sociophysiological function, and they involve neuromuscular components. Why should they be in conflict? Fundamentally they aren't and really cannot be; cotion is primary and encompassing; ditention secondary and partitive. But Burrow suggested that over the generations as words and symbols became more and more potent in negotiating with the environment and with our fellows, the linguistic, symbolic function began to encroach on the basic empathic resources of the organism, to attempt a kind of false supremacy. Burrow considered that the slight tension which he had experienced in the oculo-facial area with the frustration of affect-projection might signal this encroachment. He introduced the term neurodynamic in relation to his thesis that our world-wide interrelational disorders are due to disturbance in the organism's internal tensional patterns.

(Incidentally, with Burrow, experience always came first and theory followed after--first there was the event and later the explanation.)

As this work advanced, the atmosphere at Lifwynn Camp and at the laboratory established in New York City, became more mellow. There was not the constant activity and frequent meetings, and the group was now only twelve to fourteen people, the nucleus which had continued to live and work together as an experimental community and which indeed stayed together for more than thirty years. But group analysis always continued and mealtimes still were laboratory sessions for the observation of mood and behavior--in fact, they still are at the Foundation. In addition, writing went on constantly--papers and books and Burrow's prodigious correspondence; and in the forties, the instrumental work which showed clear differences in respiration, eye-movements and EEG when subjects shifted from dition to cotention.

#### IV

We may ask ourselves how relevant is Burrow's work to global problems of today, or even to those that we encounter more intimately in family or office. The changes since his death in 1950 have been enormous. To mention a few: the phenomenal extension of group work, as shown by this Congress, the whole development of pharmacology in relation to what is still called "mental" illness; the widespread development of meditative practices; the whole field of somatics and nonverbal work such as sensory awareness, Eugene Gendlin's "focusing" techniques, etc.; the growing emphasis in phenomenology on the intersubjective and intercorporeal; and the world-wide ecological movement demonstrating the global interdependence of our species.

Then there is the remarkable work of Patrick de Maré with reference to median groups and working through hatred to impersonal fellowship or *koiononia*; also, the theories of David Bohm which define the social neurosis and its relation to thought; the growth of dialogue groups which owes so much to both of these men; the human relations conferences based on Bion's work; Malcolm Pines' emphasis on sociality. And undoubtedly there are other movements in your own field which I should mention also but of which I am not informed. If all these trends follow a natural line of development, won't we find our way as a people to a position somewhat similar to Burrow's? Won't the social neurosis and our individual involvement in it become clear?

Perhaps. Burrow himself felt that it would not be necessary for other groups to go through the kind of picking and probing that involved him and his associates in the early days. Yet, in spite of progress, secrecy is still the norm throughout the world; we are still deeply divided and the needs of the group or community are still being short-circuited by the "I"-persona. So why can't we go directly to cotention?--why can't we just be cotentive? Well, are we, as elements in an unbalanced society, in a position to know what cotention is as yet? Or is it necessary to gradually eliminate extraneous impediments--neurodynamic, affective, attitudinal--to communication and societal well being? Is cotention what remains when all these interferences have been removed?

Burrow points out that cotention "originated as a group process and is, therefore, essentially a social as well as an individual phenomenon."<sup>1</sup> Thus, the interferences to this mode of interrelational functioning come to attention more readily in a hospitable group setting; that is, a group which recognizes itself as typical of its kind and which has no difficulty in acknowledging its own dishonesties, self-preoccupation, sentimentalities, etc. But this is research; it means examining within our own processes the tensional components that limit our outlooks as they limit also the outlooks of, say, a Saddam Hussein or the Serbian leader; and that surround us and them in our social settings. These tensions are common and continuous--we see their effects out there, we experience them here. Cotention, or balanced interrelational function, is the background from which we observe these deviant and extraneous tensions or ditention. And this does not have to be a painful process; it can be freeing and joyful as some of our group hang-ups about, for instance, authority, love and hate, etc., melt away.

I believe we still need groups working along those lines--we need many of them. Juan Campos' Grup d'Anàlisi in Barcelona, Spain is dedicated to "the study of the sociological, psychological and biological factors interfering with the human capacity for cooperative action as an individual and as a species within its ecosystem."<sup>9</sup> This is a significant commitment. And there will be others. To quote Burrow again, "...there is required throughout the world the establishment of groups of behavior students bent upon a strictly physiological technique of behavior-adjustment. Out of the work of such groups will grow a science, [of]...human behavior. Like the cancer experts, such groups will be interested in healing disorders throughout the organism of [humankind]. They will not be interested in modifying the behavior of a particular person, clique, political party or country. They will know that the healing of the [whole] will alone bring healing to the [part]."<sup>10</sup>

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- <sup>1</sup> Kurt Lewin. *Field Theory in Social Science*. New York: Harper & Brothers Publishers, 1951, P. 147.
  - <sup>2</sup> S.H. Foulkes and E.J. Anthony. *Group Psychotherapy*. London: Penguin Books, 1957, P. 216.
  - <sup>3</sup> Trigant Burrow. "The Social Neurosis." *Philosophy of Science*, 1949, Vol. 16, P. 29.
  - <sup>4</sup> \_\_\_\_\_ "The Basis of Group Analysis." *The British Journal of Medical Psychology*, 1928, Vol. 8, P. 198.
  - <sup>5</sup> William Galt et al, Eds. *A Search for Man's Sanity: The Selected Letters of Trigant Burrow*. New York: Oxford University Press, 1958, P. 126.
  - <sup>6</sup> Trigant Burrow. *Science and Man's Behavior*, William E. Galt, Ed. New York: The Philosophical Library, 1953, pp. 245-46.
  - <sup>7</sup> \_\_\_\_\_ *Preconscious Foundations of Human Experience*. William E. Galt, Ed. New York: Basic Books, Inc., 1964, pp. 164.
  - <sup>8</sup> \_\_\_\_\_ "Kymograph Records of Neuromuscular (Respiratory) Patterns in Relation to Behavior Disorders." *Psychosomatic Medicine*, 1941, Vol. III, No. 2, P. 183.
  - <sup>9</sup> *Grup d'Anàlisi Barcelona*, 1989.
  - <sup>10</sup> Trigant Burrow. *The Neurosis of Man*. London: Routledge & Kegan Paul, 1950, P. 344.

## A Developmental Perspective on Burrow's Theory of Attention *by Lloyd Gilden*

(Lloyd Gilden has been in touch with The Lifwynn Foundation since 1950. An Associate Professor at Queens College, New York, he directed a program of instrumental research for the Foundation in the nineteen sixties. He now serves as Research Associate and a member of the Foundation's Board of Directors.)

I will focus my remarks in this talk on the subject of attention, which, I believe, was the seminal concept used by Burrow to organize his study of human social adaptation and behavior. I hope to show, consistent with Burrow's thesis and writings, that the characteristic mode of attention of the human species reflects a certain level of cognitive development. But, that level of development may be transcended. In other words, a higher level of development is possible, if a specific procedure discovered by Burrow is practiced.

To fully grasp the depth and breadth of Burrow's insights regarding human attention I have found it useful to adopt a particular developmental perspective. A basic axiom of developmental psychology set forth by Heinz Werner <sup>1</sup> is that development proceeds from a relatively global state lacking differentiation to a state of increasing differentiation. Extending this idea Ken Wilbur suggests that "psychological development proceeds...level by level, stage by stage, with each successive level superimposed upon its predecessor in such a way that it includes but [also] transcends it." <sup>2</sup>

For example, when children enter puberty, hormonal changes, secondary sexual characteristics such as the development of breasts in girls and facial hair in boys, and sexual behavior are differentiated. Adolescence is a higher stage of development in which the newly differentiated features of the body are superimposed upon the prepubescent characteristics. As sexuality becomes integrated into the young person's behavior, that behavior becomes more complex. Furthermore, the previous behavior potentialities of children are included, but also transcended in the higher stage of adolescence.

I wish now to employ this developmental model to analyze one of Burrow's most important discoveries, namely a higher stage of human development, which he referred to as "cotention."

### Attention and Preconscious Experience

In a broad sense Burrow's work focused on the phenomenon of attention, which can be defined in general terms as the process by which humans experience interest in or are aware of their physical organisms and

their environment--in other words, "...the apprehension of the objects and conditions that surround" them and also the sensations that occur within them. <sup>3</sup>

Human postnatal development begins with a mode of consciousness which Burrow characterized as "preconscious," although this differed from Freud's use of the term. In Burrow's preconscious stage the mode of attention may be referred to as "co-tention," implying that the individual is experiencing his relationship with, or, as a part of, the environment. In Burrow's words, "For the infant, there is at first no interval, no distance which must be bridged in order to reach the object opposite it, outside itself. As yet there exists no object, no opposite, no other self...[in this] basic identity or consistency...the organism's mental and physical spheres of behavior" are united. <sup>4</sup>

Thus, our original mode of awareness or attention, our original mode of relationship to the external and internal world rests upon an organismic foundation. External and internal stimuli are experienced as sensations occurring within the organism as a whole. Furthermore, the organismic experience is one of integration and coordination. The whole or total organism interacts WITH objects and events. Observer and observed are united. Organism and environment fit together as a unitary system.

### Ditention

During the course of human evolution a major development took place, which eventually led to a radical shift in man's pattern of attention or mode of relating to his environment. I quote Burrow again: "Through the modification of a segment of the forebrain, man was enabled to produce...symbols or signs in substitution for actual objects or situations. In other words, there developed the faculty of language..." <sup>5</sup> In terms of Werner's developmental model, we can say that the symbol forming function became differentiated as an autonomous system within the broader framework of the organism as a whole.

The advent of human symbolic capacity and language has proved to be a double-edged sword. On the one hand, the symbol provides us with the power to encode experience, and thereby greatly facilitates interaction with the environment and communication among those employing language. It also permits us to accumulate information, upon which rests the development of culture--mores, science, and art. Without language, it is unlikely that humans living today would behave differently than our Neanderthal ancestors did one hundred thousand years ago.

The other edge of the sword, however, has all but severed our connection to both our physical and social environments, and to the deepest aspects of ourselves. For language, by its very nature, creates a "split in the universe between the knower and the known, the thinker and thought, the subject and the object..." <sup>6</sup> Furthermore, due to the ability of language to facilitate interaction with our environment, we have become preoccupied with its use--not unlike the way their newly differentiated sexuality captures the attention of adolescents.

In sum, humans have all but stepped out of a mode of consciousness or attention--namely, cotention--that knitted us inextricably to our external and internal environments, and we have entered a mode of consciousness or attention that divides us from our environments and ourselves. Burrow named this mode of attention "ditention" to emphasize the division or split that has developed with the advent of language. Where there had been a confluence between organism and environment, coordination and unity, there is now, with ditention, separation of observer and observed, accompanied by dissociation, judgment, and the potential for conflict.

### Cotention

With differentiation of the symbolic faculty and the acquisition of language, the experience of the organism as a whole is subordinated to the influence of our symbol system, our thoughts and images. The tail now wags the dog, so to speak.

I will now endeavor to describe the remedy Burrow proposed to restore organismic integrity and outline his argument that there is another form of attention, another mode of relating to the environment, that leads to improved individual and social adaptation.

During the course of his group research Burrow discovered internally perceptible, physiological signs of the imbalance between the function of the symbol system and the broader system of the organism as a whole. The discovery occurred as he and his associates, working in a group setting, engaged in persistent challenging of the habitual, everyday mode of communication, characterized by affective, self-righteous, biased behavior.

The discovery consisted of the emergence into awareness of tensions in the region of the eyes and the brow that are correlated with the symbolic (ditentive) pattern of attention. When this happened, Burrow developed the ability to internally discriminate a discrete pattern of neuromuscular activity within the framework of sensations of the organism as a whole. I suggest that the tensions Burrow discriminated were the organismic correlates of symbolic processes.

Paradoxically, just as a fish is probably unaware of the water in which it exists, until it finds itself out of water, we humans seem to be oblivious to the presence of the symbolic activity that leads to our persistent, chronic use of language throughout our waking hours. We talk, we think incessantly. But, rather than being aware of the pervasive intrusion of thoughts and images into the experience of the organism as a whole, we proceed as if language and thought are the sole proprietors of our experience.

Why was subjective discrimination of the symbolic pattern so important? It was important because Burrow overcame preoccupation with thoughts and images. He learned to voluntarily shift his attention away from symbolic processes to the broader organismic system. With persistent effort to focus upon the internal sensations around the eyes, an awareness of an "entire brain pattern" <sup>7</sup> associated with ditention became perceptible.



To illustrate the process of discrimination, let's recall the principle of figure-ground shifts demonstrated by gestalt psychology.

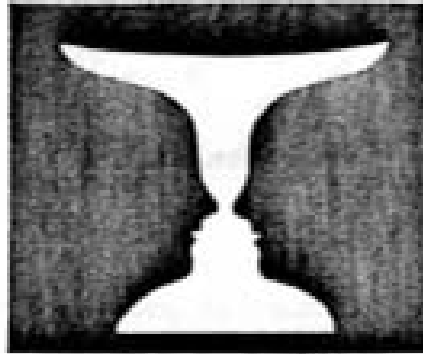


Figure 1

You are probably familiar with the famous illustration of the goblet/profiles reversible figure. Note that you can perceive either the white portion as a goblet or the black portion as two profiles, but only one at a time. We can appreciate here the ability of our attention to shift from one set of features to another set. With each shift of attention a different perception occurs. The particular set of features being attended to at any moment constitutes the figure and the other features recede into the ground.

It may also be the case that aspects of a stimulus field are not initially perceptible as a meaningful figure. For example, there are many instances in which discrimination of some aspects of the visual field as a figure fails to take place, and recognition of certain elements or a pattern does not occur. Can you discriminate a figure in Figure 2? Study it for a while, if you cannot see it right away.



Figure 2



Figure 3

Perhaps, the experience of seeing the hidden face will give you some insight into the process of discrimination that transformed Burrow's mode of attention. After extensive effort to challenge so-called normal social behavior in his group research, previously unnoticed sensations in and around the eyes were discriminated as manifestations of the symbolic faculty. When this occurred, the symbolic system became internally perceptible, just as the face in Figure 2 becomes perceptible as a discrete, unified figure or gestalt. The next set of figures will summarize what I have been saying.



Figure 4

Figure 4 depicts the preconscious mode of attention and represents the assumption that the reactions to external and internal stimuli are experienced as a set of organismic sensations and thus constitute the figure in awareness.

Figure 5 depicts dition. It assumes that with acquisition of language, symbolic thought and imagery comes to dominate attention. Now thoughts and images constitute the figure and organismic sensations recede out of attention into the ground.

After persistently challenging the habitual mode of symbolic attention—ditation—it became possible for Burrow to bring into awareness the tensions in the region of the eyes associated with the symboling function.

These tensions were signs of the differentiated symbol system existing as a component of the organism as a whole. They served as a guide, which aided Burrow in shifting his attention from thoughts and images to organismic sensations. Now organismic sensations became the figure, while thoughts and images receded into the ground. Symbols no longer dominated attention. Instead, thought was transcended. It was no longer the primary interpreter of experience. This is a fundamental aspect of cotention. See Figure 6.

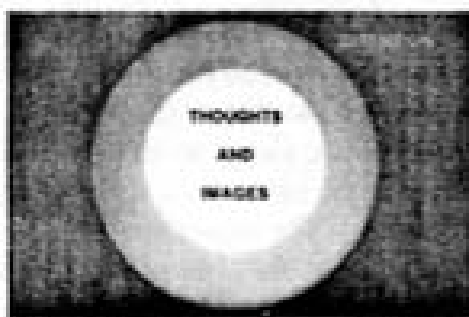


Figure 5

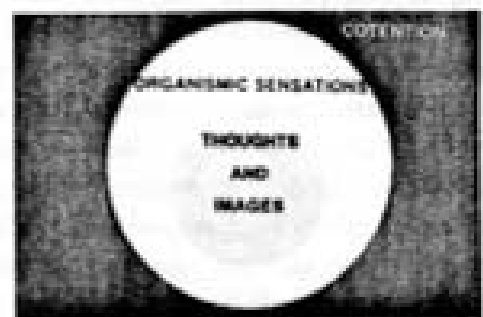


Figure 6

It is important to remember, however, that thinking is not eliminated in cotention. Attention to organismic sensations can occur concomitantly with symboling. When this happens, according to the criteria of psychological development I referred to in the introduction, a higher stage of development results, involving integration of the symbol system (with its thoughts and images) and organismic sensations. This is depicted in Figure 7 by representing both sets of processes as the figure.



Figure 7

In cotention a more complete form of apprehending experience occurs. A higher stage of awareness becomes possible when thoughts and images are integrated with the sensory capacity of the whole

organism. This integration creates a body-mind unity, which greatly expands our ability to experience our individual organism, our social relationships, and our connection to the external world in general. We as observers are now more connected with what we observe. The split produced by dominance of the symbolic faculty disappears.

In certain respects, when cotention can be achieved—a process that is achievable only with extended practice—something of the preverbal, preconscious state is regained. Once again, the whole organism encounters the environment. But now, cotention involves the adult mind, and the experience is enriched by adult knowledge; it constitutes a different mode of consciousness.

Thus, Burrow's research culminated in the discovery that ditention, our species' maladaptive mode of relating to our physical and social environments, can be replaced by cotention. When this occurs, habitual reaction patterns are eliminated. For when the organism as a whole becomes the mediator of responses to environmental events, and the symbolic faculty is subsumed or relegated to a subordinate role within that system, symbolically conditioned affective reactions are replaced by primary, organismic responses. Concurrently, the basis for moral considerations is transformed, moves to a higher level of development, whereby experience of the organism's relative position in the universe is appreciated, and an inclusive, rather than oppositional, relationship with the world occurs. The experience of separation of the observer and the observed diminishes. In adult cotention, as in the cotention of infancy, tendencies toward self-biased, acquisitive impulses wane, as do the tendencies toward domination and aggression.

In conclusion, Burrow not only made a major contribution to our understanding of the human disposition toward conflict, but also discovered a way out of our dilemma. He found a means of achieving a higher stage of human development by shifting from the divisive, oppositional tendencies associated with ditention, to an inclusive, coordinated mode of relating to the physical and social world—to cotention.

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<sup>1</sup> H. Werner. *Comparative Psychology of Mental Development*. New York: International Universities Press, 1957.

<sup>2</sup> Ken Wilbur. *The Atman Project*. Wheaton: Quest, 1980, P. 2.

<sup>3</sup> *Trigant Burrow: Toward Social Sanity and Human Survival*. Alfreda S. Galt, Ed. New York: Horizon Press, 1984.

<sup>4</sup> Trigant Burrow. *Preconscious Foundations of Human Experience*. William E. Galt, Ed. New York: Basic Books, 1964, P. 10.

<sup>5</sup> *Ibid.*, P. 107.

<sup>6</sup> Ken Wilbur. "Two Modes of Knowing," in *Beyond Ego*. R. Walsh and F. Vaughan, Eds. Los Angeles: J.P. Tarcher, 1980, P. 234.

<sup>7</sup> *Trigant Burrow: Toward Social Sanity and Human Survival*. Alfreda S. Galt, Ed. New York: Horizon Press, 1984, P. 105.

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**Dreams, Dialogue and Bi-polarity--  
Our Three-Year Experiment at The Lifwynn Foundation**  
*by John Wikse*

(John Wikse, who serves on the faculty of Shimer College, Waukegan, Illinois, was the Foundation's Research Director from 1989-92.)

I began to study Burrow's work from my background as a political theorist interested in problems of community development. Burrow conceived of neurosis as a community-wide phenomenon. Among contemporary psycho-therapeutic approaches, I think that community psychiatry, therapeutic community and milieu therapy are closest to his orientation. I was also interested in studying the socio-political meaning of my dreams--which was enough to end my career as a political scientist, for which I have a certain feeling of affinity with Burrow regarding his career as a psychotherapist. For quite some time I have as well been interested in Hatha Yoga and in the development of somatic psychology. Burrow's work bridges community and somatic psychology. He was a very integrative thinker and experimenter. So to comprehend his research method--which for the last three years at the Lifwynn Foundation, we've been trying to revivify--you need to think of something like crossing political science with Hatha Yoga.

What we've tried to do was to reconstruct the research conditions that Burrow understood to be a laboratory method. We organized a group that was not oriented towards therapy, but was interested in studying itself, a group that was motivated to enter into a "peer inquiry." Burrow coined the term "social self-inquiry," meaning neither a self inquiry in the individualistic sense, nor a social inquiry as if society's "out there," but social as something that we live and experience.

Let me share with you, first of all, a few Burrovian dreams. You all know the idea that people in Freudian therapy have Freudian dreams and in Jungian therapy, Jungian dreams. I don't know that anybody has ever talked about Burrovian dreams, but as I was starting to work at Lifwynn I had a series of dreams that seem to me to have to do with trying to understand and communicate to others what Burrow called an "altered perspective" on the very meaning of research. So let me share a few of these dreams.

The first was at a Lifwynn conference in 1976 at the camp in the Adirondacks where I first met Monte Ullman, who has been working with us over the last years as an advisor. It was in that setting that he and Alfreda Galt organized a ten-day study on dreams as social information--dreams as a research tool in social life. At the camp I dreamt that I was moving in the bloodstream of a societal organism, and as I moved through it, I was walking along the paths that connect the cabins at Lifwynn Camp. They are all separated, there is a public community area, a library, and the individual cabins that Alfreda mentions.

As I was going through this bloodstream in the dream, I was going person-to-person along the camp trails to meet the people who were there to do this research. I think meeting another person as part of a common organism is basic to Burrow's perspective. I think the dream represented that metaphor to me.

The second dream was at the time that I accepted the position of Research Director at Lifwynn. I dreamt that I was leading a group--I'm an academic usually, and I lead small group dialogues--and in the dream I was doing what I would normally do in such a group, trying to put my attention on the content of the conversation. At one point in the dream I intervened in the group process to say that people were not listening to one another. This is a common observation that I might make. Then somebody said to me in the dream, "well that's a sociological perspective on continuity, you're not very perceptive psychologically, you're not following the free association--you're looking too much at the content, not feeling the psychological connection." And that befuddled me and then I suggested we take a break. During the break we're milling around--I know I am very confused; I'm trying to think about what's happening--and behind me I hear a voice which said: "genuine continuity is neither sociological nor psychological: objectivity is biological." When I woke up I thought: "This is the voice of Burrow, I better write that down."

One last dream from July of 1989 when we were beginning to work on this new stage of social self-inquiry. In this dream I'm speaking with a group of students who are in a circle. I notice that one of them is dejected. I say to her that she's raising the question of the relationship of the individual to the group and she's experiencing the negative side of groupness--the tendency to identify the person, to inhibit, to stifle and restrain the individual and she acknowledges that that's what she's feeling. I step into the center of the circle and as I speak, I rotate, I start moving around in the middle of the circle [demonstrates] and I face everybody in turn. It's as if I'm trying to demonstrate this feeling of circularity by my movement. I feel as if I'm in the nucleus of a cell. And I say that groupness is our second body, part of our "thrownness"--this term from Heidegger--into the world and that we are part of overlapping circles, but that it is difficult for us to face this whole organism of which we are a part (just as I can only face certain individuals as I rotate and must necessarily turn my back to others), that we are encircled beings, that this is our biosocial reality, our second body which is for us second nature, that is, culture.

Now let me turn to some of the issues we've faced in our three-year experiment. Our project has been very complicated, very intense. It's had to do with issues of authority and decision-making, with trying to understand what we've come to call the "cotentive practice" which is basic to social self-inquiry, but may still be somewhat vague to people, although Lloyd Gilden has talked about cotention and dition and illustrated these concepts. I think of what Burrow engaged in as a meditative practice. I would say he was a self-taught meditator. What Burrow discovered was that by closing his eyes and attempting to feel the kinesthetic relaxation of the eyes against the field of darkness, the sensation that Lloyd described and that Alfreda referred to would come into awareness.

One aspect of our work over the last three years has been to locate the cotentive practice among the "inner felt disciplines" of contemporary life and study: in relationship to Eugene Gendlin's focusing technique, to Zen Meditation, to Sensory Awareness--and to what is called Sambhavi Mudra in Yoga

psychology. I would say Burrow developed the cotentive practice out of utter frustration. He confronted "interrelational nihilism." This happened because he attempted to give up control of the group process. There are complicated questions about whether he did this successfully or not--but this is what I think he tried to do. So that the work we did recently at Lifwynn to develop a new stage of social self-inquiry was based really upon our thinking that leaderless, agendaless dialogue is the context in which one can question what Burrow called the authority function in groups--that is, our assumptions about the very nature of authority.

That also meant that Alfreda and I have worked at many different levels with regard to the authority assumptions in our own relationship. For a year and a half, my family--my wife and our--at that time two-year-old daughter--lived in residence with Alfreda in the Foundation Center. As we shared a kitchen, my wife and daughter were sort of in the experiment, but not of it. They were often the source of issues that came up around the hearth, and around the integration of work and everyday life. At the same time, there was the attempt to carry out what I took to be my central job at Lifwynn, namely, to recognize the Burrow in other disciplines and practices, to try to bring Burrow's work into a contemporary dialogue. Alfreda had of course been doing this for quite some time--reaching out to people like David Bohm who we thought shared important assumptions and perspectives with Burrow in his concept of dialogue. Bohm developed this concept in relation to Patrick de Maré's work in the median group. We felt a strong affinity with the orientation of Pat de Maré.

I would like to come to closure rather quickly and try to have some dialogue if people have questions or observations. But first, let me speak briefly about the process of organizing a new social self-inquiry. The first thing we did was to host a four-day residential conference on the topic of addiction as a sociocultural phenomenon. We brought twenty people together in order to have the median group situation. We planned four days of leaderless, agendaless dialogue. We set up an elaborate preconference correspondence and people exchanged papers. It was quite an interdisciplinary group, some working in the fields of addiction, some interested in Burrow's work. This conference provided a setting in which we could talk about the meaning of social self-inquiry with more than just a few people who had been rather intimate with Burrow's work.

Of course (as those of you experienced in group practice would imagine) we immediately bi-polarized around those who were interested in social self-inquiry and those who were interested in addiction--and also around the question of whether or not by staying in our heads, we were ourselves manifesting our basic addiction to thought. Well, there were a variety of different dimensions to the bi-polarity. In relation to this, there emerged a powerful awareness of what I would call "the lack of public safety." We had organized this conference as a public health dialogue and it seemed to me that what we were confronting was our inadequacy as a public, our inability to face one another as peers in a public setting without the background of hierarchy and leadership that is our normal custom. Despite the fact that we had very carefully selected a group of quite remarkable, mature, capable, cooperative, loving, brilliant people, the normal patterns of the social neurosis were deeply manifest.

In January, 1991, during the process of trying to evaluate the conference, we brought together a group of about twelve of the participants from the East Coast that became the core of a social self-inquiry group that still continues to meet once a month. Our meetings came to focus on the bi-polarity experienced at the conference and by Alfredo and myself as we worked together. Our patterns of dichotomy seemed to reflect the situation that Alfredo was facing the past of Burrow, feeling the need of integrity and a following through with the original group analysis which Burrow developed; while I was feeling the need to face the future and open a dialogue with the esoteric aspects of Burrow's language--and to translate his language into other disciplines in order to facilitate an interdisciplinary dialogue.

I consider working with bi-polarity to have been basic to Burrow's research method. Practicing cotention in our group work is a way of getting "behind" dichotomy, experiencing it as projection. I think the second of my dreams above was pointing to the possibility of placing attention on the biological continuity which can be experienced somatically "behind" the apparent bi-polarities and confusions of psychological and sociological conflict. Our tendency to separate self from other is the fundamental dichotomy which Burrow brought into and examined in the laboratory. This dilemma was at the root of his discovery that group analysis was necessary to understand the social neurosis.

One of Burrow's original collaborators, Dr. Hans Syz, who worked with us until his death last year at ninety-six years of age, expressed the problem very clearly in an early paper. Let me close with a quote from that paper:

The circumstance in which only two people confront one another, the observer and the observed, physician and patient, does not offer a favorable opportunity for a clear view of the interindividual involvement and for the scrutiny of unrecognized (unconscious) social implications and suggestions. The contact is too acute. It necessarily embodies too much a repetition of collusions and emotionally toned interactions that are constantly enacted by each of the participants in everyday contacts and not submitted to observation. In such a bipolar situation it is not possible for the observer to acquire the needed perspective, correct the deviation in judgment of one's habitually biased reason, to question one's own basis. In fact, any argument or discussion, such as easily arises in an interview between two persons, tends to corroborate the uncritical stand of each.<sup>1</sup>

In working through our tendency to uncritical self-corroboration we confront a central obstacle to meaningful collaborative relationships. This is, I think, the aim of Burrow's altered research perspective.

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<sup>1</sup> Hans Syz. "On a Social Approach to Neurotic Conditions." *The Journal of Nervous and Mental Disease*, 1927, Vol. 66, P. 613.



## THE STUDY GROUP FOR GROUP ANALYSIS

On August 26, the day following the Foundation's symposium at the Congress of the International Association of Group Psychotherapy, a meeting was held of the Association's Study Group for Group Analysis.

This is a committee established in 1988 through the efforts of Juan Campos who serves as chairperson. Its purpose is to build bridges across language barriers and orientations between group analysts at the international level.

There were twenty persons present representing study groups in eight countries: Israel, Italy, Mexico, Portugal, Spain, Sweden, United Kingdom and the USA. Juan spoke of the interest in the Foundation's presentation the day before and his hope that the Study Group would take on the task of learning about Burrow's research and reporting back to the next meeting of the IAGP on its relevance to group analysis today. Five members--from England, Italy, Mexico and Sweden--volunteered to follow up on this suggestion.

In addition, there was discussion of a project undertaken by the Laboratorio di Gruppo Analisi di Palermo on the "identity of the group analyst"--a detailed questionnaire prepared by them was circulated and considered at the meeting.

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## DIALOGUE ON SOCIAL SELF-INQUIRY

Excerpts from a meeting at Queens College, New York, September 27, 1992

Present: Lloyd Gilden (LG), Steve Rosen (SR), Mary Alice Roche (MAR), Maureen Cotter (MC), Alfreda S. Galt (ASG), Margaret Rockoff (MR)

LG: I would like to work on how we actually engage in cotention, and what we experience when we do. I'm convinced that it is something that can at least be made specific enough for people to consider--whatever they do with the result of their considerations. We should have something that we can share and discuss with other people.

SR: Here we have this semantic problem: what do we mean by cotention? By implication, how does it relate to other practices?

LG: How do you feel about trying to engage in the practice, and talk about what we're doing as we do it--to, as I said, share it with other people? If each one of us could, would say something about whatever he or she does, and what the effect is--that would be a way for us to begin to have some consensus--

among ourselves as to what constitutes cotention. I don't know whether we have an answer that comes from inside, or that is subjectively clear to us.

SR: I often do something I think is related to cotending. It's a redirection of attention.

LG: Can we try something now, each of us, however we go about it, and then share whatever it is we might be experiencing? I hope we will be able to get off the abstract, theoretical level and be more phenomenological, actually trying to experience what happens when we engage in what we think of, each of us, as the cotentive practice. There's probably a certain amount of embarrassment that takes place when we close our eyes, or whatever we do, which may be a reason we avoid engaging in the practice.

MAR: I have no embarrassment about engaging in the practice or closing my eyes. Right now I'm not very comfortable in my sitting, and I'm stiff in my neck. And I can either go on and tell you what that is a manifestation of, or I can stop talking, and simply experience the stiffness and the uncomfortable sitting, and let the discomfort tell me how it wants to change, so that I can be more open to hear you, to sense you--inside me....And this is happening even while I'm talking. I find that I can attend both to what is happening organismically, and to what I am saying--which kind of surprises me.

LG: Right now you're experiencing that?

MAR: Yes. My sitting becomes more balanced, and my head becomes easier, even though I'm telling you what's happening, and, in some way, recognizing that people are hearing me--to whatever degree you are.

LG: I'm feeling the tension in the brow, which is a sign for me that I am beginning to cotend. I've been practicing trying to do it with my eyes open. I still find it useful to limit my visual field and kind of focus on some point in front of me as I engage in this, rather than look around. Although, as I become more skilled in the practice, I think I can do that, too.

MAR: I found that I was looking down when I was speaking, though it was not intentional. I was not looking at any one particular person.

SR: Earlier I was asked about what I experience. The words are difficult. For me it is a matter of becoming aware, directing my attention to the process of my own projection of feeling. Preparing myself to answer accompanied a certain fluttering in my abdomen, a certain feeling of wanting to respond, a certain feeling of being bound by the rules--to give a coherent response, to show that I'm really attuned to myself. By directing my attention in the act of preparing to respond, I could notice all of this coming out of me--and how it now feels as I'm speaking. I notice my own reactions to people, and I suspect that these are "I"-persona type reactions. In the transactions I carry on with the group my body seems to be mobilized to define myself, to affirm my position in the group. Is this clear? Is what I'm saying about how my attention becomes redirected coming across in a phenomenologically concrete way? It's a process of intercepting my own projections, as I'm trying to gain the group's attention. I

turn my own attention around on itself, and this allows me to catch myself in the attempt to put myself in a favorable position in the group.

**MAR:** I understand what you're saying, but I also want to know what it feels like in your body. You were talking about the fluttering in your stomach when you were getting ready to speak; that was very concrete. But when you talk about directing your attention from this to this....

**SR:** Directing my attention "backward," retracting it into itself in such a way that an awareness comes of my own concrete process of attending. So, in attending to another person, for example, I would not only be aware of her as the object of my attention, but also, "moving backward," become consciously aware of the act by which I project this "object." It's a question of attention not being limited to the objects it sets before itself--whether another person or even the fluttering in my stomach--but including its own objectifying process.

**LG:** What I have been doing since this summer is focusing primarily on my bodily sensations, particularly in the head area, and the more that I pay attention to the sensations in my head, the easier it is for me to become aware of them, and the more prominent they are in my awareness. It does take effort. I usually do what you described before Alfreda, namely, forget all about it, and just go about interacting with the physical and social world unaware of organismic sensations. But when I remind myself, I can shift my attention to the sensations, as I'm doing now. Among those sensations, which I find very useful, is the ringing in the ears. That's always there. And it's a good landmark, so to speak. It's something that I know emanates within, and by putting my attention on the ringing in the ears, I can bring myself, or my attention, into my head, and from there attend to the sensations in the head and around the eyes. (I'm not quite sure to what extent they're muscular sensations in the scalp and the brow, I'm sure there's some of that; I can actually relax and contract some of those muscles in my scalp and brow. But I do have a sense that there are deeper sensations that may, in fact, arise in the brain itself. That's something that needs to be somehow determined.) The effect of my putting my attention on the head region, is to give me a different sense of spatial relationship--that is my relationship to the world outside my body, including people. I have a sense that the emptiness of space disappears. There's a filling up of space--almost like I'm swimming in water, but there's no water. I'm in a filled space and everyone and everything in it is part of this universe I'm experiencing; there's a closer connection with it. I have a palpable sense of my head engaged in--or immersed in--the world....

**MC:** I have heard the discussions about what is cotention and what isn't cotention; and I think in some ways I allow that to block me. When I've had experience with it that seemed to take me into a centered way, I wasn't really setting up any expectations....But since we've been working more with cotention at Lifwynn meetings, what has been helpful when discussions got heated is either to close my eyes or not focus eye contact with somebody else, keeping limited focus. Then there is certainly a difference in the way I can hear, and a difference in my level of excitement when I think the affect is creeping in....I'd like to understand more about what cotention is to everybody, at the same time, I'd like to lose my own feeling of expectation of what it should be. I want to know what it should be, but I don't want to be locked into, "Oh, I'm not doing it!"

**MAR:** My understanding is that cotention is a type of attention. Is that right?

**LG:** Yes. That's my understanding of the definition: a form of attention--in which awareness of the organism as a whole is prominent. The distinction as I understand it between cotention and ditention is that in ditention the awareness of the organism as a whole is in some way absent--that is, is not clearly in awareness--whereas, in cotention one is aware of the sensations of the organism as a whole, as the organism encounters the world.

**SR:** There is a question about separating cotention from ditention. I wonder, if we intend to be able to get a sense of the organism as a whole, whether attention must be directed more fully on the ditentive process--that the ditentive process must become more fully aware of itself--and in that way achieve a sense of the organism as a whole.

**LG:** That's what I used to think. I did that for a long time. Not that that's necessarily wrong, you understand. I've just found it very fruitful--instead of focusing on the affect and the manifestations of the "I"-persona and the ditentive process--to shift the attention to the organismic sensations in the head, and elsewhere, too. And--I'm reporting what I experience--the effect of shifting attention that way seems to be a lessening of the tendency to have affective reactions and personalistic responses. Not zero by any means! But as my sense of my being as a whole develops, the other seems to recede, somewhat in proportion....

**ASG:** At our meeting last June, after I let the tensions around the eyes come into awareness, I had the feeling, "I am inhibiting this meeting and interfering with its progress." But there was something about that broadened beam of attention so that it became very clear to me it was the group, including me, that was inhibiting itself. And it became clear that the question of the disposal of the trust fund was something that was an affect-impediment for me; I had given attention to it, but I hadn't said to the group, "You don't know it, but this is a secret financial concern I have." After I said those things the meeting went smoothly. Lloyd, you spoke of the fact that you realized that my saying, "I don't control this meeting," made it possible for you to get a sense of how much you had been feeling that I was in authority.

**LG:** Or my projecting authority on you.

**ASG:** Projecting authority onto me, yes. Throughout the last couple of weeks, when we were getting ready for this meeting, I spent a great deal of time making up statements that I was going to present here; and it really was a noticeable distraction. It all seems to me to be a performance. The audience that I am making these speeches to is the imaginary audience of the people sitting here now. But though it feels as though that's the audience, it isn't really--it's only self-image being bounced off my projected image of Maureen, Lloyd and so on. It's always the self. But every now and then I am able to bring that projection right back, somewhat as Steve describes, so that the experience is behind the eyelids, no

longer "out there." Then there aren't any mirrors out there to bounce my image off of, it's just the sense of the curtain before my eyes and a general loosening up.

MAR: What is out there?

ASG: Well, the world is out there. The world is there to be seen and to be interacted with.

SR: This seems to speak to Lloyd's experience, a fullness of space. When Alfreda says she is withdrawing her projections back into a position behind her eyelids in recognition that she had been in the process of projecting these images--well, these images are projected into empty space. We might say that the anxieties of the "I"-persona generally arise in relation to the images we project into empty space. Could the retraction of those projections eventually lead to Lloyd's experience of a fullness of space?

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## DAVID J. BOHM 1918-1992

We have been greatly saddened by the death of David Bohm in London on October 29th. David was a member of the Foundation's Advisory Board and took part in our five-day Conference on Addiction in October, 1990, when we grew accustomed to the gentleness and clarity of his interventions. Likewise, his several contributions to *Lifwynn Correspondence* enhanced its significance. He was Professor Emeritus of Theoretical Physics at Birkbeck College in London and the author of *Wholeness and the Implicate Order*, *Unfolding Meaning* and other seminal works. David Bohm was a man of deep vision and a clear-sighted realization of the precarious position of humankind as we approach the twenty-first century. We extend heartfelt sympathy to Saral Bohm, David's wife of thirty-six years, whom we also came to regard with appreciation and affection as a participant in our 1990 conference.

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## JOINT CONFERENCE WITH THE INSTITUTE OF GENERAL SEMANTICS

"Making Connections Between General Semantics and Phylobiology" is the title of a one-day conference scheduled for February 6, 1993, in New York. The topic, to be considered from the joint perspectives of the two organizations, is "the relation between our assumptions and how we act." Half of the participants will be conversant with General Semantics and half with phylobiological studies and social self-inquiry. Writings on the two approaches will be exchanged ahead of time and the conference will include an emphasis on sensory awareness.

Plans for the conference have been developed by a small committee consisting of Charlotte Read and Susan Kodish of the Institute of General Semantics; and Alfreda Galt, Lloyd Gilden, Mary Alice Roche and Jack Wikse of The Lifwynn Foundation.

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## TRIGANT BURROW IN TRANSLATION

Arrangements are being completed for the publication in Rome of an Italian translation of *Trigant Burrow: Toward Social Sanity and Human Survival; Selections from His Writings* edited by Alfreda S. Galt. The publisher will be Edizioni Universitarie Romane and it is expected that forewords will be contributed by Dr. Leonardo Ancona, Director, Department of Psychiatry, Università Cattolica, Rome and Dr. Juan Campos, Chairperson of Grup d'Anàlisi, Barcelona, Spain.

Dr. Campos has also prepared a Spanish/English version of *The Structure of Insanity*, a 1932 treatise in which Burrow first presented his findings about a conflict in contrasting modes of attention. A German/English version is also being prepared.

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## "CELUI QUI A OSÉ"

Claude Pigott's *Introduction à la psychanalyse groupale*, published in Paris, in 1990, contains a chapter on Trigant Burrow entitled "One Who Dared." A translation of its conclusion is as follows:

"Whatever turn [Burrow's] research took, it is nonetheless true that what he wrote during the first fifteen years of his practice is quite interesting and unique from the perspective of extending psychoanalysis to groups....Burrow did not wish to do individual psychoanalysis in groups and he is the first who tried to develop the notion of a group unconscious in the individual in order to utilize it in his practice; his concept of 'primary identification' adds a theoretical coherence to his thinking. The part which refers to individual and collective training is quite specific and their inner dynamics have given rise to numerous developments. He instituted the principle of systematic group sessions within a well-defined framework (his laboratory method) centered on 'the here and now,' all the while knowing that the latter held the entire individual and collective past. Because of that we consider Trigant Burrow to be one of the few authors to have tried to conceptualize a true group psychoanalysis....One must learn to dare...."

•••••

## The Lifwynn Foundation

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Dear Doctor:

Juan Campos has asked me to send you the enclosed copy of our bulletin, *Lifwynn Correspondence*. It contains the papers given at the symposium he chaired in Montreal entitled "Beyond Dichotomy: The Orientation of Trifant Burrow." They describe Burrow's original group analysis and its historical connections and present his longterm observations regarding individual and social disorder. Both Juan and Max Rosenbaum in their papers recommend serious consideration of Burrow's innovative theories by group analysts and group therapists generally. Burrow's studies were conducted at this Foundation, now an organizational member of IAGP.

Also enclosed is a copy of Juan's report of the meeting of the Study Group for Group Analysis held in Montreal the day after the symposium. As you will note, there was interest expressed in this committee in carrying forward a study of Burrow's contributions and their relevance to group analysis today. If you would care to take part in this study, please let us know and we will be glad to provide reading lists, etc.

We would also welcome your comments and reactions to the material in *Lifwynn Correspondence*—it would be a pleasure to hear from you.

Sincerely,

Alfreda S. Galt  
President

### The Study Group for Group Analysis Meeting at the Montreal Convention Center on August 26, 1992.

There were twenty persons present, representing study groups of eight countries: from Israel Tamara Stenberg and Rachel Chazan; from Italy Leonardo Ancona, Giusy Cuomo, Vincent Morroge, Anna Maria Traveno, Girolamo Lo Verso and Francesco Di Maria; from Mexico Lilian Klein; from Portugal Antonio Guilherme Ferreira; from Spain Juan Campos and Hanne Campos; from Sweden Siv Andersson and Else Bengtsson; from UK Malcolm Pines, Earl Hopper and Piers Lyndon; and from USA Alfreda Galt, Max Rosenbaum and Jack Wikse. The meeting had been convened by letter on June 6, 1992 and the two main issues to be discussed at Montreal were the ones decided upon during the Board of Directors Meeting of IAGP in Lisbon in 1991 by the members of The Study Group for Group Analysis Committee there present -Dr. Campos, Dr. Ferreira and Dr. Di Maria-, namely a research proposal on the 'identity of the groupanalyst' and to formulate the 'aims and means' for the Study Groups in Group Analysis. A copy of the report submitted to the Board of Directors in Lisbon was made available to people convened to and the attendants of the meeting at Montreal. Juan spoke of the interest of Burrow's work during the Symposium 'Beyond Dichotomy ...' the day before and his hope that the Study Group will take on the task of

learning about Burrow's studies and reporting back on its relevance to group analysis today at the next meeting of the IAGP in Buenos Aires in 1995.

Then, the theme of the 'identity of the group analyst' was approached by the presentation of the questionnaire that the group of colleagues from the Laboratorio di Gruppo Analisi di Palermo, Girolamo Lo Verso and Francesco Di Maria, have prepared in this regard. They asked the members of the meeting to complete it and to make comments. The information asked for concerned: 1) personal and professional data, 2) theoretical references and 3) information on concrete work with groups. A discussion on the practical and financial difficulties of carrying out this research followed. From this emerged a more general discussion about the possibilities of the type of investigation proposed by the Study Group Committee. Max Rosenbaum spoke of the enormous problem of organizing this type of data and Hanne Campos mentioned -as an example of organization of such data the two-year project presented by a Spanish group of group workers on the 'Actual Significance of the Group' presented at the Amsterdam Congress. There, from the theme analysis of thirteen papers submitted by twenty-three colleagues of the Spanish Society for the Development of Group, Psychoanalysis and Psychotherapy, emerged a group conception that clustered into these three main topics: group work as it relates to psycho-analysis, group work in institutions, and the professional identity of the group worker.

Malcolm Pines suggested that the meeting was dealing with three different themes: 1) research collaboration on group analysis, 2) a study of the work of Trigant Burrow and 3) organization of data. He also spoke of the importance of a group-analytic approach. Max Rosenbaum mentioned the desirability of liaison with the Society for Psychotherapeutic Research. Jack Wikse, former research director of The Lifwynn Foundation, spoke of the need to define what is research. He mentioned the generational incoherence encountered in their 'social self inquiry' on this question.

Prompted by a very concrete question by Siv Andersson, 'What is the difference between attention and observation?', Juan talked about the interest in following up on the study of Burrow's investigations -an five people from England, Italy, Mexico and Sweden expressed their desire to do so.

The general objectives of the meeting seem to have been met. The President of the IAGP, Alberto Serrano, has been informed of the result -and in turn asked Juan Campos to continue as Chairman and to suggest a Co-Chairman for the Committee.

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**For the time being, please remember:**

**Questionnaires and comments preferably should be addressed to Girolamo Lo Verso, Largo Primavera 9, 90143 Palermo, Italy.**

**Reading lists on Trigant Burrow can be obtained from Alfreda Galt, The Lifwynn Foundation, Box 314, 1771 Post Road, Westport, Connecticut 06880, USA.**

**Groups or people interested in joining the network or obtaining further information, please contact Juan Campos at Paseo San Gervasio 30, 08022 Barcelona, Spain FAX 343 418 0331**

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**We need your subscription to assure the continuation of *Lifwynn Correspondence***

In view of the financial situation faced by the Foundation, as explained on the inside front cover of this issue, it is questionable whether we can continue publication without a greater number of subscriptions. If you feel there is value to this effort to facilitate communication about the social and somatic bases of worldwide dislocation, please send us your subscription of \$10.00 for two issues as soon as possible (an increase of \$2.50 to meet growing costs). Your encouragement will be greatly valued.

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**Please send me copies of:**

\_\_\_\_\_ The full transcript of the social self-inquiry meeting of September 27, 1992 excerpted in this issue.

\_\_\_\_\_ The chapter (in French) "One who Dared" from Claude Pigott's book, *Introduction à la psychanalyse groupale*.

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The following books are available from The Lifwynn Foundation at the price shown:

- Juan Campos' Spanish/English version of *The Structure of Insanity* (\$10.00)
- *Trigant Burrow: Toward Social Sanity and Human Survival*, edited by Alfredo S. Galt (\$20.00)
- *Burrow's Preconscious Foundations of Human Experience* (\$20.00)
- *Science and Man's Behavior* (\$25.00)
- *A Search for Man's Sanity: Selected Letters of Trigant Burrow* (\$25.00)

**STUDY GROUP OF GROUP ANALYSIS**  
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- Trigant Burrow: A Pioneer Revisited (1986) by Max Rosenbaum, Ph.D.
- The Phenomenology of "Normality" in the Context of Trigant Burrow's Group Analysis (1991) by Alford S. Galt

\* Available also in German

\*\* Available also in French