to the *iq* gene. This interpretation is supported by mating experiments which show that *iq* shuts off β galactosidase synthesis about seven times more effectively than does wild type.

Having obtained *iq* mutants, Müller-Hill, Crapo and Gilbert incorporated the *iq* gene into an inducible derivative of prophage ϕ80-λ. This prophage lysogenizes cells and can be induced but it is lysis defective. Thus by inducing the prophage the number of *iq* gene copies per cell can be increased several hundred-fold, and the net effect is a strain of *E. coli* which makes 0·5 per cent of its protein as *lac* repressor.

Riggs and his collaborators (*J. Mol. Biol.*, **34**, 361 and 365; 1968) report modifications of Gilbert and Müller-Hill's original procedure for isolating *lac* repressor. They have added two additional column chromatography steps at the end of the procedure which they claim increases the purity of the repressor. They confirm with these repressor preparations that the 7*S* repressor (molecular weight 150,000) is a polymeric molecule (their best estimate of the subunit molecular weight is 40,000 to 50,000) and that the repressor binds to the operator region of *lac* operon DNA.

More Tinkering with the Ribosome

from our Molecular Biology Correspondent

NOMURA and his collaborators have now extended their earlier observations on the partial removal of proteins from both 50*S* and 30*S* particles and on the effects of depriving the ribosomes of selected groups of these proteins. It will be recalled that by centrifugation in a caesium chloride density gradient, the 30*S* and 50*S* ribosomes of *E. coli* become transformed, through loss of protein, into 23*S* and 40*S* particles which are functionally inactive. Moreover, as also previously reported, the removable proteins from each subunit may be fractionated into two groups, one acidic and the other basic, only one of which is indispensable for restoration of activity.

Traub and Nomura (*J. Mol. Biol.*, **34**, 575; 1968) now describe these experiments in greater detail. When the acidic proteins of the 50*S* particle are not replaced there is no poly-U dependent phenylalanine incorporation, whereas restoration of the acidic group alone leads to partial restoration of activity. For the small subunit of the ribosome, however, it is the basic proteins which are essential both for phenylalanine incorporation and binding of phenylalanyl-*t*RNA. The proteins from the 30*S* and 50*S* subunits are not interchangeable. This work thus demonstrates that particular proteins have specific functions, though whether these consist of direct participation in the steps of protein synthesis or in defining the conformation of the particle is not yet obvious.

In a second paper (Traub, Söll and Nomura, *ibid.*, 595), further aspects of the activity are considered. Thus, the 23*S* particles with the basic protein fraction added back are also partially active in polypeptide synthesis directed by a natural viral messenger, as well as in binding formylmethionyl-*t*RNA in the presence of the initiation codon. More particularly, Traub *et al.* have investigated the nature of the stimulating effect of the other (acidic) group of proteins: it turns out that the total number of messenger-directed *t*RNA binding sites in a solution of the particles is increased by addition of this fraction. Because it may be supposed that no new types of binding sites are created, it follows that in the absence of the acidic proteins, a proportion of the ribosomes are in an inactive conformation. The authors infer the existence of several conformational states of the ribosome, but the simplest explanation is that the particles are in equilibrium between the active (native) state and one which is inactive (and probably partially disorganized), so that the acidic proteins serve to stabilize the native form.

Another interesting observation is that the fidelity of translation of the code is unaffected in the less active particles. It seems to have been generally accepted that the several chemical agents which cause massive misreading, with incorporation of the "wrong" amino-acids, function by virtue of some insalubrious effect on the structure of the ribosome, though nothing has ever been directly observed. This effect, if it exists, must evidently be highly localized for it is not reproduced in the much more altered particles deprived of a substantial proportion of their proteins.

A third paper (Nomura and Traub, *ibid.*, 609) contains an important stoichiometric study which shows that, by the addition of labelled proteins to unlabelled core particles, the same amount of protein is specifically taken up as was initially lost to produce the particles. Furthermore, if excess protein is added beyond the equivalence concentration, the activity of the ribosomes goes down, not up. This is a consequence of non-specific adsorption of the proteins, and is reversed by washing with 1 M ammonium chloride. This suggests that the recombination is quite specific, with each protein returning to its appointed place. The reassembly of the 50*S* particles is found to be instantaneous, as judged by the appearance of activity, but that of the 30*S* particles occurs at a measurable rate (about 1 per minute), apparently with the formation of an insoluble aggregated intermediate.

Two papers have simultaneously appeared on the role of protein thiol groups in ribosome function. McAllister and Schweet (*ibid.*, 519) and Furano (*Biochim. Biophys. Acta*, **161**, 255; 1968), using *N*-ethylmaleimide and oxidized mercaptoethanol, respectively, as thiol reagents, find that the capacity for messenger-directed binding of a specific transfer RNA is destroyed. The binding of poly-U is unimpaired. The ribosomes are in no detectable way disorganized, and do not dissociate (in contrast to earlier observations on the action of *p*-chloromercuribenzoate). One suggestion is that some form of steric obstruction of the *t*RNA site is involved, though a direct implication of the critical thiol group(s) in the binding site is also possible.

Primary and Tertiary Structures. With regard to the report by Smith *et al.*, which I discussed three weeks ago, on the seemingly astonishing differences in sequence between the subtilisins from two strains of *B. subtilis*, Professor L. L. Campbell has written to draw attention to his work (*J. Bact.*, **94**, 1124, 1131; 1967) on the BPN′ strain, in which he concludes that this is not a *B. subtilis* at all but a strain of a species which he has christened *B. amyloliquefaciens*, and genetically unrelated therefore to *B. subtilis*. He suggests that the two species have been confused by microbiologists and biochemists for more than twenty years. Readers may now decide whether to remain in a state of astonishment, because these organisms are after all of the same genus, or to return to one of apathy because they are from different species. It is worth noting that the enzymes are functionally essentially the same, at least in terms of specificity.